[COMMITTEE PRINT]

SCIENCE POLICY STUDY
BACKGROUND REPORT NO. 7

EXPERTISE AND DEMOCRATIC DECISIONMAKING: A READER

REPORT

PREPARED BY THE

CONGRESSIONAL RESEARCH SERVICE
LIBRARY OF CONGRESS

TRANSMITTED TO THE

TASK FORCE ON SCIENCE POLICY

COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

NINETY-NINTH CONGRESS

SECOND SESSION

Serial EE

DECEMBER 1986

Printed for the use of the Committee on Science and Technology

U.S. GOVERNMENT PRINTING OFFICE
47-543 WASHINGTON : 1987

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, DC 20402

H702-4

COMMITTEE ON SCIENCE AND TECHNOLOGY

DON FUQUA, Florida, *Chairman*

ROBERT A. ROE, New Jersey
GEORGE E. BROWN, Jr., California
JAMES H. SCHEUER, New York
MARILYN LLOYD, Tennessee
TIMOTHY E. WIRTH, Colorado
DOUG WALGREN, Pennsylvania
DAN GLICKMAN, Kansas
ROBERT A. YOUNG, Missouri
HAROLD L. VOLKMER, Missouri
BILL NELSON, Florida
STAN LUNDINE, New York
RALPH M. HALL, Texas
DAVE McCURDY, Oklahoma
NORMAN Y. MINETA, California
BUDDY MacKAY, Florida**
TIM VALENTINE, North Carolina
HARRY M. REID, Nevada
ROBERT G. TORRICELLI, New Jersey
RICK BOUCHER, Virginia
TERRY BRUCE, Illinois
RICHARD H. STALLINGS, Idaho
BART GORDON, Tennessee
JAMES A. TRAFICANT, Jr., Ohio

MANUEL LUJAN, Jr., New Mexico*
ROBERT S. WALKER, Pennsylvania
F. JAMES SENSENBRENNER, Jr., Wisconsin
CLAUDINE SCHNEIDER, Rhode Island
SHERWOOD L. BOEHLERT, New York
TOM LEWIS, Florida
DON RITTER, Pennsylvania
SID W. MORRISON, Washington
RON PACKARD, California
JAN MEYERS, Kansas
ROBERT C. SMITH, New Hampshire
PAUL B. HENRY, Michigan
HARRIS W. FAWELL, Illinois
WILLIAM W. COBEY, Jr., North Carolina
JOE BARTON, Texas
D. FRENCH SLAUGHTER, Jr., Virginia
DAVID S. MONSON, Utah

Harold P. Hanson, *Executive Director*
Robert C. Ketcham, *General Counsel*
Regina A. Davis, *Chief Clerk*
R. Thomas Weimer, *Republican Staff Director*

SCIENCE POLICY TASK FORCE

DON FUQUA, Florida, *Chairman*

ROBERT A. ROE, New Jersey
GEORGE E. BROWN, Jr., California
TIMOTHY E. WIRTH, Colorado
DOUG WALGREN, Pennsylvania
HAROLD L. VOLKMER, Missouri
STAN LUNDINE, New York
NORMAN Y. MINETA, California
HARRY M. REID, Nevada
RICK BOUCHER, Virginia
RICHARD H. STALLINGS, Idaho

MANUEL LUJAN, Jr., New Mexico
ROBERT S. WALKER, Pennsylvania
F. JAMES SENSENBRENNER, Jr., Wisconsin
CLAUDINE SCHNEIDER, Rhode Island
SHERWOOD L. BOEHLERT, New York
TOM LEWIS, Florida
SID W. MORRISON, Washington
RON PACKARD, California

John D. Holmfeld, *Study Director*
Harlan L. Watson, *Republican Staff Member*

---

* Ranking Republican Member.

** Serving on Committee on the Budget for 99th Congress.

LETTER OF TRANSMITTAL

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
Washington, DC, May 2, 1985.

To the Members of the Task Force on Science Policy:

We submit herewith for your use a collection of readings entitled "Expertise and Democratic Decisionmaking". This reader has been compiled at the request of the Science Policy Task Force by the American National Government and Public Administration Division of the Congressional Research Service in the Library of Congress.

A highly important but at the same time difficult issue on the agenda of our Science Policy Study is entitled "Science in the Political Process." That subject was included because we in the Congress have over the years seen a number of issues arise in which the expert scientific judgments of the professional scientist, and the more general political judgment of the Members of Congress have had to be reconciled. In the last few years this kind of question has received intense discussion as the result of a small number of cases involving the authorization and funding of scientific research facilities, and the manner in which such facilities should be considered by the Congress and the scientific community.

The present reader seeks to place that debate in a broader context at two levels. First, the reader includes descriptions and analyses of other historical cases from the post-Bush Report (1945) period where similar issues were debated. Secondly, this reader seeks to place all of these issues into the broadest context of how experts can and should function in a democratic system of government such as ours which basically relies on the citizen-legislator to make decisons about the actions of the government.

We are indebted to Dr. Charles H. Levine and Mr. Peter M. Benda of the Congressional Research Service for carefully reviewing the extensive literature in this area and selecting many of the readings to be included in this volume. They also wrote the introductions to each chapter, and the committee staff has added a small number of additional materials. All the readings are directly related to the matter of legislative concern before our Task Force on Science Policy.

I am hopeful that our witnesses, as well as the members of the Task Force, of the Committee on Science and Technology, and of the House of Representatives will find this reader helpful in reviewing the issues of science and politics. We believe that the materials can be helpful to all of us in arriving at the right policies to guide the future interaction between our political system and the science community in this country.

Manuel Lujan,
Ranking Republican Member.

Don Fuqua,
Chairman.

LETTER OF SUBMITTAL

CONGRESSIONAL RESEARCH SERVICE,
THE LIBRARY OF CONGRESS,
Washington, DC, April 18, 1985.

Hon. DON FUQUA,
Chairman, Committee on Science and Technology, U.S. House of Representatives, Washington, DC.

DEAR. MR. CHAIRMAN: In response to your request of January 11, the Congressional Research Service has prepared the enclosed reader on "expertise in democratic decisionmaking". It was compiled and edited by CRS Senior Specialist, Charles H. Levine and Senior Research, Assistant, Peter M. Benda.

We hope that our reader will serve as a useful reference in your Committee's efforts to assess the current debate about the relative roles of members of the scientific community and Members of Congress in making decisions about the allocation of funds for scientific research and associated support facilities. As you requested, however, the selections are not limited to the field of science policy but include some theoretical discussions of the role of experts in democratic decisionmaking generally.

We are pleased that your Committee has solicited the assistance of the Congressional Research Service as it sets out to conduct a comprehensive study of U.S. science policy during the 99th Congress, and hope that this reader will make a valuable contribution to that effort.

Sincerely,

GILBERT GUDE,
Director.

CONTENTS

EXPERTISE AND DEMOCRATIC DECISIONMAKING: A READER

PART I: THE LEGACY OF THE WESTERN PHILOSOPHICAL TRADITION

Page

Background and Summary 3

A. "Should Experts Rule?"—Selections from Plato and Aristotle 6

  1. Selection from Plato's *Republic*, Bk. VI (¶487e–489c). In Edith Hamilton and Huntington Cairns, ed. *The Collected Dialogues of Plato* (Pantheon Books), pp. 724–5 7
  2. Selection from Aristotle's *Politics*, Bk. III, Ch. 9, ¶10. In Richard McKeon, ed. *The Basic Works of Aristotle* (New York: Random House), pp. 1189–90 10

B. "The New Science" and Its Implications 12

  1. Selections from Francis Bacon, *The New Organon*, Book I. In Fulton H. Anderson, ed. *Francis Bacon: The New Organon and Related Writings* (New York: Liberal Arts Press, 1960), pp. LXXXI (¶81), 118–9 (¶129), 90–1 (¶92), and 68 (¶70) 13
  2. Selection from Rene Descartes, *Discourse on Method*, Part VI. Translation by John Veitch (Chicago: Open Court, 1924), pp. 65–7 20
  3. Selection from Thomas Hobbes, *Leviathan*, Part I, Chapters 5 and 10. In Sir William Molesworth, ed. *The English Works of Thomas Hobbes*, Vol. III (London: John Bohn, 1839), pp. 35–7, 74–5 25

PART II. THE PLACE OF SCIENCE AND EXPERTISE IN A DEMOCRATIC POLICY—SELECTIONS FROM THE TRADITION OF ANGLO-AMERICAN POLITICAL THOUGHT

Background and Summary 33

A. The Early Republic 36

  1. Selected Statements from the Founding Fathers:
    a. Benjamin Franklin, "A Proposal for Promoting Useful Knowledge Among the British Plantations in America" (1743). In The *Writings of Benjamin Franklin*, Albert Henry Smyth, ed. (New York: Haskell House 1970), Vol. II, pp. 228–32 37
    b. George Washington, "First Annual Address to Congress" (January 8, 1790). In *The Writings of George Washington*, John C. Fitzpatrick, ed. (Washington: U.S. Government Printing Offc.), Vol. 30, pp. 491–4 42
    c. Thomas Jefferson, Letter to Eldrige Gerry (January 26, 1799). In *The Writings of Thomas Jefferson*, Paul Leicester Ford, ed. (New York: G.P. Putnam, 1896), Vol. 7, pp. 325-9. 46
  2. Secondary Materials:
    a. John C. Greene, "Science, Learning, and Utility: Patterns of Organization In the Early American Republic." In Alexandra Oleson and Sanborn C. Brown, eds., *The Pursuit of Knowledge in the Early American Republic* (Baltimore: Johns Hopkins University Press, 1976), pp. 1-19 52

B. Twentieth Century Perspectives 73

  1. Selection from John Dewey, *The Public and Its Problems* (Chicago: Gateway Books, 1946), pp. 123-7, 203-9 74
  2. Harold J. Laski, "The Limitations of the Expert." In *Harper's Magazine*, Vol. 162 (1930-1): 101-10 85

3. Dwight D. Eisenhower, "Farewell Radio and Television Address to the American People" (January 17, 1961). In *Public Papers of the Presidents of the United States: Dwight D. Eisenhower, 1960-61*, Vol. 421 (Washington, DC: U.S. Gov't Print. Off., 1961), pp. 1035-1040 95

PART III: CONTEMPORARY PERSPECTIVES ON THE RELATIONSHIP BETWEEN DEMOCRACY AND EXPERTISE

Background and Summary 105
1. Robert E. Lane, "The Decline of Politics and Ideology in a Knowledgeable Society." In *American Sociological Review*, Vol. 31, No. 5 (October 1966): 649-662 110
2. Dorothy Nelkin, "The Political Impact of Technical Expertise." In *Social Studies of Science*, Vol. 5, No. 1 (February 1975): 35-54 124
3. Harvey Brooks, "Expertise and Politics—Problems and Tensions." In *Proceedings of the American Philosophical Society*, Vol. 119, No. 4 (August 1975): 257-261 144
4. Yaron Ezrahi, "Utopian and Pragmatic Rationalism: The Political Context of Scientific Advice." In *Minerva*, Vol. 18 (Spring 1980): 111-131 149
5. Ben L. Martin, "Experts in Policy Processes: A Contemporary Perspective." In *Polity*, Vol. 6, No. 2 (Winter 1976): 149-173 170

PART IV: EXPERTISE AND DEMOCRACY IN AMERICAN SCIENCE AND SCIENCE POLICY

Background and Summary 197
1. Donald K. Price, "The Scientific Establishment." In *Proceedings of the American Philosophical Society*, Vol. 106, No. 3 (June 1962): 235-245 205
2. Robert C. Wood, "Scientists and Politicians: The Rise of an Apolitical Elite." In Robert Gilpin and Christopher Wright, eds., *Scientists and National Policy-Making* (New York: Columbia University Press, 1964), pp. 41-72 216
3. Harvey Brooks, "The Scientific Adviser." In Gilpin and Wright, *op cit.*, pp. 73-96 249
4. Selections from John D. Miller, *The American People and Science Policy* (Oxford: Pergamon Press, 1982). Chapters 4 ("Public Participation in Science") pp. 33-54, and 10 ("The Formulation of Science Policy in a Democratic Society"), pp. 125-133 273
5. Kenneth Prewitt, "Scientific Illiteracy and Democratic Theory." In *Daedalus*, Vol. 112, No. 2 (Spring 1983): 49-64 305

PART V. NATIONAL SCIENCE POLICY AND THE SCIENCE ADVISORY SYSTEM

Background and Summary 323
A. General Features and Organization of the Science Advisory System 347
    1. Selection from Anton G. Jachim, *Science Policy Making in the United States and the Batavia Accelerator* (Carbondale and Edwardsville: Southern Illinois University Press, 1975). Chapter 3 ("The Organization of Science and Government: Some Consequences"), pp. 28-63 348
    2. Donald K. Price, "Endless Frontier or Bureaucratic Morass?" In *Daedalus*, Vol. 107, No. 2 (Spring 1978): 75-92 387
    3. Selection from Deborah Shapley and Rustum Roy, *Lost at the Frontier: U.S. Science and Technology Policy Adrift* (Philadelphia: ISI Press, 1985). Chapter 2 ("The Institutional Factor"), pp. 37-64 405
B. Patterns of Evaluation and Decisional Criteria in American Science 434
    1. Peer Review:
        a. Selection from National Commission on Research, *Review Processes: Assessing the Quality of Research Proposals* (Washington: NCR, 1980) 434
        b. Stephen Cole, Leonard Rubin, and Jonathan R. Cole, "Peer Review and the Support of Science." In *Scientific American*, Vol. 237, No. 4 (October 1977): 34-41 450
        c. Thane Gustafson, "The Controversy over Peer Review." In *Science*, Vol. 190 (December 1975): 1060-66 472

d. Ian I. Mitroff and Darryl E. Chubin, "Peer Review at the NSF: A Dialectical Policy Analysis." In *Social Studies of Science,* Vol. 9 (1979): 199–232 486
e. Assorted journal commentaries on the NSF peer review controversy (1976–78):
Fred N. Zerkel, "NSF Accused of Misusing Peer Review System." *Chemical & Engineering News,* Vol. 54 (February 1976): 13 520
John Walsh, "National Science Foundation: Criticism from Conlan, GAO." Science, Vol. 191 (February 1976): 830–32 521
James W. Symington and Thomas R. Kramer, "Does Peer Review Work?" *American Scientist,* Vol. 65 (January–February 1977): 17–20 526
"NAS President Lauds NSF Peer Review System. *Chemical & Engineering News,* Vol. 55 (May 1977): 14–15 530
"NSF Peer Review System Back on the Hot Plate. "*Chemical & Engineering News,* Vol. 56 (December 1978): 18–19 532
2. Decisional Criteria (Generally):
a. Alvin M. Weinberg "Criteria for Scientific Choice." In *Minerva,* Vol. 1, No. 2 (Winter 1963): 159–171 535
b. Selection from Michael D. Reagan, *Science and the Federal Patron* (New York: Oxford University Press, 1969). Chapter 10 ("An Approach to the Priorities Problem"), pp. 270–302 548
c. Harvey Brooks, "The Problem of Research Priorities." In *Daedalus,* Vol. 107, No. 2 (Spring 1978): 171–190 582

PART VI: THE DISPUTE SURROUNDING THE ALLOCATION OF FEDERAL FUNDS FOR SCIENTIFIC RESEARCH AND ASSOCIATED SUPPORT FACILITIES

Background and Summary 605
A. General Background 627
1. Richard D. Heimovis, "Geographic Distribution of Federal Research and Development Funds." In Thomas P. Murphy, *Science, Geopolitics, and Federal Spending* (Lexington, Mass: D.C. Heath, 1971), pp. 17–30, 41–57 627
2. Selection from Anton G. Jachim, *Science Policy-Making in the United States and the Batavia Accelerator* (Carbondale and Edwardsville: So. Ill. University Press, 1975). Portion of Chapter 5 ("Decisional Criteria Challenged"), pp. 83–99) 663
3. Selections from Ken Hechler, *Towards the Endless Frontier: The History of the Committee on Science and Technology, 1959–70* (Committee Print, 1980), pp. 512–13, 516, 536–37 682
4. Selections from Bruce L.R. Smith and Joseph Karlesky, *The State of Academic Science: The Universities in the Nation's Research Effort,* Vol. I (New Rochelle: Change Magazine Press, 1977). Portions of Chapters II ("Trends in the Support and Performance of Academic Research"), pp. 40–45, and VI ("Conclusion"), pp. 229–35 686
5. Statistical tables from *Federal Funds for Research and Development, Fiscal Years 1982, 1983, and 1984 (Vol. XXXII)* (National Science Foundation Surveys of Science Resources Series, NSF 83-319), pp. 141–147 700
B. The 98th Congress: The Dispute Intensifies 707
1. Overview of the Events of 1983–84:
Philip M. Smith and Albert H. Teich, "University Research Facilities and Pork Barrel Politics" (unpublished manuscript, © 1985, 38 pp.) 707
2. Assorted Newspaper and Journal Coverage and Commentaries [arranged chronologically]:
C. Norman, "How to Win Buildings and Influence Congress." *Science,* Vol. 222 (December 1983): 1211–1213 729
J. Hook, "Colleges Lobby Congress for Facility Funds, Avoid Academic 'Peer Review' Procedures." *Congressional Quarterly Weekly Report,* Vol. 42, No. 47 (November 24, 1984): 2992–3 734

R. Cowen, "Scraping the Bottom of the Pork Barrel. *Technology Review*, Vol. 87 (November–December 1984): 9 736

C. Congress and the Politics of Geographic Allocation 738
  1. General Reading:
    a. Selections from John A. Ferejohn, *Pork Barrel Politics: Rivers and Harbors Legislation, 1947–1968* (Stanford: Stanford University Presss, 1974). Chapter 3. ("Political Benefits and Costs"), pp. 47–68 738
  2. Case Studies from the Field of Science Policy:
    a. Selections from Thomas Murphy, *Science, Geopolitics, and Federal Spending* (Lexington, Mass.: D.C. Heath, 1971). Chapters 7 ("NASA's Project Apollo Facilities Decisions"), pp. 197–223 and 10 ("AEC's Proton Accelerator"), pp. 291–326 762

PART VII: CONGRESSIONAL AND EXECUTIVE ROLES IN NATIONAL SCIENCE POLICY

Background and Summary 833

A. Congress' Role: Overview, History, Appraisal 840
  1. Gene M. Lyons' "Science and Politics: The United States of America." In *International Social Science Journal*, Vol. 28, No. 1 (1976): 26–37) 840
  2. Sanford A. Lakoff, "Congress and National Science Policy." In *Political Science Quarterly*, Vol. 89, No. 3 (Fall 1974): 589–611 852
  3. Patrick W. Hamlett, "A Typology of Technological Policymaking in the U.S. Congress." In *Science, Technology & Human Values*, Vol. 8, No. 2 (Spring 1983): 33–40 875

B. Leadership in the Executive Branch: Recent Trends and Their Implications 883
  1. Willis H. Shapley, "Executive Leadership for Science and Technology. In William T. Golden, ed. *Science Advice to the President* (New York: Pergamon Press, 1980), pp. 165–174 883
  2. Selections from James E. Katz, *Presidential Politics and Science Policy* (New York: Praeger Publishers, 1978). Chapter 5 ("National Science Policy and the Office of Management and Budget"), pp. 62–98 894

PART VIII: LESSONS FROM OTHER COUNTRIES?

Background and Summary 935

A. Science Advisory Systems and R&D in Europe: Comparative Perspectives. 940
  1. Ronald Brickman, "Comparative Approaches to R&D Policy Coordination." In *Policy Sciences Journal*, Vol. 11 (1979): 73–91 940
  2. Ronald Brickman and Aire Rip, "Science Policy Advisory Councils in France, the Netherlands, and the United States, 1955–1977: A Comparative Analysis." In *Social Studies of Science*, Vol. 9 (Summer 1979): 167–198 959
  3. Michael Mulkay, "The Mediating Role of Scientific Elites." In *Social Studies of Science*, Vol. 6 (1976): 445–470 991

B. Alternative Criteria for Scientific Choice? 1017
  1. I. Th. M. Snellen, "Social Merit as a Criterion of Scientific Choice: Its Application in Dutch Science Policy." In *Minerva*, Vol. 21 (Spring 1983): 16–36 1017

NOTE

The materials included in this reader include many excerpts and articles which are copyrighted. They are reproduced here for the use of the Committee on Science and Technology of the House of Representatives, its members, and the witnesses coming before it only. Any further reproduction should not be made without the consent of the copyrightholder, where applicable.

PART I

THE LEGACY OF THE WESTERN PHILOSOPHICAL TRADITION

BACKGROUND AND SUMMARY

A. "Should Experts Rule?"—Selections from Plato and Aristotle

Although the concept of "expertise" has distinctly modern connotations, the suggestion that the "man of knowledge" has a special claim to decisionmaking authority is plainly not of recent origin. On the contrary, this idea is as old and as venerable as the western philosophical tradition itself.

Plato may well represent the most influential early proponent of what we would today call "expertise." Time and again in the Platonic dialogues, Socrates endeavors to elicit an acknowledgement from his interlocuters that where, for example, the necessities of bodily health are concerned, we ought to place greater faith in the judgments of the doctor than in those of the patient, that the architect is better qualified than the average citizen to design a house, and so forth. These seemingly innocuous examples of the need to rely on the advice of those more knowledgable and experienced are meant to serve an important political purpose, as can be seen in the first selection in Part I.

The selection in question is from a famous passage that appears in Book VI of Plato's *Republic.* In this passage, Socrates compares the art of rulership to the art or science of navigation. The point of the analogy is to suggest that, just as havoc would ensue if sailors rather than captains were to attempt to guide a ship at sea, so the political realm is destined to remain in a constant state of turmoil and confusion until he who is best qualified to rule—the philosopher-king—is permitted to take the helm. There is, Plato is suggesting, an inherent tension between the principle of deference to authority on the one hand and that of democracy (understood as popular rule or will or the majority) on the other.

The passage from the *Republic* discloses, as it were, the ultimate objective underlying his repeated appeals to the authority of "experts." But the comparison between the captain and the philosopher-king also reveals that Plato recognized full well the difficulties presented in attempting to extend the principle of deference to "the wise" into the realm of politics. Adimantus' critical reception of Socrates' suggestion foreshadows the result: the proposal for a philosopher-king is doomed to failure, if only because "the people" (convinced that he is indeed "useless") will never accede to the idea that the philosopher should rule.

In general terms, Aristotle may be said to stand a good deal closer to Plato than do many modern commentators on the relative merits of relying on "experts" and average citizens in matters of politics. Nonetheless, Aristotle found much to criticize in Plato's political philosophy. In the passage from his *Politics* which comprises the second selection of Part I, A, Aristotle offers a partial defense of democracy, a form of government Plato felt had little to commend it. Of particular relevance here is Aristotle's suggestion that, in certain contexts at any rate, the combined experience and understanding of average citizens may provide a more reliable standard for decisionmaking than the judgments of their supposed superiors. "Whether this principle can apply to every democracy" is not clear, but in Aristotle's view it suffices to show that the idea that "the multitude ought to be supreme rather than the few good . . . contain[s] an element of truth."

A matter of interest in its own right, the contrasting views of Plato and Aristotle on the question of the extent to which public matters should be entrusted to the "man of knowledge" or to average citizens also suggests that the tension between the claims of "democracy" and "expertise" is a timeless and fundamental problem of political life.

B. "THE NEW SCIENCE" AND ITS IMPLICATIONS

While the ancient world, and especially the Greeks, held scientists in high esteem, little concern was then voiced (as is so frequently the case today) that science *per se* might somehow have serious political implications or that its practitioners might be in a position to exert undue influence on the affairs of society at large. In large measure, this may be said to stem from the fact that the predominant view of science in the ancient world was that it was an activity essentially *contemplative* in nature. That is, the scientist was conceived as one whose principal task was to observe natural phenomena and to attempt to render the natural world more intelligible to human understanding by dividing and organizing the various phenomena (according to subject-matter, manner of apprehension, etc.) into distinct categories. Although, to be sure, the ancient Greeks (for example) were accomplished artisans and craftsmen, up through the Middle Ages "Nature" was not generally conceived as a repository or crucible for human experimentation, nor was "Science" understood as an activity whose ultimate objective was to render the natural world more susceptible to human control or manipulation.

This prevailing view of science and nature underwent a dramatic transformation during the latter 16th and early 17th centuries. Ushered in by such notable figures in the history of philosophy as Sir Francis Bacon (1561–1626), Rene Descartes (1596–1650), and Thomas Hobbes (1588–1679), the so-called "New Science" was to have profound consequences for the future of western civilization. At the risk of oversimplification, it may be said that the advocates of the New Science sought, in conscious opposition to the classical and medieval conceptions, to transform science from a passive, contemplative activity into a more "this-worldly," rigorously experimental and practical or "utilitarian" enterprise. The selections for Part I, B, representing brief passages from works by the three philosophers above mentioned—Bacon, Descartes, and Hobbes—are intended to provide some sense of the central principles and aims of the 'New Science.' Worthy of special note are Descartes' characterization (in Book VI of his *Discourse on Method)* of the aim of his new physics as that of enabling man to become the "lords and possessors of nature," Bacon's statement (in *The New Organon*) to the effect that "the true and lawful goal of the sciences, is none other than this: that human life be endowed with new discoveries and powers," and Hobbes' suggestion (in his *Leviathan*), concerning the course mankind should pursue, that "reason is the pace; increase of science the way; and benefit of mankind, the end."

As these brief statements (and others in the selections) suggest, and despite important differences between them, the advocates of the 'New Science' were united in their determination to remove what they regarded as obstacles to the advancement of a more systematic, experimental type of science whose effects, they were persuaded, could immeasurably enhance the lot of humankind. Needless to say, developments in the intervening three and a half centuries have more than redeemed their faith both in the possibilities for the advancement of the sciences as disciplines and in the rewards that might be reaped from the processes of scientific and technological innovation. But it also goes without saying that the cause of scientific progress for which they served as such eloquent spokesman has, in its more recent incarnation, had some profoundly disturbing side-effects and implications. Some might be inclined to suggest that scientists and politicians in the modern era have failed adequately to heed the lesson contained in Bacon's warning to his contemporaries (in § 70 of Book I of *The New Organon,* reproduced below): "And even when [men] seek to educe some science and theory from their experiments, . . . they almost always turn . . . with overhasty and unseasonable eagerness to practice." Be that as it may (and however easy it may be to suppose the revolution they wrought was inevitable), the proponents of the 'New Science' clearly played a critically important role in unleashing a powerful new force in the world. As the question which appears as the title of a recent book, "Science: Servant or Master?" [1], serves to remind us, we might well be said to be the inheritors of their ambiguous legacy.

---

[1] Hans J. Morgenthau, *Science: Servant or Master?* (New York: New American Library, 1972).

6

A. "SHOULD EXPERTS RULE?—SELECTIONS FROM PLATO AND ARISTOTLE

THE
COLLECTED DIALOGUES OF
PLATO

INCLUDING THE LETTERS

Edited by

EDITH HAMILTON

and

HUNTINGTON CAIRNS

With Introduction and Prefatory Notes

⁂

BOLLINGEN SERIES LXXI

PANTHEON BOOKS

You may hear, then, that I think that what they say is true.

e How, then, he replied, can it be right to say that our cities will
never be freed from their evils until the philosophers, whom we admit
to be useless to them, become their rulers?

Your question, I said, requires an answer expressed in a comparison
or parable.

And you, he said, of course, are not accustomed to speak in comparisons!

So, said I, you are making fun of me after driving me into such
an impasse of argument. But, all the same, hear my comparison so
488 that you may still better see how I strain after imagery. For so cruel
is the condition of the better sort in relation to the state that there is
no single thing like it in nature. But to find a likeness for it and a
defense for them one must bring together many things in such a combination
as painters mix when they portray goat stags and similar
creatures. Conceive this sort of thing happening either on many ships
or on one. Picture a shipmaster in height and strength surpassing all
b others on the ship, but who is slightly deaf and of similarly impaired
vision, and whose knowledge of navigation is on a par with his
sight and hearing. Conceive the sailors to be wrangling with one another
for control of the helm, each claiming that it is his right to steer
though he has never learned the art and cannot point out his teacher
or any time when he studied it. And what is more, they affirm that it
cannot be taught at all, but they are ready to make mincemeat of
anyone who says that it can be taught, and meanwhile they are always
c clustered about the shipmaster importuning him and sticking

at nothing to induce him to turn over the helm to them. And sometimes, if they fail and others get his ear, they put the others to death or cast them out from the ship, and then, after binding and stupefying the worthy shipmaster with mandragora or intoxication or otherwise, they take command of the ship, consume its stores and, drinking and feasting, make such a voyage of it as is to be expected from such, and as if that were not enough, they praise and celebrate as a navi- d
gator, a pilot, a master of shipcraft, the man who is most cunning to lend a hand in persuading or constraining the shipmaster to let them rule, while the man who lacks this craft they censure as useless. They have no suspicion that the true pilot must give his attention to the time of the year, the seasons, the sky, the winds, the stars, and all that pertains to his art if he is to be a true ruler of a ship, and that he does not believe that there is any art or science of seizing the helm with or without the consent of others, or any possibility of mastering this e
alleged art and the practice of it at the same time with the science of navigation. With such goings on aboard ship do you not think that 489
the real pilot would in very deed be called a stargazer, an idle babbler, a useless fellow, by the sailors in ships managed after this fashion?

Quite so, said Adimantus.

You take my meaning, I presume, and do not require us to put the comparison to the proof and show that the condition we have described is the exact counterpart of the relation of the state to the true philosophers.

It is indeed, he said.

To begin with, then, teach this parable to the man who is surprised that philosophers are not honored in our cities, and try to con- b
vince him that it would be far more surprising if they were honored.

I will teach him, he said.

And say to him further, You are right in affirming that the finest spirits among the philosophers are of no service to the multitude. But bid him blame for this uselessness, not the finer spirits, but those who do not know how to make use of them. For it is not the natural course of things that the pilot should beg the sailors to be ruled by him or that wise men should go to the doors of the rich. The author of that epigram was a liar. But the true nature of things is that whether c
the sick man be rich or poor he must needs go to the door of the physician, and everyone who needs to be governed to the door of the man who knows how to govern, not that the ruler should implore his natural subjects to let themselves be ruled, if he is really good for anything. But you will make no mistake in likening our present political rulers to the sort of sailors we were just describing, and those whom these call useless and stargazing ideologists to the true pilots.

Just so, he said.

9

The Basic Works of

ARISTOTLE

EDITED AND WITH AN INTRODUCTION BY

RICHARD McKEON

Dean of the Division of the Humanities, University of Chicago

RANDOM HOUSE · NEW YORK

There is also a doubt as to what is to be the supreme power in the state:—Is it the multitude? Or the wealthy? Or the good? Or the one best man? Or a tyrant? Any of these alternatives seems to involve disagreeable consequences. If the poor, for example, because they are more in number, divide among themselves the property of the rich—is not this unjust? No, by heaven (will be the reply), for the supreme authority justly willed it. But if this is not injustice, pray what is? Again, when in the first division all has been taken, and the majority divide anew the property of the minority, is it not evident, if this goes on, that they will ruin the state? Yet surely, virtue is not the ruin of those who possess her, nor is justice destructive of a state; and therefore this law of confiscation clearly cannot be just. If it were, all the acts of a tyrant must of necessity be just; for he only coerces other men by superior power, just as the multitude coerce the rich. But is it just then that the few and the wealthy should be the rulers? And what if they, in like manner, rob and plunder the people—is this just? If so, the other case will likewise be just. But there can be no doubt that all these things are wrong and unjust.

Then ought the good to rule and have supreme power? But in that

30 Cp. i. 1252^{b} 27; *N. Eth.* i. 1097^{b} 6.

30 case everybody else, being excluded from power, will be dishonoured.
For the offices of a state are posts of honour; and if one set of men
always hold them, the rest must be deprived of them. Then will it
be well that the one best man should rule? Nay, that is still more
oligarchical, for the number of those who are dishonoured is thereby
increased. Some one may say that it is bad in any case for a man, subject
35 as he is to all the accidents of human passion, to have the supreme
power, rather than the law. But what if the law itself be democratical
or oligarchical, how will that help us out of our difficulties?[31]
Not at all; the same consequences[32] will follow.

11 Most of these questions may be reserved for another occasion.[33]
40 The principle that the multitude ought to be supreme rather than the
few best is one that is maintained, and, though not free from difficulty,
yet seems to contain an element of truth. For the many, of
1281b whom each individual is but an ordinary person, when they meet together
may very likely be better than the few good, if regarded not individually
but collectively, just as a feast to which many contribute is
better than a dinner provided out of a single purse. For each individual
among the many has a share of virtue and prudence, and
5 when they meet together, they become in a manner one man, who
has many feet, and hands, and senses; that is a figure of their mind
and disposition. Hence the many are better judges than a single man
of music and poetry; for some understand one part, and some another,
10 and among them they understand the whole. There is a similar
combination of qualities in good men, who differ from any individual
of the many, as the beautiful are said to differ from those who
are not beautiful, and works of art from realities, because in them the
scattered elements are combined, although, if taken separately, the
eye of one person or some other feature in another person would be
15 fairer than in the picture. Whether this principle can apply to every
democracy, and to all bodies of men, is not clear. Or rather, by heaven,
in some cases it is impossible of application; for the argument would
equally hold about brutes; and wherein, it will be asked, do some
20 men differ from brutes? But there may be bodies of men about
whom our statement is nevertheless true. And if so, the difficulty
which has been already raised,[34] and also another which is akin to
it—viz. what power should be assigned to the mass of freemen and
25 citizens, who are not rich and have no personal merit—are both
solved.

---

[31] Cp. 1282b 6.
[32] Cp. ll. 11–34.
[33] cc. 12–17, iv., vi.
[34] c. 10.

12

B. 'The New Science' and Its Implications

FRANCIS BACON

BARON OF VERULAM, VISCOUNT ST. ALBANS

The New Organon

and Related Writings

Edited, with an Introduction, by

FULTON H. ANDERSON

Professor of Philosophy, University of Toronto

LAP

THE LIBERAL ARTS PRESS

NEW YORK

LXXXI

Again there is another great and powerful cause why the sciences have made but little progress, which is this. It is not possible to run a course aright when the goal itself has not been rightly placed. [Now the true and lawful goal of the sciences is none other than this: that human life be endowed with new discoveries and powers.] But of this the great majority have no feeling, but are merely hireling and professorial; except when it occasionally happens that some workman of acuter wit and covetous of honor applies himself to a new invention, which he mostly does at the expense of his fortunes. But in general, so far are men from proposing to themselves to augment the mass of arts and sciences, that from the mass already at hand they neither take nor look for anything more than what they may turn to use in their lectures, or to gain, or to reputation, or to some similar advantage. And if any one out of all the multitude court science with honest affection and for her own sake, yet even with him the object will be found to be rather the variety of contemplations and doctrines than the severe and rigid search after truth. And if by chance there be one who seeks after truth in earnest, yet even he will propose to himself such a kind of truth as shall yield satisfaction to the mind and understanding in rendering causes for things long since discovered, and not the truth which shall lead to new assurance of works and new light of axioms. If then the end of the sciences has not as yet been well placed, it is not strange that men have erred as to the means.

And it appears worthy of remark in Solomon that, though mighty in empire and in gold, in the magnificence of his works, his court, his household, and his fleet, in the luster of his name and the worship of mankind, yet he took none of these to glory in, but pronounced that "The glory of God is to conceal a thing; the glory of the king to search it out."

Again, let a man only consider what a difference there is between the life of men in the most civilized province of Europe, and in the wildest and most barbarous districts of New India; he will feel it be great enough to justify the saying that "man is a god to man," not only in regard to aid and benefit, but also by a comparison of condition. And this difference comes not from soil, not from climate, not from race, but from the arts.

Again, it is well to observe the force and virtue and consequences of discoveries, and these are to be seen nowhere more conspicuously than in those three which were unknown to the ancients, and of which the origin, though recent, is obscure and inglorious; namely, printing, gunpowder, and the magnet. For these three have changed the whole face and state of things throughout the world; the first in literature, the second in warfare, the third in navigation; whence have followed innumerable changes, insomuch that no empire, no sect, no star seems to have exerted greater power and influence in human affairs than these mechanical discoveries.

Further, it will not be amiss to distinguish the three kinds and, as it were, grades of ambition in mankind. The first is of those who desire to extend their own power in their native country, a vulgar and degenerate kind. The second is of those who labor to extend the power and dominion of their country among men. This certainly has more dignity, though not less covetousness. But if a man endeavor to establish and extend the power and dominion of the human race itself over the universe, his ambition (if ambition it can be called) is without doubt both a more wholesome and a more noble thing than the other two. Now the empire of man over things depends wholly on the arts and sciences. For we cannot command nature except by obeying her.

Again, if men have thought so much of some one particular discovery as to regard him as more than man who has been able by some benefit to make the whole human race his debtor, how much higher a thing to discover that by means of which all things else shall be discovered with ease! And yet (to speak the whole truth), as the uses of light are infinite in enabling us to walk, to ply our arts, to read, to recognize one another—and nevertheless the very beholding of the light is itself a more excellent and a fairer thing than all the uses of it—so assuredly the very contemplation of things as they are, without superstition or imposture, error or confusion, is in itself more worthy than all the fruit of inventions.

Lastly, if the debasement of arts and sciences to purposes of wickedness, luxury, and the like, be made a ground of objection, let no one be moved thereby. For the same may be said of all earthly goods: of wit, courage, strength, beauty, wealth, light itself, and the rest. Only let the human race recover that right over nature which belongs to it by divine bequest, and let power be given it; the exercise thereof will be governed by sound reason and true religion.

XCII

But by far the greatest obstacle to the progress of science and to the undertaking of new tasks and provinces therein is found in this—that men despair and think things impossible. For wise and serious men are wont in these matters to be altogether distrustful, considering with themselves the obscurity of nature, the shortness of life, the deceitfulness of the senses, the weakness of the judgment, the difficulty of experiment, and the like; and so supposing that in the revolution of time and of the ages of the world the sciences have their ebbs and flows; that at one season they grow and flourish, at another wither and decay, yet in such sort that when they have reached a certain point and condition they can advance no further. If therefore anyone believes or promises more, they think this comes of an ungoverned and unripened mind, and that such attempts have prosperous beginnings, become difficult as they go on, and end in confusion. Now since these are thoughts which naturally present themselves to men grave and of great judgment, we must take good heed that we be not led away by our love for a most fair and excellent object to relax or diminish the severity of our judgment. We must observe diligently what encouragement dawns upon us and from what quarter, and, putting aside the lighter breezes of hope, we must thoroughly sift and examine those which promise greater steadiness and constancy. Nay, and we must take state prudence too into our counsels, whose rule is to distrust, and to take the less favorable view of human affairs. I am now therefore to speak touching hope, especially as I am not a dealer in promises, and wish neither to force nor to ensnare men's judgments, but to lead them by the hand with their good will. And though the strongest means of inspiring hope will be to bring men to particulars, especially to particulars digested and arranged in my Tables of Discovery (the subject partly of the second, but much more of the fourth part of my Instauration), since this is not merely the promise of the thing but the thing itself; nevertheless, that everything may be done with gentleness, I will proceed with my plan of preparing men's minds, of which preparation to give hope is no unimportant part. For without it the rest tends rather to make men sad (by giving them a worse and meaner opinion of things as they are than they now have, and making them more fully to feel and know the unhappiness of their own condition) than to induce any alacrity or to whet their industry in making trial. And therefore it is fit that I publish and set forth those conjectures of mine which make hope in this matter reasonable, just as Columbus did, before that wonderful voyage of his across the Atlantic, when he gave the reasons for his conviction that new lands and continents might be discovered besides those which were known before; which reasons, though rejected at first, were afterwards made good by experience, and were the causes and beginnings of great events.

And even when they seek to educe some science or theory from their experiments, they nevertheless almost always turn aside with overhasty and unseasonable eagerness to practice; not only for the sake of the uses and fruits of the practice, but from impatience to obtain in the shape of some new work an assurance for themselves that it is worth their while to go on; and also to show themselves off to the world, and so raise the credit of the business in which they are engaged. Thus, like Atalanta, they go aside to pick up the golden apple, but meanwhile they interrupt their course, and let the victory escape them. But in the true course of experience, and in carrying it on to the effecting of new works, the divine wisdom and order must be our pattern. Now God on the first day of creation created light only, giving to that work an entire day, in which no material substance was created. So must we likewise from experience of every kind first endeavor to discover true causes and axioms; and seek for experiments of Light, not for experiments of Fruit. For axioms rightly discovered and established supply practice with its instruments, not one by one, but in clusters, and draw after them trains and troops of works. Of the paths, however, of experience, which no less than the paths of judgment are impeded and beset, I will speak hereafter; here I have only mentioned ordinary experimental research as a bad kind of demonstration.

19

DISCOURSE

ON THE

METHOD OF RIGHTLY CONDUCTING THE REASON, AND SEEKING TRUTH IN THE SCIENCES

BY

RENÉ DESCARTES

TRANSLATED FROM THE FRENCH AND COLLATED WITH
THE LATIN BY

JOHN VEITCH, LL.D.
LATE PROFESSOR OF LOGIC AND RHETORIC IN THE
UNIVERSITY OF GLASGOW

AUTHORIZED REPRINT

CHICAGO
THE OPEN COURT PUBLISHING COMPANY
1924

I have never made much account of what has proceeded from my own mind; and so long as I gathered no other advantage from the Method I employ beyond satisfying myself on some difficulties belonging to the speculative sciences, or endeavouring to regulate my actions according to the principles it taught me, I never thought myself bound to publish anything respecting it. For in what regards manners, every one is so full of his own wisdom, that there might be found as many reformers as heads, if any were allowed to take upon themselves the task of mending them, except those whom God has constituted the supreme rulers of his people, or to whom he has given sufficient grace and zeal to be prophets; and although my speculations greatly pleased myself, I believed that others had theirs, which perhaps pleased them still more. But as soon as I had acquired some general notions respecting Physics, and beginning to make trial of them in various particular difficulties, had observed how far they can carry us, and how much they differ from the principles that have been employed up to the present time, I believed that I could not keep them concealed without sinning grievously against the law by which we are bound to promote, as far as in us lies, the general good of mankind. For by them I perceived it to be possible to arrive at knowledge highly useful in life, and in room of the Speculative Philosophy usually taught in the Schools, to discover a Practical, by means of which, knowing the force and action of fire, water, air, the stars, the heavens, and all the other bodies that surround us, as distinctly as we know the various crafts of our artizans, we might also apply them in the same way to all the uses to which they are adapted, and thus render ourselves the lords and possessors of nature. And this is a result to be desired, not only in order to the invention of an infinity of arts, by which we might be enabled to enjoy without any trouble the fruits of the earth, and all its comforts, but also and especially for the preservation of health, which is without doubt, of all the blessings of this life, the first and fundamental one; for the mind is so intimately dependent upon the condition and relation of the organs of the body, that if any means can ever be found to render men wiser and more ingenious than hitherto, I believe that it is in Medicine they must be sought for. It is true that the science of Medicine, as it now exists, contains few things whose utility is very remarkable: but without any wish to depreciate it, I am confident that there is no one, even among those whose profession it is, who does not admit that all at present known in it is almost nothing in comparison of what remains to be discovered; and that we could free ourselves from an infinity of maladies of body as well as of mind, and perhaps also even from the debility of age, if we had sufficiently ample knowledge of their causes, and of all the remedies provided for us by Nature. But since I designed to employ my whole life in the search after so necessary a Science, and since I had fallen in with a path which seems to me such, that if any one follow it he must inevitably reach the end desired, unless he be hindered either by the shortness of life or the want of experiments, I judged that there could be no more effectual provision against these two impediments than if I were faithfully to communicate to the public all the little I might myself have found, and incite men of superior genius to strive to proceed farther, by contributing, each according to his inclination and ability, to the experiments which it would be necessary to make, and also by informing the public of all they might discover, so that, by the last beginning where those before them had left off, and thus connecting the lives and labours of many, we might collectively proceed much farther than each by himself could do.

23

THE

ENGLISH WORKS

OF

THOMAS HOBBES

OF MALMESBURY;

NOW FIRST COLLECTED AND EDITED

BY

SIR WILLIAM MOLESWORTH, BART.

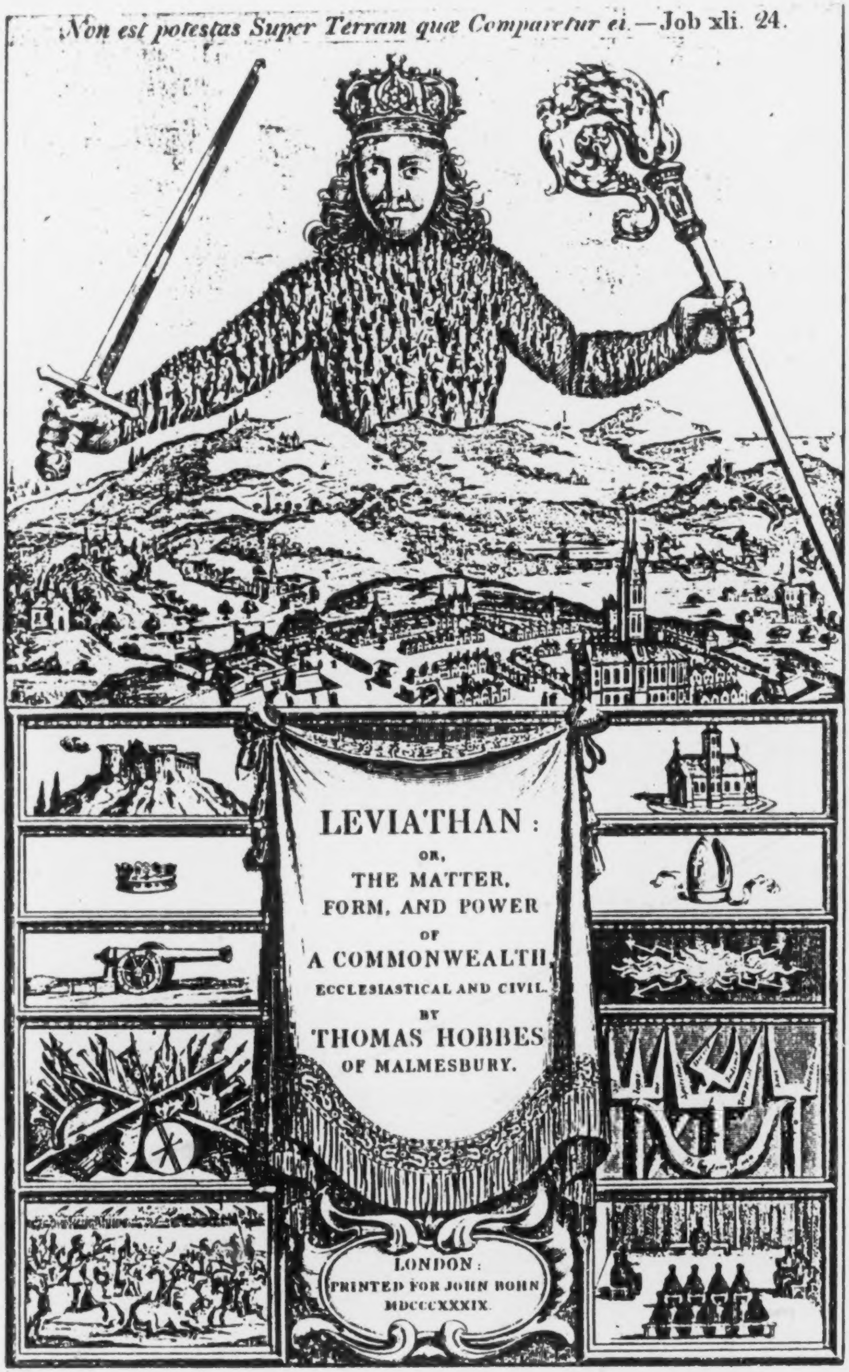
VOL III.

LONDON:
JOHN BOHN,
HENRIETTA STREET, COVENT GARDEN.

MDCCCXXXIX.

11

34705
46



Non est potestas Super Terram quæ Comparetur ei.—Job xli. 24.

LEVIATHAN:
OR,
THE MATTER,
FORM, AND POWER
OF
A COMMONWEALTH,
ECCLESIASTICAL AND CIVIL.
BY
THOMAS HOBBES
OF MALMESBURY.

LONDON:
PRINTED FOR JOHN BOHN
MDCCCXXXIX.

Science.

By this it appears that reason is not, as sense and memory, born with us; nor gotten by experience only, as prudence is; but attained by industry; first in apt imposing of names; and secondly by getting a good and orderly method in proceeding from the elements, which are names, to assertions made by connexion of one of them to another; and so to syllogisms, which are the connexions of one assertion to another, till we come to a knowledge of all the consequences of names appertaining to the subject in hand; and that is it, men call SCIENCE. And whereas sense and memory are but knowledge of fact, which is a thing past and irrevocable. *Science* is the knowledge of consequences, and dependance of one fact upon another: by which, out of that we can presently do, we know how to do something else when we will, or the like another time; because when we see how any thing comes about, upon what causes, and by what manner; when the like causes come into our power, we see how to make it produce the like effects.

Children therefore are not endued with reason

Science.

at all, till they have attained the use of speech; but are called reasonable creatures, for the possibility apparent of having the use of reason in time to come. And the most part of men, though they have the use of reasoning a little way, as in numbering to some degree; yet it serves them to little use in common life; in which they govern themselves, some better, some worse, according to their differences of experience, quickness of memory, and inclinations to several ends; but specially according to good or evil fortune, and the errors of one another. For as for *science*, or certain rules of their actions, they are so far from it, that they know not what it is. Geometry they have thought conjuring: but for other sciences, they who have not been taught the beginnings and some progress in them, that they may see how they be acquired and generated, are in this point like children, that having no thought of generation, are made believe by the women that their brothers and sisters are not born, but found in the garden.

But yet they that have no *science*, are in better, and nobler condition, with their natural prudence; than men, that by mis-reasoning, or by trusting them that reason wrong, fall upon false and absurd general rules. For ignorance of causes, and of rules, does not set men so far out of their way, as relying on false rules, and taking for causes of what they aspire to, those that are not so, but rather causes of the contrary.

To conclude, the light of human minds is perspicuous words, but by exact definitions first snuffed, and purged from ambiguity; *reason* is the *pace*; increase of *science*, the *way*; and the benefit of mankind, the *end*. And, on the contrary, metaphors, and senseless and ambiguous words, are like *ignes fatui*; and reasoning upon them is wandering amongst innumerable absurdities; and their end, contention and sedition, or contempt.

Prudence and sapience, with their difference

As much experience, is *prudence*; so, is much science *sapience*. For though we usually have one name of wisdom for them both, yet the Latins did always distinguish between *prudentia* and *sapientia*; ascribing the former to experience, the latter to science. But to make their difference appear more clearly, let us suppose one man endued with an excellent natural use and dexterity in handling his arms; and another to have added to that dexterity, an acquired science, of where he can offend, or be offended by his adversary, in every possible posture or guard: the ability of the former, would be to the ability of the latter, as prudence to sapience; both useful; but the latter infallible. But they that trusting only to the authority of books, follow the blind blindly, are like him that, trusting to the false rules of a master of fence, ventures presumptuously upon an adversary, that either kills or disgraces him.

Signs of science.

The signs of science are some, certain and infallible; some, uncertain. Certain, when he that pertendeth the science of any thing, can teach the same; that is to say, demonstrate the truth thereof perspicuously to another; uncertain, when only some particular events answer to his pretence, and upon many occasions prove so as he says they must. Signs of prudence are all uncertain; because to observe by experience, and remember all circumstances that may alter the success, is impossible.

CHAPTER X.

OF POWER, WORTH, DIGNITY, HONOUR, AND WORTHINESS.

PART I.
10.

Power.

THE POWER *of a man*, to take it universally, is his present means; to obtain some future apparent good; and is either *original* or *instrumental*.

Natural power, is the eminence of the faculties of body, or mind: as extraordinary strength, form, prudence, arts, eloquence, liberality, nobility. *Instrumental* are those powers, which acquired by these, or by fortune, are means and instruments to acquire more: as riches, reputation, friends, and the secret working of God, which men call good luck. For the nature of power, is in this point, like to fame, increasing as it proceeds; or like the motion of heavy bodies, which the further they go, make still the more haste.

The greatest of human powers, is that which is compounded of the powers of most men, united by consent, in one person, natural, or civil, that has the use of all their powers depending on his will; such as is the power of a common-wealth: or depending on the wills of each particular; such as is the power of a faction or of divers factions leagued. Therefore to have servants, is power; to have friends, is power: for they are strengths united.

Also riches joined with liberality, is power; because it procureth friends, and servants: without liberality, not so; because in this case they defend not; but expose men to envy, as a prey.

Reputation of power, is power; because it draweth with it the adherence of those that need protection.

So is reputation of love of a man's country, called popularity, for the same reason.

Power.

Also, what quality soever maketh a man beloved, or feared of many; or the reputation of such quality, is power; because it is a means to have the assistance, and service of many.

Good success is power; because it maketh reputation of wisdom, or good fortune; which makes men either fear him, or rely on him.

Affability of men already in power, is increase of power; because it gaineth love.

Reputation of prudence in the conduct of peace or war, is power; because to prudent men, we commit the government of ourselves, more willingly than to others.

Nobility is power, not in all places, but only in those commonwealths, where it has privileges: for in such privileges, consisteth their power.

Eloquence is power, because it is seeming prudence.

Form is power; because being a promise of good, it recommendeth men to the favour of women and strangers.

The sciences, are small power; because not eminent; and therefore, not acknowledged in any man; nor are at all, but in a few, and in them, but of a few things. For science is of that nature, as none can understand it to be, but such as in a good measure have attained it.

Arts of public use, as fortification, making of engines, and other instruments of war; because they confer to defence, and victory, are power: and though the true mother of them, be science, namely the mathematics; yet, because they are brought into the light, by the hand of the artificer, they be esteemed, the midwife passing with the vulgar for the mother, as his issue.

PART II

THE PLACE OF SCIENCE AND EXPERTISE IN A DEMOCRATIC POLITY: SELECTIONS FROM THE TRADITION OF ANGLO-AMERICAN POLITICAL THOUGHT

BACKGROUND AND SUMMARY

A. The Early Republic

As the historian John C. Greene has noted,[1] early American science partook very much of the spirit of "Baconian utilitarianism" as outlined in the 'Background and Summary' statement accompanying the readings in Part I. Leading American statesmen of the Founding era, like their Enlightenment counterparts in Europe, also regarded science as a progressive force on the societal level, one capable of freeing mankind from the bondage of ignorance and superstition.

The Baconian theme figures especially prominently in the first selection in Part II, Benjamin Franklin's "A Proposal for Promoting Useful Knowledge in the British Plantations in America" of 1742, in which the author calls for the formation of an American Philosophical Society to facilitate communication among "ingenious men" regarding experiments and discoveries which promise "to increase the power of man over matter, and multiply the conveniences or pleasures of life." That the cause of the advancement of science—both as a theoretical and a practical matter—continued to attract the attention of leading American statesmen after the establishment of the new republic can be seen in passages from the two selections that follow: George Washington's "First Annual Address to Congress" (1790), and a letter written by Thomas Jefferson in 1799 which reflects the Founders' conviction that the progress of science would lead to the moral improvement of the human species and the perfection of the institutions of government.

The article by Greene seeks, as its title suggests, to present an overview of the character and development of American science from the early years of the Republic up to the mid-nineteenth century. The article is of interest in this context chiefly for the insight it provides into the early history of the many scientific societies and associations that sprang up in the new nation, some of which—perhaps most notably the American Academy of Arts and Sciences in Boston—have retained prominence as spokesman for the interests of organized science in the contemporary setting. On the whole, Greene concludes, "the record of the societies for promoting knowledge in the period before 1860 was an honorable, but not brilliant, one" (p. 19).

B. Twentieth Century Perspectives

The cause of the promotion of knowledge gathered pace in the 19th and 20th centuries, as many leading American statemen, educators, and others advocated a serious commitment to specialized training in the sciences and other "useful" disciplines. They did so at least in part on the grounds that a body of well-trained "professionals" in a number of fields might prove valuable to the developing nation in its efforts to cope with a variety of social and political problems. To be sure, a faith in the emerging notion of "expertise" and all it implied was not universally shared, nor was it always predominant; the Jacksonian period, for example, has been widely interpreted as reflecting an American era of faith in "the omnicompetence of the common man" as against the claims of economic, social, and political elites. That a widespread belief in the "curative" potential of expertise retained vitality as the United States entered the twentieth century, however, can readily be discerned in the aims and programs of the Progressive Movement, which championed the establishment of a permanent, non-partisan core of civil service professionals to free state and federal government of the abuses of "party bosses" and the so-called "spoils system."

---

[1] See his article on "Science, Learning, and Utility: Patterns of Organization in the Early American Republic" which appears immediately following the selection of statements by the Founding Fathers in this section.

With the advent of the Depression, the beginning of the New Deal, and a growing awareness of the complexity of the issues which would be facing the nation as it entered a new industrial era, however, the potential for conflict between the forces of "democracy" and "expertise" began to be appreciated as never before.

The renowned American philosopher and educator John Dewey addressed this problem extensively in his 1927 treatise, *The Public and Its Problems.* (Salient passages from two chapters of that work are presented in the first selection under Part II, B, of the Reader.) While acknowledging the difficulty of ensuring that "the public" play a meaningful role in a world increasingly characterized by complex and technical policy matters, Dewey was anxious to deny the assertions of those (perhaps most prominently Walter Lippmann) who had come to regard experts as the only competent or reliable decision-makers and who argued that "the prime obstruction" to ready acceptance of this view lay "in the superstitious belief that there is a public concerned to determine the formation and execution of general social policies" (p.124).

Dewey was plainly distressed by this latter-day revival "of the Platonic notion that philosophers should be kings" (p. 205). He asserted that "A class of experts is so inevitably removed from common interests as to become a class with private interests and private knowledge" (p. 207); that "No government by experts in which the masses do not have a chance of inform the experts as to their needs can be anything but an oligarchy managed in the interests of the few" (p. 208). To prevent a further "eclipse of the public" at the hands of experts, Dewey urged "the improvement of the methods and conditions of debate, discussion, and persuasion" *(ibid.).* His suggestion that "It is not necessary that the many . . . have the knowledge and skill necessary to carry on the needed investigations [themselves]; what is required is that they have the ability to judge of the bearing of the knowledge supplied by others upon common concerns" (p. 209) has been a recurrent theme in the post-War debate on the decision-making roles of experts and the lay public.[2]

As the debate between Dewey, Lippmann, and other luminary figures in New Deal-era America raged on, the issue of the proper place of experts in modern society was also attracting attention in the European context. In late 1930, the famous British political scientist Harold J. Laski entered the fray with the publication of an article entitled "The Limitations of The Expert."

In many of its themes and contentions (e.g., the suggestion, on page 109, that "Government by experts would . . . mean after a time government in the interest of experts"), Laski's warnings about the dangers of expertise are similar to those of Dewey. But in undertaking to describe in rather more detail what he regards as the peculiar and worrisome propensities of "the expert" as a species, Laski charts a somewhat different course. The article is full of provocative remarks to the effect that, for instance, the expert "sacrifices the insight of common sense to the intensity of experience" (p. 102) and "tends to make his subject the measure of life, instead of making life the measure of his subject" (p. 105).

Lest it be thought that concern over the potentially pernicious implications of a reliance upon experts was confined to left-leaning figures—Laski, after all, was a self-proclaimed Socialist, and Dewey of similar persuasion in matters of politics—the reader might do well to ponder the bearing of the last selection in Part II, B. In the same Farewell Address in which he shocked scientists and other "establishment" elements of society by alluding to the specter of an emerging "military-industrial complex" in America, Dwight D. Eisenhower—a President who most would acknowledge was a bona fide conservative—urged the continued support of scientific research but warned of the dangerous possibility implicit in this and allied practices, *viz.*, that "public policy could itself become the captive of a scientific-technological elite" (p. 1039).

The suggestion has been made that Eisenhower's farewell address was actually the product of one of his advisers and that the President did not fully appreciate the ramifications of his remarks. Be that as it may, and especially given the esteem in which the spokesman himself was generally held, Eisenhower's warning undoubtedly had a sobering effect upon American society as the nation emerged to a position of pre-eminence in the post-War world.

---

[2] See, for example, the article by Kenneth Prewitt entitled "Scientific Illiteracy and Democratic Theory" which is reproduced in Part IV of this Reader.

36

A. The Early Republic

THE WRITINGS

OF

BENJAMIN FRANKLIN

II

COLLECTED AND EDITED
WITH A LIFE AND INTRODUCTION

BY

ALBERT HENRY SMYTH

VOLUME II
1722–1750

HASKELL HOUSE PUBLISHERS LTD.
Publishers of Scarce Scholarly Books
NEW YORK, N. Y. 10012
1970

Wouldst thou enjoy a long Life, a healthy Body, and a vigorous Mind, and be acquainted also with the wonderful Works of God, labour in the first place to bring thy Appetite into Subjection to Reason.

35. A PROPOSAL

FOR PROMOTING USEFUL KNOWLEDGE AMONG THE BRITISH PLANTATIONS IN AMERICA[1]

Philadelphia, May 14, 1743.

THE English are possessed of a long tract of continent, from Nova Scotia to Georgia, extending north and south through different climates, having different soils, producing different plants, mines, and minerals, and capable of different improvements, manufactures, &c.

The first drudgery of settling new colonies, which confines the attention of people to mere necessaries, is now pretty well over; and there are many in every province in circumstances that set them at ease, and afford leisure to cultivate the finer arts and improve the common stock of knowledge. To such of these who are men of speculation, many hints must from time to time arise, many observations occur, which if well examined, pursued, and improved, might produce discoveries to the advantage of some or all of the British plantations, or to the benefit of mankind in general.

[1] This paper appears to contain the first suggestion, in any public form, for an *American Philosophical Society*. It was originally printed on a separate sheet, as a circular letter, and sent by the author to his different correspondents. — S.

Reproduced with the permission of Haskell House, © 1970 from *The Writings of Benjamin Franklin* by Albert Henry Smyth, v. II, 1970: 228-232.

But as from the extent of the country such persons are widely separated, and seldom can see and converse or be acquainted with each other, so that many useful particulars remain uncommunicated, die with the discoverers, and are lost to mankind; it is, to remedy this inconvenience for the future, proposed,

That one society be formed of *virtuosi* or ingenious men, residing in the several colonies, to be called *The American Philosophical Society*, who are to maintain a constant correspondence.

That Philadelphia, being the city nearest the centre of the continent colonies, communicating with all of them northward and southward by post, and with all the islands by sea, and having the advantage of a good growing library, be the centre of the Society.

That at Philadelphia there be always at least seven members, viz. a physician, a botanist, a mathematician, a chemist, a mechanician, a geographer, and a general natural philosopher, besides a president, treasurer, and secretary.

That these members meet once a month, or oftener, at their own expense, to communicate to each other their observations and experiments, to receive, read, and consider such letters, communications, or queries as shall be sent from distant members; to direct the dispersing of copies of such communications as are valuable, to other distant members, in order to procure their sentiments thereupon.

That the subjects of the correspondence be: all new-discovered plants, herbs, trees, roots, their virtues, uses, &c.; methods of propagating them, and making such as are useful, but particular to some plantations, more general; improvements of vegetable juices, as ciders, wines, &c.; new methods of curing or preventing diseases; all new-discovered fossils in different countries, as mines, minerals, and quarries; new and useful improvements in any branch of mathematics; new discoveries in chemistry, such as improvements in distillation, brewing, and assaying of ores; new mechanical inventions for saving labour, as mills and carriages, and for raising and conveying of water, draining of meadows, &c.; all new arts, trades, and manufactures, that may be proposed or thought of; surveys, maps, and charts of particular parts of the sea-coasts or inland countries; course and junction of rivers and great roads, situation of lakes and mountains, nature of the soil and productions; new methods of improving the breed of useful animals; introducing other sorts from foreign countries; new improvements in planting, gardening, and clearing land; and all philosophical experiments that let light into the nature of things, tend to increase the power of man over matter, and multiply the conveniences or pleasures of life.

That a correspondence, already begun by some intended members, shall be kept up by this Society with the ROYAL SOCIETY of London, and with the DUBLIN SOCIETY.

That every member shall have abstracts sent him quarterly, of every thing valuable communicated to the Society's Secretary at Philadelphia; free of all charge except the yearly payment hereafter mentioned.

That, by permission of the postmaster-general, such communications pass between the Secretary of the Society and the members, postage-free.

That, for defraying the expense of such experiments as the Society shall judge proper to cause to be made, and other contingent charges for the common good, every member send a piece of eight per annum to the treasurer, at Philadelphia, to form a common stock, to be disbursed by order of the President with the consent of the majority of the members that can conveniently be consulted thereupon, to such persons and places where and by whom the experiments are to be made, and otherwise as there shall be occasion; of which disbursements an exact account shall be kept, and communicated yearly to every member.

That, at the first meetings of the members at Philadelphia, such rules be formed for regulating their meetings and transactions for the general benefit, as shall be convenient and necessary; to be afterwards changed and improved as there shall be occasion, wherein due regard is to be had to the advice of distant members.

That, at the end of every year, collections be made and printed, of such experiments, discoveries, and improvements, as may be thought of public advantage; and that every member have a copy sent him.

That the business and duty of the Secretary be to receive all letters intended for the Society, and lay them before the President and members at their meetings; to abstract, correct, and methodize such papers as require it, and as he shall be directed to do by the President, after they have been considered, debated, and digested in the Society; to enter copies thereof in the Society's books, and make out copies for distant members; to answer their letters by direction of the President, and keep records of all material transactions of the Society.

Benjamin Franklin, the writer of this Proposal, offers himself to serve the Society as their secretary, till they shall be provided with one more capable.[1]

THE WRITINGS OF
George Washington
from the Original Manuscript Sources 1745-1799

Prepared under the direction of the UNITED STATES
GEORGE WASHINGTON BICENTENNIAL COMMISSION
and published by authority of CONGRESS

JOHN C. FITZPATRICK, *Editor*

Volume 30

June 20, 1788-January 21, 1790

United States
Government Printing Office
Washington

FIRST ANNUAL ADDRESS TO CONGRESS[87]

January 8, 1790.

Fellow Citizens of the Senate and House of Representatives: I embrace with great satisfaction the opportunity which now presents itself, of congratulating you on the present favourable prospects of our public affairs. The recent accession of the important State of North Carolina to the Constitution of the United States (of which official information has been received); the rising credit and respectability of our Country; the general and increasing good will towards the Government of the Union, and the concord, peace, and plenty, with which we are blessed are circumstances auspicious in an eminent degree, to our national prosperity.

In resuming your consultations for the general good, you cannot but derive encouragement from the reflection that the measures of the last Session have been as satisfactory to your Constituents, as the novelty and difficulty of the work allowed you to hope. Still further to realize their expectations and to secure the blessings which a Gracious Providence has placed within our reach, will in the course of the present important Session, call for the cool and deliberate exertion of your patriotism, firmness and wisdom.

Among the many interesting objects, which will engage your attention, that of providing for the common defence will merit particular regard. To be prepared for War is one of the most effectual means of perserving peace.

A free people ought not only to be armed but disciplined; to which end a uniform and well digested plan is requisite: And their safety and interest require, that they should promote such manufactories, as tend to render them independent on others for essential, particularly for military supplies.

---

[87] Delivered in the Senate Chamber.

The proper establishment of the Troops, which may be deemed indispensable, will be entitled to mature deliberation.[88] In the arrangements, which may be made respecting it, it will be of importance to conciliate the comfortable support of the Officers and Soldiers with a due regard to œconomy.

There was reason to hope that the pacific measures adopted with regard to certain hostile tribes of Indians would have relieved the Inhabitants of our Southern and Western frontiers from their depredations. But you will perceive from the information contained in the papers which I shall direct to be laid before you (comprehending a communication from the Commonwealth of Virginia) that we ought to be prepared to afford protection to those parts of the Union; and if necessary to punish aggressors.

The interests of the United States requires that our intercourse with other nations should be facilitated, by such provisions as will enable me to fulfill my duty in that respect, in the manner which circumstances may render most conducive to the public good: And to this end that the compensations to be made to the persons who may be employed, should according to the nature of their appointments, be defined by law; and a competent fund designated for defraying the expenses incident to the conduct of our foreign affairs.

Various considerations also render it expedient, that the terms on which foreigners may be admitted to the rights of Citizens should be speedily ascertained by a uniform rule of naturalization.

---

[88] Ford, Sparks, and Richardson print this word as "consideration."

Uniformity in the Currency, Weights and Measures of the United States is an object of great importance, and will I am persuaded be duly attended to.

The advancement of Agriculture, Commerce and Manufactures by all proper means, will not I trust need recommendation. But I cannot forbear intimating to you the expediency of giving effectual encouragement as well to the introduction of new and useful inventions from abroad, as to the exertions of skill and genius in producing them at home; and of facilitating the intercourse between the distant parts of our Country by a due attention to the Post-Office and Post-Roads.

Nor am I less persuaded, that you will agree with me in opinion, that there is nothing which can better deserve your patronage than the promotion of Science and Literature. Knowledge is in every country the surest basis of public happiness. In one in which the measures of Government receive their impression so immediately from the sense of the Community as in ours it is proportionably essential. To the security of a free Constitution it contributes in various ways: By convincing those who are intrusted with the public administration, that every valuable end of Government is best answered by the enlightened confidence of the people: and by teaching the people themselves to know and to value their own rights; to discern and provide against invasions of them; to distinguish between oppression and the necessary exercise of lawful authority; between burthens proceeding from a disregard to their convenience and those resulting from the inevitable exigencies of Society; to discriminate the spirit of Liberty from that of licentiousness, cherishing the first, avoiding the last, and uniting a speedy, but temperate vigilance against encroachments, with an inviolable respect to the Laws.

46

THE WRITINGS

OF

THOMAS JEFFERSON

COLLECTED AND EDITED

BY

PAUL LEICESTER FORD

VOLUME VII

1795-1801

G. P. PUTNAM'S SONS
NEW YORK LONDON
27 WEST TWENTY-THIRD STREET 24 BEDFORD STREET, STRAND
The Knickerbocker Press
1896

TO ELBRIDGE GERRY. J. MSS.

PHILADELPHIA, Jan 26, 1799.

My Dear Sir,—Your favor of Nov. 12 was safely delivered to me by mr. Binney, but not till Dec. 28, as I arrived here only three days before that date. It was received with great satisfaction. Our very long intimacy as fellow-laborers in the same cause, the recent expressions of mutual confidence which had preceded your mission, the interesting course which that had taken, & particularly & personally as it regarded yourself, made me anxious to hear from you on your return. I was the more so too, as I had myself during the whole of your absence, as well as since your return, been a constant butt for every shaft of calumny which malice & falsehood could form, & the presses, public speakers, or private letters disseminate. One of these, too, was of a nature to touch yourself; as if, wanting confidence in your efforts, I had been capable of usurping powers committed to

Reproduced with the permission of G. P. Putnam, © 1896
from *The Writings of Thomas Jefferson*, v. 7, 1896: 325-329.

you, & authorizing negociations private & collateral to yours. The real truth is, that though Dr Logan, the pretended missionary, about 4. or 5. days before he sailed for Hamburgh, told me he was going there, & thence to Paris, & asked & received from me a certificate of his citizenship, character, & circumstances of life, merely as a protection, should he be molested on his journey, in the present turbulent & suspicious state of Europe, yet I had been led to consider his object as relative to his private affairs; and tho', from an intimacy of some standing, he knew well my wishes for peace and my political sentiments in general, he nevertheless received then no particular declaration of them, no authority to communicate them to any mortal, nor to speak to any one in my name, or in anybody's name, on that, or on any other subject whatever; nor did I write by him a scrip of a pen to any person whatever. This he has himself honestly & publicly declared since his return; & from his well-known character & every other circumstance, every candid man must perceive that his enterprise was dictated by his own enthusiasm, without consultation or communication with any one; that he acted in Paris on his own ground, & made his own way. Yet to give some color to his proceedings, which might implicate the republicans in general, & myself particularly, they have not been ashamed to bring forward a suppositious paper, drawn by one of their own party in the name of Logan, and falsely pretended to have been presented by him to the government of France; counting that the bare mention of my name therein, would connect that in the eye of the public with this transaction. In confutation of these and all future calumnies, by way of anticipation, I shall make to you a profession of my political faith; in confidence that you will consider every future imputation on me of a contrary complexion, as bearing on its front the mark of falsehood & calumny.

I do then, with sincere zeal, wish an inviolable preservation of our present federal constitution, according to the true sense in which it was adopted by the States, that in which it was advocated by it's friends, & not that which it's enemies apprehended, who therefore became it's enemies; and I am opposed to the monarchising it's features by the forms of it's administration, with a view to conciliate a first transition to a President & Senate for life, & from that to a hereditary tenure of these offices, & thus to worm out the elective principle. I am for preserving to the States the powers not yielded by them to the Union, & to the legislature of the Union it's constitutional share in the division of powers; and I am not for transferring all the powers of the States to the general government, & all those of that government to the Executive branch. I am for a government rigorously frugal & simple, applying all the possible savings of the public revenue to the discharge of the national debt; and not for a multiplication of officers & salaries merely to make partisans, & for increasing, by every device, the public debt, on the principle of it's being a public blessing. I am for relying, for internal defence, on our militia solely, till actual invasion, and for such a naval force only as may protect our coasts and harbors from such depredations as we have experienced; and not for a standing army in time of peace, which may overawe the public sentiment; nor for a navy, which, by it's own expenses and the eternal wars in which it will implicate us, will grind us with public burthens, & sink us under them. I am for free commerce with all nations; political connection with none; & little or no diplomatic establishment. And I am not for linking ourselves by new treaties with the quarrels of Europe; entering that field of slaughter to preserve their balance, or joining in the confederacy of kings to war against the principles of liberty. I am for freedom of religion, & against all maneuvres to bring about a legal ascendancy of one sect over another: for freedom of the press, & against all violations of the constitution to silence by force & not by reason the complaints or criticisms, just or unjust, of our citizens against the conduct of their agents. And I am for encouraging the progress of science in all it's branches; and not for raising a hue and cry against the sacred name of philosophy; for awing the human mind by stories of raw-head & bloody bones to a distrust of its own vision, & to repose implicitly on that of others; to go backwards instead of forwards to look for improvement; to believe that government, religion, morality, & every other science were in the highest perfection in ages of the darkest ignorance, and that nothing can ever be devised more perfect than what was established by our forefathers.] To these I will add, that I was a sincere well-wisher to the success of the French revolution, and still wish it may end in the establishment of a free & well-ordered republic; but I have not been insensible under the atrocious depredations they have committed on our commerce. The first object of my heart is my own country. In that is embarked my family, my fortune, & my own existence. I have not one farthing of interest, nor one fibre of attachment out of it, nor a single motive of preference of any one nation to another, but in proportion as they are more or less friendly to us. But though deeply feeling the injuries of France, I did not think war the surest means of redressing them. I did believe, that a mission sincerely disposed to preserve peace, would obtain for us a peaceable & honorable settlement & retribution; and I appeal to you to say, whether this might not have been obtained, if either of your colleagues had been of the same sentiment with yourself.

These, my friend, are my principles; they are unquestionably the principles of the great body of our fellow citizens, and I know there is not one of them which is not yours also. In truth, we never differed but on one ground, the funding system; and as, from the moment of it's being adopted by the constituted authorities, I became religiously principled in the sacred discharge of it to the uttermost farthing, we are united now even on that single ground of difference.

52

THE PURSUIT OF KNOWLEDGE IN THE EARLY AMERICAN REPUBLIC

American Scientific and Learned Societies from Colonial Times to the Civil War

EDITED BY ALEXANDRA OLESON
AND SANBORN C. BROWN

The Johns Hopkins University Press
Baltimore and London

Science, Learning, and Utility: Patterns of Organization in the Early American Republic

JOHN C. GREENE

Nothing is more characteristic of modern Western civilization than the organized pursuit of secular knowledge. If the Greeks valued knowledge for its own sake and the Middle Ages knowledge of a different kind for the sake of salvation, the modern world, taking its cue from Bacon and Descartes, values knowledge as power. On the continent of Europe the emerging nation states took charge of the organized pursuit of knowledge, including it as part of a system of centralized, governmentally sponsored academies and schools for cultivating science, literature, and the fine and practical arts. This system reached its apogee in Napoleonic France, where science and technology, learning and literature became at once the glory and the servants of the state.

In England, free enterprise rather than state patronage guided the search for knowledge, drawing on the private resources of lords and gentlemen rather than the largesse of the state. Societies for the promotion of useful knowledge sprang up everywhere, but the preeminence of London as the political, cultural, and economic center of the kingdom, combined with the limited geographical area of the country, provided an element of centralization without which the organized pursuit of knowledge by private enterprise might have remained dispersed and ineffective. Thanks to these circumstances the Royal Society of London, the Geological Society of London, the Linnean Society of London, the Zoological Society of London, and the Royal Astronomical Society were national as well as London societies. True, there were numerous provincial societies, but these acknowledged the hegemony of the London groups. At the same time, the dominant utilitarianism of the Baconian tradition was qualified and softened by an older tradition of patronage of science, learning, and art for their own sake.

In Britain's daughter nation across the Atlantic a different situation prevailed. Here, too, private enterprise took full responsibility for organizing the pursuit of knowledge, but private resources were less substantial than those in England; the gentleman scientist was a relative rarity; political, economic, and cultural power were diffused over a vast area; and the national capital after 1800 was a raw, new "city" inhabited chiefly by diplomats and government officials. With the defeat of the Federalists and the removal of the capital to Washington, D.C., in 1800, it became clear that institutions for the promotion of knowledge would develop on

Reproduced with the permission of Johns Hopkins University Press, © 1976 from *The Pursuit of Knowledge in the Early American Republic*, 1976: 1-19.

a regional or local scale for many years to come. Philadelphia, New York, Albany, Boston, New Haven, Charleston, Cincinnati, Nashville, St. Louis, New Orleans—these would provide organizations for seeking knowledge until such time as the development and professionalization of American science required organization on a national scale.

Of these urban centers, Philadelphia was the most active and fecund in generating institutions for the pursuit of knowledge. The largest city, leading commercial port, and political center of the nascent nation until 1800, Philadelphia had established its preeminence as a medical center through the Pennsylvania Hospital and the School of Medicine of the University of Pennsylvania, and had led the way in organizing for the pursuit of knowledge with the formation of the American Philosophical Society, distinguished by the names and researches of Franklin, Rittenhouse, Jefferson, the medical professors at the University, and such notable foreign scientists and savants as Joseph Correa da Serra, William Maclure, Joseph Priestley, and André Michaux and his son, François André. The Chemical Society of Philadelphia, the Philadelphia Society for Promoting Agriculture, Peale's Museum, and the botanical garden created by John Bartram, followed in later years by the Academy of Natural Sciences and the Franklin Institute, gave further evidence of the city's capacity to create institutions for improving knowledge.

These institutions were mutually reinforcing, and were mutually sustained by a community of college professors, medical men, scientifically inclined gentlemen and artisans, and visiting foreign scientists that composed their overlapping memberships. Members of the Chemical Society of Philadelphia—James Woodhouse, Adam Seybert, and Robert Hare, for example—were active in the American Philosophical Society, some of whose members, such as John Vaughan, Benjamin Rush, Benjamin S. Barton, Zacchaeus Collins, Caspar Wistar, and James Mease, were involved in the Philadelphia Society for Promoting Agriculture or contributed articles to its *Memoirs*. The overlap in membership between the American Philosophical Society and the Academy of Natural Sciences was substantial. Most of the leading members of the Academy—Maclure, Troost, Say, Harlan, Morton, Wetherill, Ord, Lesueur, Bonaparte, Nuttall—were also members of the Society. In the physical sciences there was an important overlap between the members of the American Philosophical Society and those of the Franklin Institute. Isaiah Lukens, William Keating, Robert M. Patterson, John P. Wetherill, and William Strickland—all members of the American Philosophical Society—were among the officers of the Franklin Institute when it was founded in 1825, and other leading members of the Society were active in the Institute and contributed to its *Journal*. In the field of the earth sciences there was considerable overlap between the membership of the Institute and that of the Academy of Natural Sciences.

Partly because of their overlapping memberships, these various institutions for promoting knowledge were mutually sustaining rather than competitive. The oldest and most prestigious of them, the American Philosophical Society, had begun as a society for promoting useful knowledge, with a heavy emphasis on practical utility. Franklin and his associates had set the tone in dedicating the Society to the cultivation of "all philosophical experiments that let light into the nature of things, tend to increase the power of man over matter, and multiply the conveniences and pleasures of life."[1] When the first volume of the Society's *Transactions* appeared nearly thirty years later, Baconian utilitarianism was still the dominant theme. "Knowledge," the preface to this volume declared, "is of little use, when confined to mere speculation. But when speculative truths are reduced to practice, when theories, grounded upon experiments, are applied to the common purposes of life; and when, by these, agriculture is improved, trade enlarged, the arts of living made more easy and comfortable, and, of course, the increase and happiness of mankind promoted; knowledge then becomes really useful."[2] To this end the members of the Society proposed to devote their energies chiefly to "such subjects as tend to the improvement of their country, and advancement of its interest and prosperity." Among these were listed improvements in agriculture; the preservation of timber for shipbuilding and other purposes; the improvement of materia medica; the discovery and introduction of "new vegetable juices"; improvements in the manufacture of wine, cider, and vinegar; and methods of constructing highways, causeways, and bridges and of linking rivers and improving inland navigation. Physics, mechanics, astronomy, and mathematics were also mentioned as forms of useful knowledge.

For the implementation of these ambitious plans for human improvement through the pursuit of knowledge, the Society looked primarily to "gentlemen . . . whom Providence hath blessed with affluence, and whose understanding is improved by a liberal education" but also to practical farmers and to "ingenious mechanics," whose knowledge might be communicated to the Society if stimulated by the award of premiums for important discoveries and inventions. Mention was also made of the desirability of forming a cabinet of "fossil," vegetable, and animal substances "that may enlarge the bounds of natural history in general, and of this part of the world in particular." There was, however, no reference to the fine arts or to history, philosophy in the modern sense, or literature. Of the six committees established by the Society to referee communications, the first was responsible for geography, mathematics, natural philosophy, and astronomy; the second, for medicine and anatomy; the third, for natural history and chemistry; the fourth, for trade and commerce; the fifth, for mechanics and architecture; and the sixth, for husbandry and American improvements.

But although practical utility was the central theme in the founding of the American Philosophical Society, it failed to dominate the activities and publications of the Society thereafter. Interest in agricultural improvement lapsed quickly after the publication of the first volume of the *Transactions*, to be taken up by the Philadelphia Society for Promoting Agriculture. Articles devoted to mechanical improvements–a "self-moving or sentinel register," an improved ship pump, a horizontal windmill, an improved method of quilling harpsichords, improvements in water mills and gristmills, Jefferson's moldboard, suggestions for improving steam engines, and the like–continued to appear until the 1820s, after which they were few and far between, probably because of the founding of the Franklin Institute and its *Journal.*

---

[1] Notes to this chapter begin on page 19.

Meanwhile, the launching of the Academy of Natural Sciences of Philadelphia (1812) and its *Journal* (1817) had challenged the Society's leadership in the field of natural history. Fortunately for the Society, the naturalists of Philadelphia continued to contribute to the Society's *Transactions*, even though the Academy and its museum now became the focus of research and publication in natural history. In the 1850s the Society recognized the Academy's leadership in this field by transferring its collections of fossils, accumulated since the days of Franklin, Jefferson, and Wistar, to the custody of the Academy.[3]

In 1815 the American Philosophical Society had taken an important step toward expanding its conception of the realm of "useful knowledge" by establishing a Committee of History, Moral Science, and General Literature under the chairmanship of the Philadelphia lawyer and linguist Peter S. Duponceau.[4] To the disappointment of those members who viewed this innovation as a move in the direction of transforming the Society into a "literary and philosophical" society (some of these members went on to organize the Historical Society of Pennsylvania), Duponceau's interests were confined almost exclusively to comparative linguistics, especially the study of Indian languages. The chief result of the committee's work, aside from its occasional publications, seems to have been the addition of comparative linguistics to the list of topics represented in the *Transactions* of the Society. In his annual address to the Society in December 1854, President Franklin Bache reported that the 261 members of the Society resident in Philadelphia included 158 scientific men and "artists," 83 literary men, and 20 naturalists, adding that the literary members, "comprising, for the most part, gentlemen of the legal and clerical professions," had made very few communications to the Society, "as if they considered the object of the Society to be exclusively the cultivation of science."[5]

The literary members' view of the Society was not without foundation. Judging from the contents of its *Transactions* and *Proceedings*, the American Philosophical Society had become a general scientific society, dividing its attention more or less equally among the physical sciences, the earth sciences, and the natural history of plants and animals. In the physical sciences, observational science was represented by astronomy, geodesy, meteorology, and terrestrial magnetism. On the experimental side, chemistry and electricity and magnetism were ably represented by Robert Hare and Joseph Henry respectively, with subsidiary contributions by R. A. Tilghman, James C. Booth, and others in the chemical field. Because of its central location and its early lead in astronomy and related geophysical sciences such as geodesy, meteorology, and terrestrial magnetism, the Society was able to attract contributions on a national scale–from

Lt. James Gillis, William Lambert, and Ferdinand Hassler in Washington, D.C., Elias Loomis at the Hudson College Observatory in Ohio, Dr. John Locke in Cincinnati, C. G. Forshey in Natchez, William H. C. Bartlett at West Point, Robert Treat Paine and Benjamin Peirce in Boston, William C. Redfield and Robert Adrain in New York, and J. N. Nicollet and other explorers. The presence in or near Philadelphia of James Espy, Robert Hare, William C. Redfield, and Joseph Henry—all interested in meteorology—made Philadelphia a center for research of this kind in the period 1830-1850. The interest of Alexander Dallas Bache and Loomis in terrestrial magnetism and Bache's magnetic researches at the Girard College Observatory had a similar effect in this field of inquiry. At the same time the research of Joseph Henry, Robert Hare, and various workers at the Franklin Institute such as Walter R. Johnson, designer of a machine for testing the strength of metals, made Philadelphia the leading center of experimental science in the United States.

But the preeminence of Philadelphia and of the American Philosophical Society in these branches of science in the 1830s and early 1840s was not destined to last. During the 1840s, Hare resigned his chair at the School of Medicine of the University of Pennsylvania, and Henry, Bache, Espy, and Sears Walker moved to Washington, D.C., to become employees of the federal government. Meanwhile other avenues of publication for the research of these and other physical scientists were opening up: Silliman's *American Journal of Science,* the *Proceedings* of the American Association for the Advancement of Science, Gould's *Astronomical Journal,* and the publications of the Smithsonian Institution, the United States Naval Observatory, and the Coast Survey, for example. The federal government was beginning to emerge as a significant patron, organizer, and publisher of science, and Philadelphia was no longer the seat of that government.

For its part, the American Philosophical Society was in no position to make a bid for national leadership in science at this juncture. The "Account of the American Philosophical Society," written in 1841 by the aged treasurer and librarian, John Vaughan, in response to inquiries from the Prussian Minister in Washington, did not give the impression of a flourishing society spurred on by vigorous leadership. Vaughan himself was eighty-six and would die before the end of the year. The president of the Society since 1828, Peter S. Duponceau, was also in his eighties and in very poor health. The financial condition of the Society was not encouraging.

> Our annual income is about 1800 dollars [Vaughan wrote], arising from the rents of some property & the contributions of the resident members of 5 Dollars a year. . . . It may appear a matter of surprise that our Society has published so little, but it must be observed that having no assistance from Government, our pecuniary means were small & our exertions paralised for want of funds. . . . It has only been by a rigid economy that we have attained our present situation, which enables us to be more regular in our publications, which will appear from the dates of our more recent ones.[6]

The Society's situation seems to have deteriorated rather than improved during the remaining years before the Civil War. In the mid-1840s an unfortunate financial reverse suffered in connection with the Society's efforts to buy the Philadelphia Museum imperiled the existence of the Society and forced it to appeal to the Philadelphia community for assistance. The interval between successive issues of its *Transactions*, which had been narrowed to two or three years in the late 1830s and early 1840s, expanded to seven years between 1846 and 1860. With the retirement of Hare and the departure of Henry, Bache, Espy, and Walker for Washington, the physical science component of the resident membership was enfeebled, and more reliance had to be placed on the natural historians–especially Joseph Leidy, Isaac Lea, and John P. Wetherill–for contributions to the *Transactions*. In 1859, aware that its position was deteriorating, the Society appointed a special committee to study "the present condition of the Society and the means of increasing its usefulness." In its report, submitted May 6, 1859, the committee described the circumstances that had made the Society "the centre to which more appropriately than towards any other should come the problems which control special investigations." But the Society's preeminence could no longer be taken for granted, the committee warned:

> . . . it is well known to the Society that during some years past neither its Transactions nor its Proceedings have equalled the expectations which might justly be entertained of its fruitfulness. In number and in variety of subjects the contributions of its members and others have not corresponded with its claims and means. The attendance of its members has been greatly below the standard consistent with its proper effectiveness, and a constant diminution of general interest in the meetings has been the natural consequence.[7]

The Society's library was in a condition that precluded satisfactory use of it by the members, but the librarian had made progress in analyzing and rearranging the collection, and the expected removal of the Society to new quarters would provide the space needed for proper arrangement of the books, the committee reported. The chief matter of concern was the dearth of scientific and literary communications to the Society by its members. To correct this situation the committee recommended the establishment of a standing Committee on Premiums, charged with advertising and administering the existing premiums of the Society and with recommending new premiums "directly chosen in reference to the actual state of science and the comparative value of particular problems." By these measures and by renewed appeals to the general membership, it was hoped that the Society could be reinvigorated and restored to its original position of leadership in American scientific circles.

Meanwhile the Academy of Natural Sciences and the Franklin Institute had sloughed off their early spirit of Baconian utilitarianism and had emerged as flourishing institutions dedicated to the discovery and dissemination of knowledge through the labors of professional naturalists, physical scientists, and engineers. Organized in 1824 for "the promotion and encouragement of manufactures and the mechanic and useful arts by the establishment of popular lectures on the sciences connected with them, by the formation of a cabinet of models and minerals and a library, by offering premiums on all objects deemed worthy of encouragement, by examining all new inventions submitted to them, and by such other measures as they may judge expedient," the Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts symbolized its dedication to useful knowledge by adopting the name of Franklin. Indeed, Franklin would have been pleased to belong to an institution of "manufacturers, mechanics, artizans and persons friendly to the mechanic arts" whose membership list included a variety of merchants, manufacturers, engravers, jewellers, carpenters, hatters, dentists, metalworkers, druggists, lawyers, coachmakers, tailors, accountants, and instrument makers, as well as some of Philadelphia's leading physical scientists.[8]

In no other institution in Philadelphia or elsewhere in the nation was there as strong a working interaction between scientists, technologists, educators, and men of business and industry. Within a few years the Institute had erected a building; sponsored a series of public lectures; created professorships of natural philosophy, mineralogy and chemistry, architecture, and mechanics; established a high school with instruction in geography, history, Latin, Greek, French, Spanish, and German as well as theoretical and practical sciences; begun a collection of minerals and mechanical models; built up a library; launched a journal; and inaugurated a series of industrial exhibits that were to continue annually or biennially throughout the period to 1860. On its own initiative, or at the request of the federal, state, or city government, it undertook investigations of water as a source of power, of the causes and prevention of steam boiler explosions, of weights and measures, and of methods of paving highways. The Institute also published monthly weather reports based on the research of its Committee on Meteorology. It was in the lecture hall of the Franklin Institute, not at Philosophical Hall, that the Association of American Geologists was organized in 1840, seven of the eighteen founders being members of the Institute. During the 1850s the Institute experienced difficulty in raising funds for its activities, but in the following decade more than $30,000 was raised by the sale of stock in the Institute to industrial concerns in the Philadelphia area.[9]

In the *Journal* of the Institute the growing interaction of technology with science was reflected throughout the period up to 1860. In its early years the *Journal* eschewed abstract science, devoting its pages almost entirely to technological articles that showed little influence of science, with occasional digressions into natural history from the pens of John Godman, Alexander Wilson, and John James Audubon. In the field of metallurgy, the contributions of the early period concerned such processes as gilding and silvering, methods of producing alloys, and the manufacture of wire. By midcentury, however, the study of metals had become important in relation to railroad and locomotive construction, steam boilers, ship hulls, pumps, canals, and the like; a long series of articles in the *Journal* discussed the virtues of Jacob Perkins's high-pressure steam engine. By this time the main contributors to the *Journal* were no longer artisans, amateur inventors, clergymen and physicians interested in and anonymous contributors, but college professors of science, engineers, and other professionals, most of whom were careful to display their credentials, academic or otherwise, in connection with their reports. These contributions–on steam engines, bridges, the strength of materials, ships, railroads, metallurgy, engraving, and photography–were increasingly of a kind that could properly be described as applied science rather than practical technology, as indicated by the use of mathematical, mechanical, chemical, and geological principles in the discussion of practical problems.

In like manner, the Academy of Natural Sciences grew in membership, resources, and the spirit of professionalism as the century progressed. Founded, like most other Philadelphia institutions for promoting knowledge, in a spirit of Baconian collection and dissemination of useful information, the Academy was preserved in that spirit for many years by its president and benefactor, William Maclure, a devout Baconian empiricist in science and an ardent champion of scientific and technical education for the common man. After Maclure's death in 1840, however, the Academy became increasingly a place for scientific research and publication under the leadership of Samuel G. Morton, John Cassin, Joseph Leidy, William S. Vaux, Timothy Conrad, Samuel S. Haldeman, and others.

Although the Academy never regained the leadership in botany it had enjoyed earlier in the century through the work of Thomas Nuttall, C. S. Rafinesque, and others, it continued to be an important center of zoological and paleontological research. In 1846 Dr. Thomas B. Wilson loaned the Academy the collection of ten thousand birds he had purchased from the Duc de Rivoli, and paid for a sizeable extension of the Academy's building to house the collection. He also helped subsidize a second series of the Academy's *Journal*. In 1860 his entire collection of birds, comprising twenty-six thousand mounted specimens and two thousand skins, was donated to the Academy.

Meanwhile, the building had been enlarged again, and a Biological Department, headed by Dr. Joseph Leidy, had been added. And just as the publications of the American Philosophical Society had served as an outlet for a broad range of studies in astronomy and related sciences in the 1830s and 1840s, so the *Proceedings* and *Journal* of the Academy of Natural Sciences became an important avenue for publication in natural history during the 1850s and subsequent decades. As Edward Nolan, historian of the Academy, has written:

> The Academy entered on its second half century under the brightest prospects and with a most gratifying record of honorable achievements. The year 1862 was made notable by the work of illustrious veterans who were still active, and by what there was reason to expect from their successors. But few of the great collections which have since come into prominence were then in existence. The Smithsonian Institution was then rather a distributing agency than a storehouse of scientific material. The United States Government had not become, through the Agricultural Department, the National Museum, the Fish Commission and the Geological Surveys, one of the largest publishing concerns in the world, and a formidable rival in the publication of scientific matter, so that the work of Gill, Meek, Hayden, Coues, Stimson, Kennicott, Yarrow and others in Washing-

ton and elsewhere, was issued promptly and accurately in the pages of the *Proceedings* and *Journal*.[10]

In the same publications appeared the research of Joseph Leidy, John Cassin, Isaac Lea, George W. Tryon, Jr., James Meigs, John L. Leconte, and other members of the Academy. The Academy, like the Franklin Institute, ended the ante-bellum period on a strong note.

Throughout the first half of the nineteenth century, the Philadelphia societies for promoting knowledge, although they failed to attain the national prestige and hegemony enjoyed by their counterparts in London, maintained a steady level of intellectual achievement and mutual cooperation, educated the Philadelphia public, kept up contacts with similar institutions abroad and in the United States, served in some degree the needs of the city, the state, and the national government, and provided avenues of publication for a scientific community extending well beyond the limits of Pennsylvania. If the Philadelphia institutions were not national in scope and prestige, at the very least they established the models on which similar institutions in other regions of the growing Republic were formed.

Among the first cities to emulate the example of Philadelphia in organizing societies for promoting knowledge was Boston. Outstripped by Philadelphia in population, wealth, and trade, and lacking the Quaker City's central location on the Atlantic seaboard, Boston nevertheless had pretensions to intellectual eminence based on its Puritan past and institutionalized in "the University at Cambridge," as Harvard College was often called in the early nineteenth century. Not to be outdone by Philadelphia, Boston organized its literati into the American Academy of Arts and Sciences in 1780, swelling the roster of its members with the names of its most illustrious citizens.

According to John Adams, the American Academy was modeled more on the Académie Royale des Sciences in Paris than on the American Philosophical Society, but the absence of government patronage and the strong overtones of Baconian utilitarianism in the preface to the first volume of its *Memoirs* (1785) belied the claim. The preface began by reciting the rise and contributions of societies for promoting useful knowledge in Europe, pointing in particular to the Royal Society of London, the Académie des Sciences in Paris, and the Society Instituted for the Encouragement of Arts, Manufactures, and Commerce in London. Through the efforts of these and similar societies, the preface declared, "knowledge of various kinds, and greatly useful to mankind, has taken the place of dry and uninteresting speculations of schoolmen; and bold and erroneous hypothesis has been obliged to give way to demonstrative experiment."[11]

The American Academy, then, was to be another society for promoting useful knowledge. Its aim was

> to promote and encourage the knowledge of the antiquities of America, and of the natural history of the country, and to determine the uses to which the various natural productions of the country may be applied; to promote and

> encourage medical discoveries, mathematical disquisitions, philosophical enquiries and experiments; astronomical, meteorological and geographical observations; and improvements in agriculture, arts, manufactures and commerce; and in fine, to cultivate every art and science, which may tend to advance the interest, honor, dignity and happiness of a free, independent and virtuous people.[12]

The astronomical papers in the first volume of *Memoirs*, the preface explained, were "chiefly of the practical kind . . . such observations and deductions, as are subservient to the cause of geography and navigation, the improvement of which is of great importance to this country." The articles on natural history and "antiquities," too, were justified in terms of their value, actual or potential, in practical affairs. The medical papers, though containing little that was new, would serve to disseminate useful discoveries recently made in Europe. Looking to the future, the founders of the Academy recommended attention to improvements in agriculture; researches in natural history, especially those useful to medicine; astronomical observations, "particularly those . . . which will serve to perfect the geography of the country, and improve navigation"; mechanical arts, manufactures, and commerce, which would "enrich and aggrandize these confederated States"; and, in general, "useful experiments and improvements, whereby the interest and happiness of the rising empire may be essentially advanced."

Further light on the character and objectives of the American Academy can be derived from the "Philosophical Discourse" delivered before the Academy by its president, James Bowdoin, upon his induction into office on November 8, 1780. After describing the extensive field of investigation opened up to the "sons of literature" by the Academy, Bowdoin devoted himself chiefly to the subjects of civil and natural history, stressing the value of a thorough knowledge of the natural history of one's country for the development of its agriculture, commerce, and manufactures. With a prescient eye, he stressed the central role that Harvard College would play in the activities of the Academy. Most of the Academy's members, he noted, were sons of Harvard. So, too, would most of its future members be Harvard men. Then, somewhat less presciently, he assumed the mantle of the Academy's historian one hundred years hence. Looking backward from that vantage point, he recorded that the Academy's members, true to the philosophy of Bacon and Newton, had

> laid it down as a fundamental principle, that as true physics must be founded on experiments, so all their enquiries should, as far as possible, be carried on, and directed by them. . . . This has been the uniform practice of the society: whose members, from time to time, having been chosen from the men of every country, from every class and profession, without any other distinction than was dictated by the dignity of their characters, by their morality, good sense, and professional abilities, we find in the printed transactions of the society, the best compositions on every subject, within the line of their department. We find in those transactions new facts, new observations and discoveries; or old ones placed in a new light, and new deductions made from them.

> They have particularly attended to such subjects as respected the growth, population, and improvement of their country: in which they have so happily succeeded, that we now see agriculture, manufactures, navigation and commerce, in a high degree of cultivation; and all of them making swift advances in improvement, as population increases. In short, they have, agreeably to the declared end of their institution, "cultivated every art and science, which might tend to advance the interest and honour of their country, the dignity and happiness of a free, independent, and virtuous people."
>
> This is demonstrably evident from the numerous volumes the society have published of their transactions. These volumes are a noble collection of useful knowledge; and considered together in their miscellaneous state, strike the mind with a splendour, resembling the galaxy in the heavens, derived from the combined light of countless myriads of constellations: and like that too, when the several corresponding parts are viewed in their proper connections, they appear to be parts of a whole; and to constitute the most useful systems: systems distinguished by their beauty, regularity, and proportion.[13]

Alas for prophecy! In the next eighty years the Academy published only eleven volumes of *Memoirs* containing but 206 contributions to knowledge. Of these only two or three were based on experiments. As to practical improvements in agriculture, navigation, commerce, manufactures, and the like, only the first two volumes contained a significant number of articles of this sort. The improvement of agriculture was taken up by the Massachusetts Society for Promoting Agriculture, which was also responsible for the founding of a professorship of natural history and a botanical garden at Harvard College. Medical articles soon disappeared from the Academy's *Memoirs*, probably because of the founding in 1812 of the *New England Journal of Medicine and Surgery*. By 1821, when the fourth volume of the *Memoirs* appeared, articles of immediate practical utility had disappeared from the publication, their place being taken by contributions on linguistics and classical literature.

Thereafter, "useful knowledge" was represented by two contributions from the Rumford Professor and Lecturer on the Application of Science to the Useful Arts (one on cannon construction, the other on a machine for spinning hemp and flax); a description of a new telescope stand by the Reverend John Prince; an account of the inner harbor of Boston "with a Synopsis of the General Principles to be observed in the Improvements of Harbors"; some determinations of latitude and longitude by Robert Treat Paine; and an article "On the Practice of Circummeridian Altitudes at Sea or on Shore" by Captain W. F. W. Owen of the Royal Navy. Technology, manufactures, navigation, and commerce—though not agriculture—were indeed flourishing by midcentury in New England, but not as a result of the research and publications of the American Academy of Arts and Sciences.

The Academy's gradual transformation from a society for promoting "useful knowledge" to a literary and philosophical, or learned, society became apparent in 1851, when the Academy decided to distribute its 280 members (of whom

130 were residents of Massachusetts, 80 lived elsewhere in the United States, and 70 were Foreign Honorary Members) into three classes, each with four subdivisions.[14] The first class, Mathematical and Physical Sciences, included mathematics, practical astronomy and geology, physics and chemistry, and technology and engineering. The second, Natural and Physiological Sciences, embraced geology, mineralogy, and physics of the globe; botany and vegetable physiology; zoology and animal physiology; and medicine and surgery. In the third class, Moral and Political Sciences, were moral and intellectual philosophy; philology and ethnology; politics, political economy, and jurisprudence; and aesthetics. In practice, the publications of the Academy, like those of the American Philosophical Society, were dominated by astronomy (and related geophysical sciences) and natural history, with occasional essays on linguistics. There was less experimental and theoretical science in the Academy's *Memoirs* than in the American Philosophical Society's *Transactions*, but the Academy was more successful in securing contributions from its "literary" members.

Whereas the American Philosophical Society reached its highest eminence in its early period and struggled manfully to maintain that eminence in subsequent years, the American Academy increased gradually in stature as Harvard University developed into an important center of science and learning through increasing financial support from the burgeoning Boston community. Whatever the claims of Boston-Cambridge to intellectual distinction in 1800, they were not based primarily on scientific achievement. Science was at a low ebb at Harvard in the first four decades of the nineteenth century. There was no worthy successor to John Winthrop IV in astronomy until Benjamin Peirce and William C. Bond were brought to the College–Peirce in 1831 and Bond in 1839. However, President Joseph Willard did maintain the Winthropian tradition in the Academy to the end of the eighteenth century, by which time Nathaniel Bowditch had begun to emerge as New England's leading astronomer and the Academy's main ornament for more than thirty years. In natural history nothing much was accomplished until the founding of the Boston Society of Natural History in 1830 and the coming of Asa Gray, Louis Agassiz, and Jeffries Wyman to Harvard in the 1840s.

In vain did Benjamin Vaughan seek to persuade the Academy to expand its horizons, enlarge its membership, and become a regional, if not a national, institution. Writing to Nathaniel Bowditch in 1817, Vaughan lamented that Massachusetts, which, in terms of the number of well educated persons residing there, ought to be the "headquarters of science" in the United States, had done little to establish intellectual leadership apart from Harvard College.

> Its American Academy is asleep perhaps in the sleep of death; and while it sleeps, other petty rivals take (as it were) from between its teeth, the materials which ought to go to its support. Instead of being the organ of New England's learning, its own journals and its own petty societies carry away what ought to belong to it: and Connecticut has instituted a rival Society. So far from our dividing the force of *Massachusetts*, it ought to combine the whole of *New*

> ***England*** in favor of knowledge. Massachusetts will not be allowed to be the *representative* of New England; but it may be the chief mover in a *central society*; other states sending representatives, with authority to select pieces for publication belonging to the whole, and philosophical and literary societies of the other states, and (in default of these) their colleges may provide the representatives . . . remembering that no separate *society* and no separate publication in the joint state [of Massachusetts and Maine] can singly have magnitude or interest enough to attract general notice in the U. S. and much less in Europe. The *whole* of our philosophers and literary men *united* can only do it by making exertions in unison with each other. The American Academy by the suspension or slow rate of its publications, and by its want of zeal in soliciting pieces, must receive the chief share of censure for what has occurred on this occasion. An annual publication would satisfy the impatience of authors and the public; and thus prevent many contributions from being diverted from this channel, and seeking the public notice through other publications. But where would be the difficulty of the Linnean Societies [Society's] having a department in this publication allotted to it? The Agricultural Society also, when it has a piece which conveys to the world natural knowledge, and the Connecticut Society may be provided with similar opportunities. The printing might be so contrived, that each Society might have copies to bind up separately, and make volumes of its own, when its papers admitted of it. . . . For myself I perceive no real objection to collecting the published pieces of New England on the subjects in question, yearly; and annexing them (when consent is obtained) to the Massachusetts Volume: but if that be thought degrading let pieces which can not otherwise be obtained, be first presented to the Academy, then published elsewhere with the *leave* of the Academy (affixed to the head of it), and be resumed by the Academy at the end of the year, with the corrections of the author and the remarks of others. The Academy would maintain an easy ascendancy on this occasion; for, if its own contributions did not exceed *all* of the others, they would at least exceed those of *any* of the others; and it would besides have the merit of giving to the whole a currency which others could not expect by their separate efforts. As matters stand arranged, little can be expected from the dislocated action of each state, in consequence of the favors of each branch of knowledge acting separately.[15]

Yet such cooperation was not to be, despite the fact that the Massachusetts Society for Promoting Agriculture, the Linnaean Society of New England, and the American Academy of Arts and Sciences had overlapping memberships and shared an interest in natural history. Perhaps because it enjoyed state support, the Massachusetts Society for Promoting Agriculture felt obliged to apologize for the inclusion of a considerable number of scientific articles in their *Massachusetts Agricultural Repository and Journal* (1801-1832).

> . . . We are reproached for introducing articles which are above the capacity of common farmers [the editors wrote]. If it be intended as an intimation that we devote too large a proportion of this work to philosophical agriculture, we deny the fact. We have always given the preference to the home bred practical essays and experiments. But we are not ready to admit the fact that the introduction of

rational and scientific speculations, such as those of Kirwan and Davy is inexpedient. Massachusetts has scarcely a town which does not furnish educated men. Knowledge must be first communicated to them and from them it will inevitably reach their less informed neighbors.[16]

The Linnaean Society of New England, organized in 1814, showed no disposition to collaborate with either the agricultural society or the American Academy, and soon went under from inability to pay for the housing and care of its rapidly growing collections. Meanwhile, the Connecticut Academy of Arts and Sciences lost much of its reason for existence when the success of Benjamin Silliman's *American Journal of Science* provided an outlet for publishing scientific contributions of the kind that had formerly been published in the Academy's *Memoirs.*[17] New Haven, not Boston, was to become the site of the most important American scientific journal in ante-bellum America, but this journal was to be a family enterprise, not the organ of a society for promoting knowledge. The Connecticut Academy of Arts and Sciences lingered on—indeed, it is still in existence—but it failed to become the instrument for promoting the arts and sciences envisioned by Ezra Stiles, Timothy Dwight, and Noah Webster.

In Boston events took a different turn. Although the American Academy continued to rely heavily on Harvard professors and graduates for membership and for scientific and scholarly contributions, the rapid growth of Boston as a commercial, industrial, and financial center, combined with a longstanding tradition of philanthropic donations in support of the "University at Cambridge," created conditions favorable to the development of scientific and cultural institutions. Between 1830 and 1860 a series of bequests to Harvard College provided for the establishment of professorships in astronomy, botany and natural history, anatomy, and mathematics and natural philosophy, as well as the Lawrence Scientific School, the Museum of Comparative Zoology, and a first-rate observatory with a powerful telescope. In the same decades in which Philadelphia was losing some of its leading physical scientists to the federal government, Harvard acquired the services of Benjamin Peirce and William and George Bond. Moreover, the operations of the United States Coast Survey and the United States *Nautical Almanac* in the Boston area began providing employment for other astronomers and natural philosophers, such as Lt. Charles W. Davis, Simon Newcomb, John D. Runkle, William and Maria Mitchell, William Ferrel, and Joseph Winlock. In 1849 Benjamin Apthorp Gould began publishing his *Astronomical Journal* in Cambridge. Runkle's short-lived *Mathematical Monthly* began publication in 1858.

In the field of natural history, Harvard acquired Asa Gray, Jeffries Wyman, and Louis Agassiz. Meanwhile the Boston Society of Natural History, aided by benefactions from Jonathan Phillips, provided a meeting place for the devotees of natural history. About the same time the talented and energetic Rogers brothers, William and Henry, moved to Boston, there to promote the founding of an institute of technology and to contribute substantially to the scientific discussions at the American Academy and the Boston Society of Natural

History. In Boston, as in Philadelphia, the natural historians divided their contributions between the publications of the younger natural history society and those of the older and more general scientific society. Naturalists like Asa Gray, William Sullivant, and David Humphrey Storer contributed to the *Memoirs* of the American Academy some of the botanical and zoological studies that might more naturally have found publication in the *Boston Journal of Natural History*. These contributions, added to occasional ones from outside the Boston area–from S. S. Haldeman and Joseph Leidy in Philadelphia, John L. LeConte in New York and later Philadelphia, Edward Hitchcock in Amherst, and James Hall and Fielding B. Meek in Albany–gave the Academy a respectable showing in natural history. Indeed, its *Proceedings* for the 1850s, in contrast to the volumes of the 1840s, showed more activity in natural history than in natural philosophy, perhaps because the Boston-Cambridge astronomers had found a new outlet for their contributions in Gould's *Astronomical Journal*, Runkle's *Mathematical Monthly*, and the *Proceedings* of the American Association for the Advancement of Science.

Besides astronomy and natural history, the chief topics that recurred in the *Memoirs* of the Academy from time to time were Indian languages, the calculation of life expectancy, Indian mounds, and thermometric, barometric, magnetic, and other geophysical records. Humanistic research was represented by "A Glossary of Later and Byzantine Greek," a long article entitled "The Age of Petronius Arbiter," Jacob Bigelow's essay "On the Death of Pliny the Elder," and John Pickering's linguistic research.

In Boston, as in Philadelphia, the natural historians, drawn chiefly though not exclusively from the ranks of medical men, formed a major component of the scientific community. Like the Academy of Natural Sciences of Philadelphia, the Boston Society of Natural History led a precarious financial existence for many years, struggling to house and care for its rapidly augmenting collections until private benefactions enabled it to function more efficiently. A turning point came in 1860, when Jonathan Phillips, a member who had donated $2,000 to pay the debts of the Society a decade earlier, bequeathed an additional $10,000 to the Society. In the following year came the first of a series of large donations to the Society by Dr. William J. Walker. About the same time the state legislature granted a square of land for the use of the Society and the projected Massachusetts Institute of Technology. By the spring of 1864 the Society was established in a handsome new building, the Museum of the Boston Society of Natural History, dedicated and opened to the public on May 27, 1864.[18]

Like its sister institution in Philadelphia, the Boston Society of Natural History attracted contributions from far outside the Boston area–from John Bachman and Lewis R. Gibbes in South Carolina; from Moses Ashley Curtis in Wilmington, North Carolina; from N. M. Hentz in Alabama; from Samuel G. Morton, Joseph Leidy, and S. S. Haldeman in Philadelphia; from Jacob W. Bailey at West Point, James Hall in Albany, and John LeConte in New York; and from Josiah D. Whitney in the Lake Superior region. However, it never became an outlet for publishing the collections of government expeditions. Locally it drew heavily on the Boston medical community, made up of Harvard medical professors and their students, but as Harvard College emerged as a major center of research in natural history through the labors of Gray, Agassiz, Jeffries Wyman, and others, the College began to overshadow the medical school as a center for research in natural history.

In the short run these developments in the College greatly strengthened the American Academy and the Boston Society of Natural History. In the long run, however, Harvard University was to dwarf both of these institutions, rendering them peripheral to its ever-expanding intellectual empire. With the Museum of Comparative Zoology, the Geological Museum, the Gray Herbarium, the Arnold Arboretum, and the Peabody Museum of American Archaeology and Ethnology all flourishing at Harvard University in the twentieth century, what need was there for the Museum of the Boston Society of Natural History? James Bowdoin had spoken truly when he described the American Academy as the offspring of Harvard College. So, too, was the Boston Society of Natural History. But the parent institution was to prove too vigorous and powerful for its offspring. The universities, not the various academies and societies, were to be the chief agencies for promoting knowledge in the United States after the Civil War.

In New York, where population, commerce, industry, wealth, and geographic location might have led one to expect the development of an array of institutions for promoting knowledge rivaling or surpassing the achievements of Philadelphia, nothing of the kind occurred. Reflecting on this anomaly, De Witt Clinton offered in explanation the practical motives of the original settlers in New York; the debilitating effects of a royal colonial government that tended to alienate the governing classes from the governed; the cultural and linguistic diversity of the population, leading to political and religious factionalism; the absence of provisions for elementary education; the low condition of the professions of law, medicine, and divinity; the disastrous effects of the Revolution; the schisms within the medical community; and the dominance of the commercial spirit.[19] Doubtless some, if not all, of these factors were influential, but an even more important circumstance seems to have been the removal of the state capital to Albany, thus dividing the scientific resources of the area between two poles. Since the meetings of the Society for the Promotion of Agriculture, Arts, and Manufactures (later the Society for the Promotion of Useful Arts) were tied to the meetings of the legislature, the removal of the capital to Albany shifted the center of gravity of the Society's operations to that city, although many of the most active members lived in New York. When the Natural History Survey of New York was inaugurated in 1836, Albany, not New York City, became the center of its operations and the repository of its collections, and the Albany Institute mobilized energies that might otherwise have been devoted to building scientific and technical institutions in New York. Just as the removal of the national capital to Washington, D.C., prevented Philadelphia from becoming a national scientific center, so the transfer of the New York State capital to Albany reduced New York City's chances of becoming a strong regional center.

Certainly there was no dearth of scientific talent and enterprise in New York City in the opening decades of the nineteenth century. Samuel Latham Mitchill's *Medical Repository* served as a national scientific journal, drawing contributions from far and wide, and Mitchill himself was a tireless organizer and promoter of useful knowledge. New York's medical community was badly rent by factional strife, but it managed to produce and sustain a high level of scientific activity at the College of Physicians and Surgeons and rival institutions and to generate a surprising number of enterprises for promoting useful knowledge. Dr. David Hosack's Elgin Botanic Garden, established and developed at his own expense, became an important adjunct of the College of Physicians and Surgeons by gift of the State of New York, which purchased it when Hosack could no longer afford to maintain it. Dr. Archibald Bruce's *American Mineralogical Journal* (1810-1814) provided a national outlet and rallying point for Americans interested in geology, mineralogy, and paleontology. Mitchill, Bruce, Hosack, and, later, John Torrey and William MacNeven trained a large number of students in chemistry, the earth sciences, and natural history and played a prominent role in organizing the pursuit of knowledge. John Griscom's popular lectures on chemistry and natural philosophy were well attended for many years, and John Scudder's American Museum, although not the equal of Peale's Museum, stimulated public interest in natural history. At the same time, important public figures—notably Robert R. Livingston, Robert Fulton, and De Witt Clinton—were ardent promoters of useful knowledge. Not far up the Hudson River, the United States Military Academy provided a cadre of professors well trained in mathematics, natural philosophy, and engineering, and Columbia College supplied the talents of Robert Adrain, James Renwick, and others. In 1814 Colonel George Gibbs, a wealthy gentleman scientist from Newport, Rhode Island, established a residence on Long Island and became active in New York scientific and cultural affairs. Soon afterward young John Torrey began building an herbarium and a competence in botany that were to make New York a clearing house for the botany of the Western expeditions in the 1840s and 1850s.

The efforts of the New York scientists, literati, and promoters of useful knowledge to organize in pursuit of their objectives reached maximum intensity in the period during and just after the War of 1812. In these years the Society for the Promotion of Useful Arts was invigorated by the activities of such avid devotees of science—especially chemistry, mineralogy, geology, and general natural history—as Samuel L. Mitchill, Archibald Bruce, Theodric Romeyn Beck, Colonel George Gibbs, Thomas C. Brownell, and Josiah Noyes. Many of these same individuals took the lead in promoting similar studies in the New York Historical Society. A cabinet of natural history was organized in the Society in 1816, and in March of the following year the Society established lectureships in the various branches of science and appointed lecturers who also chaired committees charged with collecting information and specimens in their respective departments. Mitchill was appointed chairman for zoology and geology, David Hosack for botany and vegetable physiology, John Griscom for chemistry and natural philosophy, and Colonel Gibbs for mineralogy.

Soon afterwards Gibbs reported in Biglow's *American Monthly Magazine and Critical Review* that the Society was planning to display specimens of minerals from every state in the Union in specially designed cases, "one case being devoted to each state, after the manner adopted in the national collection at the École des Mines at Paris."[20] Meanwhile, two entirely new institutions for promoting knowledge–the Literary and Philosophical Society of New York and the Lyceum of Natural History of New York–were being organized, and the city was providing meeting space for these and other institutions–the Academy of Fine Arts, the New York Historical Society, Scudder's American Museum, Griscom's chemical laboratory, and the United States Military Philosophical Society–in the Old Almshouse, now designated The New York Institution for the Promotion of the Arts and Sciences.

In examining the membership lists, public pronouncements, and activities of the various societies housed in the Old Almshouse, one is impressed with the strength of the interest in natural history, ranging from mineralogy and geology to paleontology, botany, and zoology. This interest had been present from the beginning in the Society for the Promotion of Useful Arts. It was equally apparent in De Witt Clinton's inaugural discourse before the Literary and Philosophical Society of New York and in the first and only volume of the Society's *Transactions.* It even manifested itself in the New York Historical Society, whose zealous natural historians threatened to transform the Society into an institution for promoting natural history. Finally, the passion for natural history produced in 1817 the Lyceum of Natural History of New York, the one scientific society that was to endure in New York despite many financial vicissitudes and the eventual destruction of its collections by fire.

The Lyceum was to be a worthy though impecunious counterpart of the Academy of Natural Sciences of Philadelphia and the Boston Society of Natural History, but the absence of a counterpart of the American Philosophical Society and the American Academy of Arts and Sciences meant that the mathematicians and physical scientists in the New York vicinity, including West Point, Rutgers University, Princeton, and New York itself, had to look elsewhere–to Philadelphia or Boston or Albany–for stimulation and publication. In vain did William C. Redfield assure Elias Loomis that his contributions to meteorology would be welcome at the Lyceum of Natural History. Philadelphia was a more natural outlet. Joseph Henry, too, looked to Philadelphia for scientific company after he left Albany for Princeton. As for John W. Draper, New York's leading physical scientist before the Civil War, his papers appeared in the *Journal of the Franklin Institute,* the *American Journal of Science,* the *American Journal of the Medical Sciences,* and the *Philosophical Magazine.* The founding of the American Ethnological Society and the New York Academy of Medicine in New

York in the 1840s indicated that the impetus toward organizing for the promotion of science and related arts was not extinct, but the rapid decline of the Literary and Philosophical Society of New York after 1830 brought an end to all efforts to emulate the example of the American Philosophical Society and the American Academy of Arts and Sciences.

And so it went in the various urban centers scattered from Boston to Charleston, from Cincinnati to New Orleans. The number and character of the local institutions for promoting knowledge varied with the resources–economic, cultural, and scientific–of the region, the enterprise of its citizens, and the peculiar circumstances of history, culture, geography, and individual genius. In Cincinnati Dr. Daniel Drake attempted with limited success to replicate the pattern of institutions for promoting knowledge he had admired while a student in Philadelphia. Charleston had its Literary and Philosophical Society for a time, New Orleans and St. Louis their academies of science at a later period. Each began with a broad statement of purposes and aspirations and a framework of classes or committees embracing the whole range of science and literature. In practice, however, natural history proved to be the only area of investigation capable of enlisting a sufficient number of professionals–usually medical men–and amateur cultivators to maintain the society in existence, if indeed it could survive.

It appears, then, that the record of societies for promoting knowledge in the period before 1860 was an honorable, though not a brilliant, one. Deprived for the most part of government support, they could not hope to emulate the model of the academies on the continent of Europe. Scattered in urban centers, none of which could claim national preeminence, dependent for support on members for the most part of limited means, hampered by public attitudes that placed no high value on intellectual achievement per se, they were equally incapable of attaining the eminence and influence of the leading British societies. Yet, despite these limitations, their role was a highly useful one. Without them the scientific communities scattered over the face of the country would have languished for lack of communication among themselves and with each other, and the general public, already too prone to neglect the cultivation of science and letters, would have been denied the stimulus, both practical and intellectual, provided by the publications, the collections, and the example of these societies for the improvement of knowledge and the human condition.

NOTES

1. Benjamin Franklin, *Writings*, ed. A. H. Smyth, 10 vols. (New York, 1907), 2:230.

2. "Preface," *Transactions of the American Philosophical Society* 1 (1771): xvii. The patriotic and utilitarian context of the early activities of the American Philosophical Society is discussed at length in Brooke Hindle, *The Pursuit of Science in Revolutionary America 1735-1789* (Chapel Hill: University of North Carolina Press, 1956), chaps. 6-7. See also *American Philosophical Society; an Historical Account of the Origin and Formation of the American Philosophical Society . . . and the Report of the Committee on the Date of the Foundation of the Society Accepted May 1, 1914* (Philadelphia: American Philosophical Society, 1914).

3. *Proceedings of the American Philosophical Society* 5 (1848-1853): 111-12. See also Whitfield J. Bell, Jr., "The American Philosophical Society and Medicine," *Bulletin of the History of Medicine* 40 (1966): 112-23.

4. "Old Minutes of the Society, from 1743 to 1838," *Proceedings of the American Philosophical Society* 22 (1885), pt. 3:453. A footnote comments concerning the establishment of this committee: "This was in fact the birth of the Historical Society of Pennsylvania."

5. *Proceedings of the American Philosophical Society* 6 (1854-1855): 67. A good account of the Society in the years 1835-1850 is Walter E. Gross, "The American Philosophical Society and the Growth of Science in the United States 1835-1850" (Ph.D. diss., University of Pennsylvania, 1970).

6. John Vaughan, *An Account of the American Philosophical Society* (Philadelphia: The American Philosophical Society, 1972).

7. "Report of the Special Committee on the Present Condition of the Society and the Means of Increasing Its Usefulness," Minutes of the American Philosophical Society, January 7, 1853, to April 21, 1865, Library of the American Philosophical Society, 291(May 1859):6.

8. *First Annual Report of the Proceedings of the Franklin Institute of the State of Pennsylvania, for the Promotion of the Mechanic Arts* . . . (Philadelphia: J. Harding, Printer, 1825), pp. 4-63.

9. Sydney L. Wright, *The Story of the Franklin Institute* (Philadelphia: Franklin Institute, 1938), chap. 1. I am indebted for information concerning the Institute's changing financial fortunes to Bruce Sinclair, whose study of the Franklin Institute is an important contribution to knowledge of scientific and technological activity in Philadelphia.

10 Edward J. Nolan, *A Short History of the Academy of Natural Sciences of Philadelphia* (Philadelphia: Academy of Natural Sciences, 1909), p. 16.

11. "Preface," *Memoirs of the American Academy of Arts and Sciences* 1 (1785): iv-vii.

12. "Charter of Incorporation, May 4, 1780," *Memoirs of the American Academy of Arts and Sciences* 11(1888): 78.

13. James Bowdoin, "A Philosophical Discourse, Publickly Addressed to the American Academy of Arts and Sciences, in Boston, on the Eighth of November, 1780: When the President Was Inducted into Office," *Memoirs of the American Academy of Arts and Sciences* 1:18-19.

14. *Proceedings of the American Academy of Arts and Sciences* 2 (1848-1852): 285-88.

15. Benjamin Vaughan to Nathaniel Bowditch, 13 August 1817, quoted in Nathan Reingold, ed., *Science in Nineteenth-Century America: A Documentary History* (New York: Hill and Wang, 1964), pp. 17-19.

16. *Massachusetts Agricultural Repository and Journal* 7 (1823): 320.

17. Simeon E. Baldwin, *The First Century of the Connecticut Academy of Arts and Sciences, 1799-1899* . . . (New Haven: Tuttle, Morehouse and Taylor Co. Press, 1901), p. xxii.

18. Thomas T. Bouvé, *Historical Sketch of the Boston Society of Natural History: With a Notice of the Linnaean Society, Which Preceded It* (Boston: Boston Society of Natural History, 1880), pp. 80-88.

19. De Witt Clinton, "An Introductory Discourse Delivered on the 4th of May, 1814," *Transactions of the Literary and Philosophical Society of New York* . . . 1 (1815):21 ff.

20. *American Monthly Magazine and Critical Review* 1 (1817): 48, 124.

B. Twentieth Century Perspectives

THE PUBLIC AND ITS PROBLEMS

An Essay in Political Inquiry

BY
JOHN DEWEY

CHICAGO
GATEWAY BOOKS
1946

What is the public? If there is a public, what are the obstacles in the way of its recognizing and articulating itself? Is the public a myth? Or does it come into being only in periods of marked social transition when crucial alternative issues stand out, such as that between throwing one's lot in with the conservation of established institutions or with forwarding new tendencies? In a reaction against dynastic rule which has come to be felt as despotically oppressive? In a transfer of social power from agrarian classes to industrial?

Is not the problem at the present time that of securing experts to manage administrative matters, other than the framing of policies? It may be urged that the present confusion and apathy are due to the fact that the real energy of society is now directed in all non-political matters by trained specialists who manage things, while politics are carried on with a machinery and ideas formed in the past to deal with quite another sort of situation. There is no particular public concerned in finding expert school instructors, competent doctors, or business managers. Nothing called a public intervenes to instruct physicians in the practice of the healing art or merchants in the art of salesmanship. The conduct of these callings and others characteristic of our time are decided by science and pseudo-science. The important governmental affairs at present, it may be argued, are also technically complicated matters to be conducted properly by experts. And if at present people are not educated to the recognition of the importance of finding experts and of entrusting administration to them, it may plausibly be asserted that the prime obstruction lies in the superstitious belief that there is a public concerned to determine the formation and execution of general social policies. Perhaps the apathy of the electorate is due to the irrelevant artificiality of the issues with which it is attempted to work up factitious excitement. Perhaps this artificiality is in turn mainly due to the survival of political beliefs and machinery from a period when science and technology were so immature as not to permit of a definite technique for handling definite social situations and meeting specific social needs. The attempt to decide by law that the legends of a primitive Hebrew people regarding the genesis of man are more authoritative than the results of scientific inquiry might be cited as a typical example of the sort of thing which is bound to happen when the accepted doctrine is that a public organized for political purposes, rather than experts guided by specialized inquiry, is the final umpire and arbiter of issues.

The questions of most concern at present may be said to be matters like sanitation, public health, healthful and adequate housing, transportation, planning of cities, regulation and distribution of immigrants, selection and management of personnel, right methods of instruction and preparation of competent teachers, scientific adjustment of taxation, efficient management of funds, and so on. These are technical matters, as much so as the construction of an efficient engine for purposes of traction or locomotion. Like it they are to be settled by inquiry into facts; and as the inquiry can be carried on only by those especially equipped, so the results of inquiry can be utilized only by trained technicians.. What has counting heads, decision by majority and the whole apparatus of traditional government to do with such things? Given such considerations, and the public and its organization for political ends is not only a ghost, but a ghost which walks and talks, and obscures, confuses and misleads governmental action in a disastrous way.

Personally I am far from thinking that such considerations, pertinent as they are to administrative activities, cover the entire political field. They ignore forces which have to be composed and resolved before technical and specialized action can come into play. But they aid in giving definiteness and point to a fundamental question: What, after all, is the public under present conditions? What are the reasons for its eclipse? What hinders it from finding and identifying itself? By what means shall its inchoate and amorphous estate be organized into effective political action relevant to present social needs and opportunities? What has happened to the Public in the century and a half since the theory of political democracy was urged with such assurance and hope?

Previous discussion has brought to light some conditions out of which the public is generated. It has also set forth some of the causes through which a "new age of human relationships" has been brought into being. These two arguments form the premises which, when they are related to each other, will provide our answer to the questions just raised. Indirect, extensive, enduring and serious consequences of conjoint and interacting behavior call a public into existence having a common interest in controlling these consequences. But the machine age has so enormously expanded, multiplied, intensified and complicated the scope of the indirect consequences, have formed such immense and consolidated unions in action, on an impersonal rather than a community basis, that the resultant public cannot identify and distinguish itself. And this discovery is obviously an antecedent condition of any effective organization on its part. Such is our thesis regarding the eclipse which the public idea and interest have undergone. There are too many publics and too much of public concern for our existing resources to cope with. The problem of a democratically organized public is primarily and essentially an intellectual problem, in a degree to which the political affairs of prior ages offer no parallel.

Our concern at this time is to state how it is that the machine age in developing the Great Society has invaded and partially disintegrated the small communities of former times without generating a Great Community. The facts are familiar enough; our especial affair is to point out their connections with the difficulties under which the organization of a democratic public is laboring. For the very familiarity with the phenomena conceals their significance and blinds us to their relation to immediate political problems.

This phase of the discussion may be concluded by consideration of the relation of experts to a democratic public. A negative phase of the earlier argument for political democracy has largely lost its force. For it was based upon hostility to dynastic and oligarchic aristocracies, and these have largely been reft of power. The oligarchy which now dominates is that of an economic class. It claims to rule, not in virtue of birth and hereditary status, but in virtue of ability in management and of the burden of social responsibilities which it carries, in virtue of the position which superior abilities have conferred upon it. At all events, it is a shifting, unstable oligarchy, rapidly changing its constituents, who are more or less at the mercy of accidents they cannot control and of technological inventions. Consequently, the shoe is now on the other foot. It is argued that the check upon the oppressive power of this particular oligarchy lies in an intellectual aristocracy, not in appeal to an ignorant, fickle mass whose interests are superficial and trivial, and whose judgments are saved from incredible levity only when weighted down by heavy prejudice.

It may be argued that the democratic movement was essentially transitional. It marked the passage from feudal institutions to industrialism, and was coincident with the transfer of power from landed proprietors, allied to churchly authorities, to captains of industry, under conditions which involved an emancipation of the masses from legal limitations which had previously hemmed them in. But, so it is contended in effect, it is absurd to convert this legal liberation into a dogma which alleges that release from old oppressions confers upon those emancipated the intellectual and moral qualities which fit them for sharing in regulation of affairs of state. The essential fallacy of the democratic creed, it is urged, is the notion that a historic movement which effected an important and desirable release from restrictions is either a source or a proof of capacity in those thus emancipated to rule, when in fact there is no factor common in the two things. The obvious alternative is rule by those intellectually qualified, by expert intellectuals.

This revival of the Platonic notion that philosophers should be kings is the more taking because the idea of experts is substituted for that of philosophers, since philosophy has become something of a joke, while the image of the specialist, the expert in operation, is rendered familiar and congenial by the rise of the physical sciences and by the conduct of industry. A cynic might indeed say that the notion is a pipe-dream, a revery entertained by the intellectual class in compensation for an impotence consequent upon the divorce of theory and practice, upon the remoteness of specialized science from the affairs of life: the gulf being bridged not by the intellectuals but by inventors and engineers hired by captains of industry. One approaches the truth more nearly when one says that the argument proves too much for its own cause. If the masses are as intellectually irredeemable as its premise implies, they at all events have both too many desires and too much power to permit rule by experts to obtain. The very ignorance, bias, frivolity, jealousy, instability, which are alleged to incapacitate them from share in political affairs, unfit them still more for passive submission to rule by intellectuals. Rule by an economic class may be disguised from the masses; rule by experts could not be covered up. It could be made to work only if the intellectuals became the willing tools of big economic interests. Otherwise they would have to ally themselves with the masses, and that implies, once more, a share in government by the latter.

A more serious objection is that expertness is most readily attained in specialized technical matters, matters of administration and execution which postulate that general policies are already satisfactorily framed. It is assumed that the policies of the experts are in the main both wise and benevolent, that is, framed to conserve the genuine interests of society. The final obstacle in the way of any aristocratic rule is that in the absence of an articulate voice on the part of the masses, the best do not and cannot remain the best, the wise cease to be wise. It is impossible for high-brows to secure a monopoly of such knowledge as must be used for the regulation of common affairs. In the degree in which they become a specialized class, they are shut off from knowledge of the needs which they are supposed to serve.

The strongest point to be made in behalf of even such rudimentary political forms as democracy has already attained, popular voting, majority rule and so on, is that to some extent they involve a consultation and discussion which uncover social needs and troubles. This fact is the great asset on the side of the political ledger. De Tocqueville wrote it down almost a century ago in his survey of the prospects of democracy in the United States. Accusing a democracy of a tendency to prefer mediocrity in its elected rulers, and admitting its exposure to gusts of passion and its openness to folly, he pointed out in effect that popular government is educative as other modes of political regulation are not. It forces a recognition that there are common interests, even though the recognition of *what* they are is confused; and the need it enforces of discussion and publicity brings about some clarification of what they are. The man who wears the shoe knows best that it pinches and where it pinches, even if the expert shoemaker is the best judge of how the trouble is to be remedied. Popular government has at least created public spirit even if its success in informing that spirit has not been great.

A class of experts is inevitably so removed from common interests as to become a class with private interests and private knowledge, which in social matters is not knowledge at all. The ballot is, as often said, a substitute for bullets. But what is more significant is that counting of heads compels prior recourse to methods of discussion, consultation and persuasion, while the essence of appeal to force is to cut short resort to such methods. Majority rule, just as majority rule, is as foolish as its critics charge it with being. But it never is *merely* majority rule. As a practical politician, Samuel J. Tilden, said a long time ago: "The means by which a majority comes to be a majority is the more important thing": antecedent debates, modification of views to meet the opinions of minorities, the relative satisfaction given the latter by the fact that it has had a chance and that next time it may be successful in becoming a majority. Think of the meaning of the "problem of minorities" in certain European states, and compare it with the status of minorities in countries having popular government. It is true that all valuable as well as new ideas begin with minorities, perhaps a minority of one. The important consideration is that opportunity be given that idea to spread and to become the possession of the multitude. No government by experts in which the masses do not have the chance to inform the experts as to their needs can be anything but an oligarchy managed in the interests of the few. And the enlightenment must proceed in ways which force the administrative specialists to take account of the needs. The world has suffered more from leaders and authorities than from the masses.

The essential need, in other words, is the improvement of the methods and conditions of debate, discussion and persuasion. That is *the* problem of the public. We have asserted that this improvement depends essentially upon freeing and perfecting the processes of inquiry and of dissemination of their conclusions. Inquiry, indeed, is a work which devolves upon experts. But their expertness is not shown in framing and executing policies, but in discovering and making known the facts upon which the former depend. They are technical experts in the sense that scientific investigators and artists manifest *expertise.* It is not necessary that the many should have the knowledge and skill to carry on the needed investigations; what is required is that they have the ability to judge of the bearing of the knowledge supplied by others upon common concerns.

THE LIMITATIONS OF THE EXPERT

BY HAROLD J. LASKI

THE day of the plain man has passed. No criticism of democracy is more fashionable in our time than that which lays emphasis upon his incompetence. This is, we are told, a big and complex world, about which we have to find our way at our peril. The plain man is too ignorant and too uninterested to be able to judge the adequacy of the answers suggested to our problems. As in medicine we go to a doctor, or in bridge-building to an engineer, so in matters of social policy we should go to an expert in social questions. He alone, we are told with increasing emphasis, can find his way about the labyrinthine intricacies of modern life. He alone knows how to find the facts, and determine what they mean. The plain man is simply obsolete in a world he has never been trained to understand. Either we must trust the making of fundamental decisions to experts, or there will be a breakdown in the machinery of government.

Now much of this skepticism is a natural and justifiable reaction from the facile and romantic optimism of the nineteenth century. Jefferson in America, Bentham in England did too easily assume not only an inherent rightness in the opinions of the multitude but also an instinctive wisdom in its choices. They did tend to think that social problems could be easily understood, and that public interest in their solution would be widespread and passionate. From their philosophy was born the dangerous inference that any man, without training in affairs, could hope usefully to control their operation. They did not see that merely to formulate rightly the nature of a social problem is far more difficult than to formulate rightly a problem in physics or chemistry. No one assumes that the plain man is entitled to an opinion about the ether or vitamins or the historicity of the Donation of Constantine. Why should it be assumed that he has competence about the rates of taxation, or the validity of tariff-schedules, or the principles of a penal code? Here, as in the fields of pure and applied science, his well-being, it is argued, depends essentially upon accepting the advice of the disinterested expert. The more elbow-room the latter possesses, the more likely we are to arrive at adequate decisions.

No one, I think, could seriously deny to-day that in fact none of our social problems is capable of wise resolution without formulation of its content by an expert mind. A Congressman at Washington, a member of Parliament at Westminster cannot hope to understand the policy necessary to a proper understanding of Soviet Russia merely by the light of nature. The facts must be gathered by men who have been trained to a special knowledge of the new Russia, and the possible inferences from those facts must be set out by them. The plain man cannot plan a town, or devise a drainage system, or decide upon the wisdom of compulsory vaccination without aid and knowledge at every turn from men who have specialized in those themes. He will make grave mistakes about them, possibly even fatal mistakes. He will not know what to look for; he may easily miss the significance of what he is told. That the contours of any subject must be defined by the expert before the plain man can see its full significance will, I believe, be obvious to anyone who has reflected upon the social process in the modern world.

II

But it is one thing to urge the need for expert consultation at every stage in making policy; it is another thing, and a very different thing, to insist that the expert's judgment must be final. For special knowledge and the highly trained mind produce their own limitations which, in the realm of statesmanship, are of decisive importance. *Expertise*, it may be argued, sacrifices the insight of common sense to intensity of experience. It breeds an inability to accept new views from the very depth of its preoccupation with its own conclusions. It too often fails to see round its subject. It sees its results out of perspective by making them the center of relevance to which all other results must be related. Too often, also, it lacks humility; and this breeds in its possessors a failure in proportion which makes them fail to see the obvious which is before their very noses. It has, also, a certain caste-spirit about it, so that experts tend to neglect all evidence which does not come from those who belong to their own ranks. Above all, perhaps, and this most urgently where human problems are concerned, the expert fails to see that every judgment he makes not purely factual in nature brings with it a scheme of values which has no special validity about it. He tends to confuse the importance of his facts with the importance of what he proposes to do about them.

Each one of these views needs illustration, if we are to see the relation of *expertise* to statesmanship in proper perspective. The expert, I suggest, sacrifices the insight of common sense to the intensity of his experience. No one can read the writings of Mr. F. W. Taylor, the efficiency-engineer, without seeing that his concentration upon the problem of reaching the maximum output of pig-iron per man per day made him come to see the laborer simply as a machine for the production of pig-iron. He forgot the complexities of human nature, the fact that the subject of his experiments had a will of his own whose consent was essential to effective success. Business men prophesied the rapid breakdown of the Russian experiment because it had eliminated that profit-making motive which experience had taught them was at the root of Western civilization. But they failed to see that Russia might call into play new motives and new emotions not less powerful, even if different in their operation, from the old. The economic experts of the early nineteenth century were fairly unanimous in insisting that the limitation of the hours of labor must necessarily result in a decrease of prosperity. They lacked the common sense to see that a prohibition upon one avenue of profit would necessarily lead to so intense an exploration of others as to provide a more than adequate compensation for the effort they deplored.

The expert, again, dislikes the appearance of novel views. Here, perhaps, the experience of science is most suggestive since the possibility of proof in this realm avoids the chief difficulties of human material. Everyone knows of the difficulties encountered by Jenner in his effort to convince his medical contemporaries of the importance of vaccination. The Royal Society refused to print one of Joule's most seminal papers. The opposition of men like Sir Richard Owen and Adam Sedgwick to Darwin resembled nothing so much as that of Rome to Galileo. Not even so great a surgeon as Simpson could see merit in Lister's discovery of antiseptic treatment. The opposition to Pasteur among medical men was so vehement that he declared regretfully that he did not know he had so many enemies. Lacroix and Poisson reported to the French Academy of Sciences that Galois' work on the theory of groups, which Cayley later put among the great mathematical achievements of the nineteenth century, was quite unintelligible. Everyone knows how biologists and physicists failed to perceive for long years the significance of Gregor Mendel and Willard Gibbs.

These are instances from realms where, in almost every case, measurable proof of truth was immediately obtainable; and, in each case, novelty of outlook was fatal to a perception of its importance. In social matters, where the problem of measurement is infinitely more difficult, the expert is entitled to far less assurance. He can hardly claim that any of his fundamental questions have been so formulated that he can be sure that the answer is capable of a certainly right interpretation. The student of race, for instance, is wise only if he admits that his knowledge of his subject is mainly a measure of his ignorance of its boundaries. The student of eugenics can do little more than insist that certain hereditary traits, deaf-mutism, for example, or hæmophilia, make breeding from the stocks tainted by them undesirable; he cannot tell us what fitness means nor show us how to breed the qualities upon which racial adequacy depends. It would be folly to say that we are destined never to know the laws which govern life; but, equally certainly, it would be folly to argue that our knowledge is sufficient to justify any expert, in any realm of social importance, claiming finality for his outlook.

He too often, also, fails to see his results in their proper perspective. Anyone who examines the conclusions built, for example, upon the use of intelligence tests will see that this is the case. For until we know exactly how much of the ability to answer the questions used as their foundation is related to differentiated home environments, how effectively, that is, the experiment is really pure, they cannot tell us anything. Yet the psychologists who accept their results have built upon them vast and glittering generalizations as, for instance, about the inferior mental quality of the Italian immigrant in America; as though a little common sense would not make us suspect conclusions indicating mental inferiority in the people which produced Dante and Petrarch, Vico and Machiavelli. Generalizations of this kind are merely arrogant; and their failure to see, as experts, the *a priori* dubiety of their results, obviously raises grave issues about their competence to pronounce upon policy.

Vital, too, and dangerous, is the expert's caste-spirit. The inability of doctors to see light from without is notorious; and a reforming lawyer is at least as strange a spectacle as one prepared to welcome criticism of his profession from men who do not practice it. There is, in fact, no expert group which does not tend to deny that truth may possibly be found outside the boundary of its private Pyrenees. Yet, clearly enough, to accept its dicta as final, without examination of their implications, would be to accept grave error as truth in almost every department of social effort. Every expert's conclusion is a philosophy of the second best until it has been examined in terms of a scheme of values not special to the subject matter of which he is an exponent.

Everyone knows, for example, that admirals invariably fail to judge naval policy in adequate terms; and in Great Britain, at any rate, the great military organizers, men like Cardwell and Haldane, have had to pursue their task in face of organized opposition from the professional soldier. The Duke of Wellington was never brought to see the advantage of the breech-loading rifle; and the history of the tank in the last war is largely a history of civilian enterprise the value of which the professional soldier was brought to see only with difficulty.

The expert, in fact, simply by reason of his immersion in a routine, tends to lack flexibility of mind once he approaches the margins of his special theme. He is incapable of rapid adaptation to novel situations. He unduly discounts experience which does not tally with his own. He is hostile to views which are not set out in terms he has been accustomed to handle. No man is so adept at realizing difficulties within the field that he knows; but, also, few are so incapable of meeting situations outside that field. Specialism seems to breed a horror of unwonted experiment, a weakness in achieving adaptability, both of which make the expert of dubious value when he is in supreme command of a situation.

This is, perhaps, above all because the expert rarely understands the plain man. What he knows, he knows so thoroughly that he is impatient with men to whom it has to be explained. Because he practices a mystery, he tends to assume that, within his allotted field, men must accept without question the conclusions at which he has arrived. He too often lacks that emollient quality which makes him see that conclusions to which men assent are far better than conclusions which they are bidden, without persuasion, to decline at their peril. Everyone knows how easily human personality becomes a unit in a statistical table for the bureaucrat; and there must be few who have not sometimes sympathized with the poor man's indignation at the social worker. People like Jane Addams, who can retain, amid their labors, a sense of the permanent humanity of the poor are rare enough to become notable figures in contemporary life.

The expert, in fact, tends to develop a certain condescension towards the plain man which goes far towards the invalidation of his *expertise*. Men in India who have become accustomed to the exercise of power, cannot believe, without an imaginative effort of which few of them are capable, that the Indian is entitled to his own ideas of how he should be governed. Civil servants tend easily to think that members of Parliament or Congress are an ignorant impediment to their labors. Professional historians, who cultivate some minute fragment of an epoch's history, cannot appreciate the superb incursions of a brilliant amateur like Mr. H. G. Wells. It has taken professional economists more than a generation to realize that the trade unions have a contribution to make to the understanding of industrial phenomena without which their own interpretation is painfully incomplete.

There is, in fact, not less in the expert's mind than in that of the plain man what Mr. Justice Holmes has termed an "inarticulate major premise" quite fundamental to his work. I have known an expert in the British Foreign Office whose advice upon China was built upon the assumption that the Chinese have a different human nature from that of Englishmen; and what was, in fact, an obvious private prejudice was, for him, the equally obvious outcome of a special experience which could not brook contradiction. Judges of the Supreme Court have had no difficulty in making the Fourteenth Amendment the embodiment of the *laissez-faire* philosophy of the nineteenth century; and few of them have realized that they were simply making the law express their unconscious dislike of governmental experiment. The history of trade-union law in England is largely an attempt, of course mainly unconscious, by judicial experts to disguise their dislike of working-men's organization in terms of a mythology to which the convenient name of "public policy" could be attached. The attitude of the British High Command to the death-penalty, of lawyers like Lord Eldon to the relaxation of penal severity, of business men to secrecy in finance, of statesmen to proposals for institutional reconstruction are all revelations of the expert's dislike of abandoning premises which, because he has grown accustomed to them, he tends to equate with the inevitable foundations of truth.

The expert tends, that is to say, to make his subject the measure of life, instead of making life the measure of his subject. The result, only too often, is an inability to discriminate, a confusion of learning with wisdom. "The fixed person for the fixed duties," Professor Whitehead has written, "who in older societies was such a godsend, in the future will be a public danger." In a sense, indeed, the more expert such fixed persons are, the more dangerous they are likely to be. For your great chemist, or doctor, or engineer, or mathematician is not an expert about life; he is precisely an expert in chemistry or medicine, engineering or mathematics. And the more highly expert he is, the more profoundly he is immersed in his routine, the less he is likely to know of the life about him. He cannot afford the time or the energy to give to life what his subject demands from him. He restrains his best intellectual effort within the routine about which he is a specialist. He does not co-ordinate his knowledge of a part with an attempt at wisdom about the whole.

This can be seen from many angles. Lord Kelvin was a great physicist, and his discoveries in cable-laying were of supreme importance to its development; but when he sought to act as a director of a cable-laying company, his complete inability to judge men resulted in serious financial loss. Faraday was obviously one of the half-dozen outstanding physicists of modern times; but in the field of theological belief, he retained convictions which no man of common sense could accept. Mr. Henry Ford is obviously a business man of genius; but, equally obviously, his table talk upon themes outside his special sphere reveals a mentality which is mediocre in the extreme. Charles Babbage rendered immense service to the development of statistical science; but when he came to judge one of Tennyson's most famous poems he missed its beauty through an over-vivid sense of its failure to conform to the revelations of the census returns.

The expert, in short, remains expert upon the condition that he does not seek to co-ordinate his specialism with the total sum of human knowledge. The moment that he seeks that coordination he ceases to be an expert. A doctor, a lawyer, an engineer who sought to act in terms of his specialism as President or Prime Minister would inevitably fail; to succeed, he must cease to be an expert. The wisdom that is needed for the direction of affairs is not an expert technic but a balanced equilibrium. It is a knowledge of how to use men, a faculty of judgment about the practicability of principles. It consists not in the possession of specialized knowledge, but in a power to utilize its results at the right moment, and in the right direction.

III

My point may perhaps be made by saying that *expertise* consists in such an analytic comprehension of a special realm of facts that the power to see that realm in the perspective of totality is lost. Such analytic comprehension is purchased at the cost of the kind of wisdom essential to the conduct of affairs. The doctor tends to think of men as patients; the teacher sees them as pupils; the statistician as units in a table. Bankers too often fail to realize that there is humanity even in men who have no check-books; Marxian socialists see sinister economic motive in the simplest expressions of the universal appetite for power. To live differently is to think differently; and to live as an expert in a small division of human knowledge is to make its principles commensurate with the ultimate deposit of historic experience. Not in that way does wisdom come.

Because a man is an expert on medieval French history, that does not make him the best judge of the disposition of the Saar Valley in 1919. Because a man is a brilliant prison doctor, that does not make him the person who ought to determine the principles of a penal code. The skill of the great soldier does not entitle him to decide upon the scale of military armament; just as no anthropologist, simply as an anthropologist, would be a fitting governor for a colonial territory peopled by native races. To decide wisely, problems must be looked at from an eminence. Intensity of vision destroys the sense of proportion. There is no illusion quite so fatal to good government as that of the man who makes his expert insight the measure of social need. We do not get progress in naval disarmament when admirals confer. We do not get legal progress from meetings of Bar associations. Congresses of teachers seem rarely to provide the means of educational advance. The knowledge of what can be done with the results obtained in special disciplines seems to require a type of co-ordinating mind to which the expert, as such, is simply irrelevant.

This may be looked at from two points of view. "Political heads of departments are necessary," said Sir William Harcourt, "to tell the civil service what the public will not stand." That is, indeed, an essential picture of the place of the expert in public affairs. He is an invaluable servant and an impossible master. He can explain the consequences of a proposed policy, indicate its wisdom, measure its danger. He can point out possibilities in a proposed line of action. But it is of the essence of public wisdom to take the final initiative out of his hands.

For any political system in which a wide initiative belongs to the expert is bound to develop the vices of bureaucracy. It will lack insight into the movement and temper of the public mind. It will push its private nostrums in disregard of public wants. It will become self-satisfied and self-complacent. It will mistake its technical results for social wisdom, and it will fail to see the limits within which its measures are capable of effective application. For the expert, by definition, lacks contact with the plain man. He not only does not know what the plain man is thinking; he rarely knows how to discover his thoughts. He has dwelt so austerely in his laboratory or his study that the content of the average mind is a closed book to him. He is at a loss how to manipulate the opinions and prejudices which he encounters. He has never learned the art of persuading men into acceptance of a thing they only half understand. He is remote from the substance of their lives. Their interests and hopes and fears have never been the counters with which he has played. He does not realize that, for them, his technical formulæ do not carry conviction because they are, as formulæ, incapable of translation into terms of popular speech. For the plain man, he is remote, abstract, alien. It is only the juxtaposition of the statesman between the expert and the public which makes specialist conclusions capable of application.

That, indeed, is the statesman's basic task. He represents, at his best, supreme common sense in relation to *expertise*. He indicates the limits of the possible. He measures what can be done in terms of the material at his disposal. A man who has been for long years in public affairs learns the art of handling men so as to utilize their talents without participating in their experience. He discovers how to persuade antagonistic views. He finds how to make decisions without giving reasons for them. He can judge almost by intuition the probable results of giving legislative effect to a principle. He comes to office able to co-ordinate varied aspects of *expertise* into something which looks like a coherent program. He learns to take risks, to trust to sub-conscious insight instead of remaining dependent upon reasoned analysis. The expert's training is, as a rule, fatal to these habits which are essential to the leadership of a multitude. That is why, for example, the teacher and the scholar are rarely a success in politics. For they have little experience of the need for rapid decision; and their type of mental discipline leads them to consider truth in general rather than the truth of popular discussion. They have not been trained to the business of convincing the plain man; and modern government is impossible to those who do not possess this art.

Nothing, indeed, is more remarkable in a great public department than to watch a really first-rate public man drive his team of expert officials. He knows far less than they do of the affairs of the Department. He has to guess at every stage the validity of their conclusions. On occasion, he must either choose between alternatives which seem equally balanced or decide upon a policy of which his officials disapprove. Not seldom, he must quicken their doubts into certainties; not seldom, also, he must persuade them into paths they have thus far refused to tread. The whole difference between a great Minister and a poor one lies in his ability to utilize his officials as instruments. His success depends upon weaving a policy from the discrete threads of their *expertise*. He must discover certain large principles of policy and employ them in finding the conditions of its successful operation. He must have the power to see things in a big way, to simplify, to co-ordinate, to generalize. Anyone who knows the work of Lord Haldane at the British War Office from 1906 to 1911, or of Mr. Arthur Henderson as Foreign Secretary in the last eighteen months, can understand the relation between the statesman and his expert which makes, and which alone can make, for successful administration.

Its essence, as a relation, is that the ultimate decisions are made by the amateur and not by the specialist. It is that fact which gives them coherence and proportion. A cabinet of experts would never devise a great policy. Either their competing specialisms would clash, if their *expertise* was various in kind, or its perspective would be futile because it was similar. The amateur brings to them the relevance of the outer world and the knowledge of men. He disposes of private idiosyncrasy and technical prejudice. In convincing the non-specialist Minister that a policy propounded is either right or wrong, the expert is already halfway to convincing the public of his plans; and if he fails in that effort to convince, the chances are that his plans are, for the environment he seeks to control, inadequate or mistaken. For politics by its nature is not a philosophy of technical ideals, but an art of the immediately practical. And the statesman is pivotal to its organization because he acts as the broker of ideas without whom no bridges can be built between the expert and the multitude. It is no accident, but an inherent quality of his character, that the expert distrusts his fellow-specialist when the latter can reach that multitude. For him the gift of popular explanation is a proof of failure in the grasp of the discipline. His intensity of gaze makes him suspect the man who can state the elements of his mystery in general terms. He knows too much of minutiæ to be comfortable upon the heights of generalization.

Nor must we neglect the other aspect of the matter. "The guest," said Aristotle with his homely wisdom, "will judge better of a feast than the cook." However much we may rely upon the expert in formulating the materials for decision, what ultimately matters is the judgment passed upon the results of policy by those who are to live by them. Things done by government must not only appear right to the expert; their consequences must seem right to the plain and average man. And there is no way known of discovering his judgment save by deliberately seeking it. This, after all, is the really final test of government; for, at least over any considerable period, we cannot maintain a social policy which runs counter to the wishes of the multitude.

It is not the least of our dangers that we tend, from our sense of the complexity of affairs, to underestimate both the relevance and the significance of those wishes. We are so impressed by the plain man's ignorance that we tend to think his views may be put aside as unimportant. Not a little of the literature upon the art of government to-day is built upon the supposition that the plain man has no longer any place in social economy. We know, for example, that he does not understand the technicalities of the gold standard. It is clear that it would be folly to consult him upon matters like the proper area for the generation of electricity supply, or the amount that it is wise for a government to spend in testing the action of pavements under changing temperatures and variations of load. But the inference from a knowledge that the plain man is ignorant of technical detail and, broadly speaking, uninterested in the methods by which its results are attained, is certainly not the conclusion that the expert can be left to make his own decisions.

For the results of the gold standard are written plain in the life of the average man. The consequences of an inefficient electricity supply are apparent to him every day. It is his motor car which uses the roads, and he makes up his mind about the quality of the road-service with which he is provided. Every degree by which he is separated from consultation about decisions is a weakening of the governmental process. Neither goodwill in the expert nor efficiency in the performance of his function ever compensates in a state for failure to elicit the interest of the plain man in what is being done. For the nature of the result is largely unknown save as he reports his judgment upon it; and only as he reports that judgment can the expert determine in what direction his plans must move. Every failure in consultation, moreover, separates the mind of the governors from those who are governed; this is the most fertile source of misunderstanding in the state. It is the real root of the impermanence of autocracies which fail from their inability to plumb the minds of those by whose opinions, ultimately, they must live.

The importance of the plain man's judgment is, in short, the foundation upon which the expert, if he is to be successful, must seek to build. It is out of that judgment, in its massive totality, that every society forms its scheme of values. The limits of possible action in society are always set by that scheme. What can be done is not what the expert thinks ought to be done. What can be done is what the plain man's scheme of values permits him to consider as just. His likes and dislikes, his indifference and his inertia, circumscribe at every stage the possibilities of administration. That is why a great expert like Sir Arthur Salter has always insisted upon the importance of advisory committees in the process of government. He has seen that the more closely the public is related to the work of *expertise*, the more likely is that work to be successful. For the relation of proximity of itself produces conviction. The public learns confidence, on the one hand, and the expert learns proportion on the other. Confidence in government is the secret of stability, and a sense of proportion in the expert is the safeguard against bureaucracy.

At no time in modern history was it more important than now that we should scrutinize the claims of the expert more critically; at no time, also, was it more important that he himself should be skeptical about his claims. Scientific invention has given us a material power of which the possible malignancy is at least as great as its contingent benefits. The danger which confronts us is the quite fatal one that, by the increase of complexity in civilization, we may come to forget the humanity of men. A mental climate so perverted as this would demonstrate at a stroke the fragility of our social institutions. For it would reveal an abyss between rulers and subjects which no amount of technical ingenuity could bridge. The material power that our experts multiply brings with it no system of values. It can only be given a system related to the lives of ordinary people to the degree that they are associated with its use. To exclude them from a share in its direction is quite certainly to exclude them also from a share in its benefits; for no men have been able in the history of past societies exclusively to exercise its authority without employing it ultimately for their own ends. Government by experts would, however ardent their original zeal for the public welfare, mean after a time government in the interest of experts. Of that the outcome would be either stagnation, on the one hand. or social antagonism, upon the other.

IV

Our business, in the years which lie ahead, is clearly to safeguard ourselves against this prospect. We must ceaselessly remember that no body of experts is wise enough, or good enough, to be charged with the destiny of mankind. Just because they are experts, the whole of life is, for them, in constant danger of being sacrificed to a part; and they are saved from disaster only by the need of deference to the plain man's common sense. It is, I believe, upon the perpetuation of this deference that our safety very largely depends.

But it will be no easy thing to perpetuate it. The expert, to-day, is accustomed to a veneration not very different from that of the priest in primitive societies; for the plain man he, like the priest, exercises a mystery into which the uninitiated cannot enter. To strike a balance between necessary respect and skeptical attack is a difficult task. The experience of the expert is so different, his approach to life so dissimilar, that expert and plain man are often impatient of each other's values. Until we can somehow harmonize them, our feet will be near to the abyss.

Nor must we forget that to attain such harmony immense changes in our social habits will be necessary. We shall have to revolutionize our educational methods. We shall have to reconstruct the whole fabric of our institutions. For the first time, perhaps, in the history of mankind, we shall have, as a civilization, deliberately to determine what kind of life we desire to live. We must so determine it remembering that the success of our effort will depend upon harnessing to its fortunes the profounder idealism of ordinary men and women. We shall appeal to that idealism only as we give it knowledge and persuade it that the end we seek is one in which it, too, can hope to share.

PUBLIC PAPERS OF THE PRESIDENTS
OF THE UNITED STATES

Dwight D. Eisenhower

1960–61

Containing the Public Messages, Speeches, and
Statements of the President

JANUARY 1, 1960, TO JANUARY 20, 1961

421 ¶ Farewell Radio and Television Address to the American People. *January* 17, 1961

[Delivered from the President's Office at 8:30 p.m.]

My fellow Americans:

Three days from now, after half a century in the service of our country, I shall lay down the responsibilities of office as, in traditional and solemn ceremony, the authority of the Presidency is vested in my successor.

This evening I come to you with a message of leave-taking and farewell, and to share a few final thoughts with you, my countrymen.

Like every other citizen, I wish the new President, and all who will labor with him, Godspeed. I pray that the coming years will be blessed with peace and prosperity for all.

Our people expect their President and the Congress to find essential agreement on issues of great moment, the wise resolution of which will better shape the future of the Nation.

My own relations with the Congress, which began on a remote and tenuous basis when, long ago, a member of the Senate appointed me to West Point, have since ranged to the intimate during the war and immediate post-war period, and, finally, to the mutually interdependent during these past eight years.

In this final relationship, the Congress and the Administration have, on most vital issues, cooperated well, to serve the national good rather than mere partisanship, and so have assured that the business of the Nation should go forward. So, my official relationship with the Congress ends in a feeling, on my part, of gratitude that we have been able to do so much together.

II.

We now stand ten years past the midpoint of a century that has witnessed four major wars among great nations. Three of these involved our own country. Despite these holocausts America is today the strongest, the most influential and most productive nation in the world. Understandably proud of this pre-eminence, we yet realize that America's leadership and prestige depend, not merely upon our unmatched material progress, riches and military strength, but on how we use our power in the interests of world peace and human betterment.

III.

Throughout America's adventure in free government, our basic purposes have been to keep the peace; to foster progress in human achievement, and to enhance liberty, dignity and integrity among people and among nations. To strive for less would be unworthy of a free and religious people. Any failure traceable to arrogance, or our lack of comprehension or readiness to sacrifice would inflict upon us grievous hurt both at home and abroad.

Progress toward these noble goals is persistently threatened by the conflict now engulfing the world. It commands our whole attention, absorbs our very beings. We face a hostile ideology—global in scope, atheistic in character, ruthless in purpose, and insidious in method. Unhappily the danger it poses promises to be of indefinite duration. To meet it successfully, there is called for, not so much the emotional and transitory sacrifices of crisis, but rather those which enable us to carry forward steadily, surely, and without complaint the burdens of a prolonged and complex struggle—with liberty the stake. Only thus shall we remain, despite every provocation, on our charted course toward permanent peace and human betterment.

Crises there will continue to be. In meeting them, whether foreign or domestic, great or small, there is a recurring temptation to feel that some spectacular and costly action could become the miraculous solution to all current difficulties. A huge increase in newer elements of our defense; development of unrealistic programs to cure every ill in agriculture; a dramatic expansion in basic and applied research—these and many other possibilities, each possibly promising in itself, may be suggested as the only way to the road we wish to travel.

But each proposal must be weighed in the light of a broader consideration: the need to maintain balance in and among national programs—balance between the private and the public economy, balance between cost and hoped for advantage—balance between the clearly necessary and the comfortably desirable; balance between our essential requirements as a nation and the duties imposed by the nation upon the individual; balance between actions of the moment and the national welfare of the future. Good judgment seeks balance and progress; lack of it eventually finds imbalance and frustration.

The record of many decades stands as proof that our people and their government have, in the main, understood these truths and have responded to them well, in the face of stress and threat. But threats, new in kind or degree, constantly arise. I mention two only.

IV.

A vital element in keeping the peace is our military establishment. Our arms must be mighty, ready for instant action, so that no potential aggressor may be tempted to risk his own destruction.

Our military organization today bears little relation to that known by any of my predecessors in peacetime, or indeed by the fighting men of World War II or Korea.

Until the latest of our world conflicts, the United States had no armaments industry. American makers of plowshares could, with time and as required, make swords as well. But now we can no longer risk emergency improvisation of national defense; we have been compelled to create a permanent armaments industry of vast proportions. Added to this, three and a half million men and women are directly engaged in the defense establishment. We annually spend on military security more than the net income of all United States corporations.

This conjunction of an immense military establishment and a large arms industry is new in the American experience. The total influence—economic, political, even spiritual—is felt in every city, every State house, every office of the Federal government. We recognize the imperative need for this development. Yet we must not fail to comprehend its grave implications. Our toil, resources and livelihood are all involved; so is the very structure of our society.

In the councils of government, we must guard against the acquisition of unwarranted influence, whether sought or unsought, by the military-industrial complex. The potential for the disastrous rise of misplaced power exists and will persist.

We must never let the weight of this combination endanger our liberties or democratic processes. We should take nothing for granted. Only an alert and knowledgeable citizenry can compel the proper meshing of the huge industrial and military machinery of defense with our peaceful methods and goals, so that security and liberty may prosper together.

Akin to, and largely responsible for the sweeping changes in our industrial-military posture, has been the technological revolution during recent decades.

In this revolution, research has become central; it also becomes more formalized, complex, and costly. A steadily increasing share is conducted for, by, or at the direction of, the Federal government.

Today, the solitary inventor, tinkering in his shop, has been overshadowed by task forces of scientists in laboratories and testing fields. In the same fashion, the free university, historically the fountainhead of free ideas and scientific discovery, has experienced a revolution in the conduct of research. Partly because of the huge costs involved, a government contract becomes virtually a substitute for intellectual curiosity. For every old blackboard there are now hundreds of new electronic computers.

The prospect of domination of the nation's scholars by Federal employment, project allocations, and the power of money is ever present—and is gravely to be regarded.

Yet, in holding scientific research and discovery in respect, as we should, we must also be alert to the equal and opposite danger that public policy could itself become the captive of a scientific-technological elite.

It is the task of statesmanship to mold, to balance, and to integrate these and other forces, new and old, within the principles of our democratic system—ever aiming toward the supreme goals of our free society.

V.

Another factor in maintaining balance involves the element of time. As we peer into society's future, we—you and I, and our government—must avoid the impulse to live only for today, plundering, for our own ease and convenience, the precious resources of tomorrow. We cannot mortgage the material assets of our grandchildren without risking the loss also of their political and spiritual heritage. We want democracy to survive for all generations to come, not to become the insolvent phantom of tomorrow.

VI.

Down the long lane of the history yet to be written America knows that this world of ours, ever growing smaller, must avoid becoming a community of dreadful fear and hate, and be, instead, a proud confederation of mutual trust and respect.

Such a confederation must be one of equals. The weakest must come to the conference table with the same confidence as do we, protected as we are by our moral, economic, and military strength. That table, though scarred by many past frustrations, cannot be abandoned for the certain agony of the battlefield.

Disarmament, with mutual honor and confidence, is a continuing imperative. Together we must learn how to compose differences, not with arms, but with intellect and decent purpose. Because this need is so sharp and apparent I confess that I lay down my official responsibilities in this field with a definite sense of disappointment. As one who has witnessed the horror and the lingering sadness of war—as one who knows that another war could utterly destroy this civilization which has been so slowly and painfully built over thousands of years—I wish I could say tonight that a lasting peace is in sight.

Happily, I can say that war has been avoided. Steady progress toward our ultimate goal has been made. But, so much remains to be done. As a private citizen, I shall never cease to do what little I can to help the world advance along that road.

VII.

So—in this my last good night to you as your President—I thank you for the many opportunities you have given me for public service in war and peace. I trust that in that service you find some things worthy; as for the rest of it, I know you will find ways to improve performance in the future.

You and I—my fellow citizens—need to be strong in our faith that all nations, under God, will reach the goal of peace with justice. May we be ever unswerving in devotion to principle, confident but humble with power, diligent in pursuit of the Nation's great goals.

To all the peoples of the world, I once more give expression to America's prayerful and continuing aspiration:

We pray that peoples of all faiths, all races, all nations, may have their great human needs satisfied; that those now denied opportunity shall come to enjoy it to the full; that all who yearn for freedom may experience its spiritual blessings; that those who have freedom will understand, also, its heavy responsibilities; that all who are insensitive to the needs of others will learn charity; that the scourges of poverty, disease and ignorance will be made to disappear from the earth, and that, in the goodness of time, all peoples will come to live together in a peace guaranteed by the binding force of mutual respect and love.

PART III

CONTEMPORARY PERSPECTIVES ON THE RELATIONSHIP BETWEEN DEMOCRACY AND EXPERTISE

BACKGROUND AND SUMMARY

Despite the misgivings voiced by President Eisenhower in the concluding selection from Part II of this Reader, as the United States entered the 1960s most Americans continued to place considerable store in "experts" and the notion of "expertise." Many looked forward to a society in which partisan political disputes might be diminished or defused through the mediation of professional advisers. Expert participation in the political process, it was hoped, would lead to broader agreement on the proper objections of government policy and help to clarify the best means to realize those ends.

This vision of a relatively harmonious, progressively more "rational" and tolerant America received perhaps its most classic enunciation in a 1966 article by Robert E. Lane entitled "The Decline of Politics and Ideology in a Knowledgable Society", the first selection in Part III. Although much of the earlier portion of the piece is of at best nostalgic interest (and despite the fact that his conception of science and scientific progress has since been largely discredited), Lane's article remains important in the present context. At the heart of his argument is the two-fold contention that (a) the standards and criteria of "objective knowledge" (as reflected in the modern scientific disciplines) provide an alternative basis for decision-making to those associated with the domain of "pure politics", where "calculations of influence, power, or electoral advantage" tend to prevail (p. 658); and (b) that in the contemporary United States, the spheres of "politics" and "ideology" will continue to decline in proportion as the domain of "objective knowledge" increases (pp. 658-60). Both of these propositions—especially the first—retain a hold on popular imagination, and each provides a justification or rationale for a reliance on professional experts as a means by which to "de-politicize" controversial issues of public policy.

Developments in the intervening two decades suggest that the prognoses of Lane and others like him were unduly optimistic, not to say naive. Whereas Lane had a benign, if not complacent view of the contribution of the various organized professions in translating "knowledge" into "public policy—"not all good, but generally responsive to the needs of society" (p. 662)—current appraisals of the role of experts in the policy-making process tend to be a good deal more ambivalent. Indeed, as Dorothy Nelkin notes in the second selection in this section of the Reader, disputes in recent years reveal that "access to knowledge and expertise has itself become a source of conflict, as various groups realize its growing implications for political choice" (p. 37).

In her article, "The Political Impact of Technical Expertise," Nelkin chronicles the growing reliance on "countervailing expertise" in the context of the so-called "advocacy politics" which began to characterize disputes in areas such as environmental and nuclear policy during the 1970s, and proposes by means of two case studies to examine "some of the implications of the increasing involvement of scientists in controversial areas" of public concern. (p. 35). From a review and appraisal of these two cases—one involving a power plant citing controversy in New York, the second a dispute surrounding a proposed runway extension at Boston's Logan Airport—Nelkin concludes that, far from contributing to a more consensual style of decision-making (as Lane seems to have envisaged), the involvement of experts in complex policy matters often serves only to intensify the level of conflict. She makes a number of other interesting general observations based on the case studies, and these are presented in summary form on pages 51–4 of her article.

What view do "the experts" themselves entertain of their involvement in policy disputes in the contemporary setting? The short article that follows the Nelkin selection, by Harvey Brooks, a professor of physics at Harvard and a leading figure in the American scientific community, provides a useful representative statement (all the more so since Nelkin addressed herself specifically to the role of scientists).

Writing in 1975, Brooks opens with the observation that "A profound change has taken place in the relation between the expert community and the political community in the last ten years" and expresses his dismay over the fact that the image of the American scientist, along with that of other experts, has been "severely tarnished" (p. 257). In seeking to answer the question, "What is happening, and why?", Brooks seizes upon a point Nelkin had mentioned in passing in her article, namely, the increasing involvement of scientists in policy areas with significant redistributive implications for society as a whole. So long as scientists were principally called upon to advise government officials in such areas as the design of the space program, where their role "was generally seen as evaluating alternatives to generally agreed upon ends" and where "Nobody's ox was being gored" (p. 258), Brooks suggests, the prestige of the scientific community was not seriously endangered. But once they became implicated in decision-making processes in areas (such as environmental policy) that involve "intense conflicts or apparent conflicts of value", in which "the distinction between means and ends is often blurred" and attention is therefore more likely to be called to "the non-technical premises that often underlie technical or professional advice" *(ibid.)*, American scientists were confronted with a distinctive set of problems which their prior experience as counsellors to government had not adequately prepared them to cope with, and their public image suffered accordingly.

Having laid out "the nub of the matter" as he conceives it, Brooks devotes the rest of his article to an overview of particular problems confronted by today's scientists when trying to work with politicians in controversial policy domains—for example, the problem of presenting "tentative evidence and provisional conclusions" to the politician "who naturally wants clear-cut answers" (pp. 258–9). He concludes by suggesting that so long as they are able to adapt to new circumstances in a resourceful and responsible manner, the credibility of scientists need not be jeopardized.

The progression from Brook's closing admonition and expression of hope for the continued vitality of scientific input into the American policy-making process, to the article by Yaron Ezrahi that follows, is a natural one. For in this article, Ezrahi (an Israeli sociologist who has written extensively on American science) argues that many members of the scientific community have been frustrated in their advisory roles because they have an unrealistic and, in his view, undesirable view of how the policy-making process should function. This view, which Ezrahi labels the "utopian rationalist view", "regards any political aims and considerations as obstacles to be overcome on the way to the progressive rationalization of decisions and actions" (p. 112).

What distinguishes the "utopian rationalist", according to Ezrahi, is the belief that "when knowledge relevant to treating problems of policy is available, the process of making policy should, and in principle can, be adjusted to ensure the maximum assimilation of that knowledge into the decisions made" (p. 111). It is because they tend to subscribe to this view, Ezrahi suggests, that scientists often find it difficult fully to accept the notion that "the political components of the process of public policy-making [are] inherent ingredients which neither can nor—in so far as one is committed to a democratic political order—should be eradicated" (p. 112). Unlike the "pragmatic rationalist", who recognizes that policies "are not exclusively means of achieving technically rational goals but are also, and often incompatibly, the means of legitimating decisions politically, of furnishing publicly defensible records . . . and of attaining other ends which are primarily political" (*ibid.*), the ideo-typical "utopian rationalist" is perpetually" at war with politicians and with politics" (p. 130).

In the opening stages of his article, Ezrahi attempts to highlight some of the theoretical and practical limitations of the "utopian rationalist" perspective. In general, he argues that that perspective tends to blind scientific advisers to the very different political and "strategic" contexts in which they may find themselves, as well as to the ways in which their responsibilities may vary depending on the particular roles assigned them. Drawing on a wide array of illustrative cases, Ezrahi then attempts to flesh out these claims by calling attention to some of the important distinctions between policy-making processes characterized by, respectively, "Agreement on Objectives with Scientific Consensus" (pp. 114–5), "Agreement on Objectives Without Scientific Consensus" (pp. 115–7), "Scientific Consensus and Disagreements about Objectives" (pp. 117–120), and "Disagreement about Objectives Coupled with Scientific Dissensus" (pp. 120–4).

Although he notes that "Utopian and pragmatic rationalists share a common commitment to the rationalisation of public policy by increasing the incorporation of scientific knowledge in the definition and treatment of problems" (p. 130), Ezrahi concludes that the "utopian rationalist" approach to the assimilation of scientific knowledge into policy-making has particularly dangerous implications from the point of view of democratic theory. In closing, he warns that "the very idea that the scientific adviser should act as a manipulator of political arrangements to enhance the influence of knowledge is unacceptable, even in the service of the ideal of rationality. . . . The justifiable partial autonomy of politics vis-a-vis the pretensions of scientific experts rests upon the premise that there is no legitimate alternate to the publicly chosen procedures for the democratic ordering and compromise of diverse values, desires and interests" (p. 131).

One clear lesson that may be gleaned from Ezrahi's article, as well as from the earlier selections by Nelkin and Brooks, is that although they generally command considerable respect and are viewed as unquestioned experts in their specialized domains of inquiry, the type of reception given scientists (and scientific pronouncements) in matters of politics varies considerably depending upon the situation or the issues being considered. The analyses of all three authors suggest, for example, that when they purport—either on their own initiative or at the behest of others—to speak with authority as to the merits of competing and controversial policy objectives (as opposed to advising others as to the most effective means to realize agreed-upon ends), the deference traditionally accorded scientists may not to be forthcoming at all, or only grudgingly. Under such circumstances, and especially when there are suggestions of dissensus within the scientific community itself, the ideal of "scientific objectivity" is likely to be called into question. Who deserves to be called an "expert"—and why—may begin to appear very much a context-dependent matter.

If Ezrahi and Nelkin provide a glimpse of some of the disturbing implications that such oft-cited phenomena as disputes among scientists may have for the conventional view of "experts" and "expertise", that potential is fully exploited in the admittedly "iconoclastic" article by Ben L. Martin that closes our Part III. Whereas Ezrahi and Nelkin might grant that there are instances in which the special knowledge or training of scientists and others gives them a *prima facie* claim to speak with finality on a given issue, Martin quite self-consciously sets out to defend the thoroughly "relativist" proposition that in *all* cases and circumstances, the purported "expert" is an "authority" on a subject "especially and specifically because he enjoys *acceptance* as such" (p. 159), and for no other reason.

In the introductory phases of "Experts in Policy Processes", Martin argues that Western culture in general, and American culture in particular, has resisted this view of experts and expertise because it remains under the spell of what he terms "the empiricist tradition" in philosophy. The hallmark of the empiricist tradition is the notion that *"truth is manifest"*, "knowable through induction and reliance upon the senses" (pp. 151–2); it holds forth, in Martin's words, "an optimistic view of the possibility of grasping reality" (that is, the "essence" or "true nature" of a thing) and "produced the notion of the expert . . . as a man with access to the blueprints of reality, 'dis-covering' the laws of the cosmos and society" (pp. 152).

Martin's claim is that one the empiricist ideal of "objective knowledge" is rejected—and many if not most students of philosophy today do in fact repudiate the tenets of the tradition he attacks[1]—it no longer makes sense to view the "expert" as one whose knowledge-claims can be validated "regardless of setting", without reference to the appraisal of others (p. 151). Instead, "expertness" ought to be seen entirely as an ascribed quality, a status or role assigned to certain individuals who are thought by society (or a segment of society) to possess some valuable trait or skill.

In the latter portion of his article, Martin explores some of the political implications of this "relativistic" conception of "expertise." In any given situation, he suggests, there exists a pool of "candidate-experts" whose members may be "recruited" to inform decision-making. Who is selected from this pool—and what the content of their role will be—invariably depends on the client, "the client's image of the subject matter" (p. 160), and, perhaps most critically (at least in Martin's view), "sensitivity to partisanship" (p. 163). Citing a variety of examples of client-adviser relationships in which "one can find the thread of partisanship guiding . . . ascriptions of expertness," Martin concludes—as Nelkin and Ezrahi also suggest—that clients often "use experts as persuasive instruments, borrowing authority and prestige for their ideas and perspectives" (p. 167).

Scientists and others are inclined to worry that the partisan recruitment and use of experts threatens to undermine what they regard as a generally healthy respect for their authority. But Martin plainly does not share that concern. On the contrary, his concluding statement suggests that public awareness of the pervasiveness of these practices may serve a valuable educational purpose. "If the mediation of intellectual constructs is inevitable in decision-making"—that is, if it is inherently impossible to judge matters from an "objective," "apolitical" vantage-point—"the choice and application of ideologies can at least be [made] voluntary" (p. 173). We would be better off, in other words, if we disabused ourselves of the notion that by relying on "experts" we can really give our postures or positions a legitimacy they might otherwise lack. By frankly acknowledging, both to ourselves and to others, that "political" or "ideological" considerations weigh uppermost in our judgments on matters (even seemingly "technical" matters) of public concern, Martin seems to be suggesting, we would best promote the cause of honest and truly deliberative democratic discourse.

---

[1] For a recent and quite influential critique of the entire Western philosophical tradition along the lines sketched out by Martin, see Richard Rorty's *Philosophy and the Mirror of Nature* (Princeton: Princeton University Press, 1979).

THE DECLINE OF POLITICS AND IDEOLOGY IN A KNOWLEDGEABLE SOCIETY

ROBERT E. LANE

Yale University

From reports on the growth and changing character of contemporary knowledge, it is argued that we live in a "knowledgeable society" with certain epistemological characteristics, among which are the development of more fruitful categories of thought, increased differentiation of ego from inner and outer worlds, an imagination of situations contrary to fact, reflective abstraction, changing truth criteria, and a changed philosophy of knowledge. This increase in knowledge and change in thoughtways lead to changes in policy-making procedures. There is increased application of scientific criteria for policy determination at the expense of the usual short-term political criteria and ideological thinking as well. In this situation, social knowledge is creating its own attitudinal disequilibrium.

It has been a common thing to speak of a "democratic society," and recently of an "affluent society." Could one, in some analogous sense, speak of a "knowledgeable society," or perhaps historically of an "age of knowledge"? Good scholars are likely to be so aware of what they do not know to regard the term as pretentious, yet they are familiar with, and perhaps accept the implications of conventional statements on the "scientific age." The purpose of this piece is to explore the concept of a knowledgeable society, and to examine some of its political implications.

The strands of thinking which may be woven into such a conceptual fabric are many and varied and curiously isolated from one another. There are, in the first place, certain early sociological and anthropological thinkers, each with a somewhat different interpretation of the stages of development of knowledge.[1] The Marxian dialectic offers a further developmental analysis, in the tradition of the sociology of knowledge.[2] Students of social change,[3] historians of science,[4] and philosophers of science[5] add to the picture. Knowledge is cognition—psychologists dealing with cognitive processes and concept formation and thinking illuminate the microprocesses given greater emphasis in an age of knowledge.[6] Even economists have recently dealt with knowledge.[7] The organization and professionalization of knowledge is analyzed in the works of contemporary sociologists.[8] The relationship of science to government has many contemporary students, some of them gathered together in a recent collection.[9] Finally, the current controversies dealing with the "end of ideology" on the one hand, and the place of the intellectual in modern society on the other, bear on the matter.[10] Obviously the scope of the problem is large, the complexity great, and the various treatments disparate. Nevertheless in discussing the concept of a knowledgeable society, we have the help of many others.

---

[1] See Auguste Comte, *A General View of Positivism*, trans. by J. H. Bridges, Stanford: Academic Reprints, 1958; and comments in Howard Becker and Harry Elmer Barnes, *Social Thought from Lore to Science*, New York: Dover, 1961, 3rd ed., Vol. 2, pp. 573–74; Emile Durkheim and Marcel Mauss, *Primitive Classification*, trans. by Rodney Needham, Chicago: University of Chicago Press, 1963; Bronislaw Malinowski, *Magic, Science and Religion*, Garden City, N.Y.: Doubleday Anchor, 1955; A. L. Kroeber, *Configurations of Culture Growth*, Berkeley: University of California Press, 1944.

[2] See Marx's discussion of consciousness and of ideas as superstructures in his "Economic and Philosophical Manuscripts" and "German Ideology," in *Marx's Concept of Man*, edited by Erich Fromm, New York: Ungar, 1961; Karl Mannheim, *Ideology and Utopia*, trans. by Louis Wirth and Edward Shils, London: Routledge & Kegan Paul, 1949; Robert K. Merton, *Social Theory and Social Structure*, New York: Free Press of Glencoe, 1957, rev. ed., Parts 3 and 4.

[3] See, especially, "Social Evolution Reconsidered" (1950), in *William F. Ogburn On Culture and Social Change*, edited by Otis D. Duncan, Chicago: University of Chicago Press, 1964.

[4] Derek J. de Sola Price, *Science Since Babylon*, New Haven: Yale University Press, 1961; Herbert Butterfield, *The Origins of Modern Science*, New York: Macmillan, 1961, rev. ed.

[5] Alfred N. Whitehead, *Science and the Modern World*, Cambridge: Cambridge University Press, 1933; Carl G. Hempel, *Fundamentals of Concept Formation*, Chicago: University of Chicago Press, 1952, Vol. 2, No. 7 of the *International Encyclopedia of Unified Science;* Hans Reichenbach, *The Rise of Scientific Philosophy*, Berkeley: University of California Press, 1953.

[6] For a general overview, see D. E. Berlyne, *Structure and Direction in Thinking*, New York: Wiley, 1965; a developmental (individual and social) view is presented in O. J. Harvey, D. E. Hunt, and H. M. Schroder, *Conceptual Systems and Personality Organization*, New York: Wiley, 1961; I have found Milton Rokeach's work especially helpful (*The Open and Closed Mind*, New York: Basic Books, 1960.)

[7] Fritz Machlup, *The Production and Distribution of Knowledge in the United States*, Princeton: Princeton University Press, 1962.

"Knowledge," of course is a broad term and I mean to use it broadly. It includes both "the known" and "the state of knowing." [11] Thus a knowledgeable society would be one where there is much knowledge, and where many people go about the business of knowing in a proper fashion. As a first approximation to a definition, the knowledgeable society is one in which, more than in other societies, its members: (a) inquire into the basis of their beliefs about man, nature, and society; (b) are guided (perhaps unconsciously) by objective standards of veridical truth, and, at the upper levels of education, follow scientific rules of evidence and inference in inquiry; (c) devote considerable resources to this inquiry and thus have a large store of knowledge; (d) collect, organize, and interpret their knowledge in a constant effort to extract further meaning from it for the purposes at hand; (e) employ this knowledge to illuminate (and perhaps modify) their values and goals as well as to advance them. Just as the "democratic society" has a foundation in governmental and interpersonal relations, and "the affluent society" a foundation in economics, so the knowledgeable society has its roots in epistemology and the logic of inquiry.

In order to support such an epistemological effort, a society must be open, i.e., free discussion must be allowed on every topic, with the outer limit posed not by threats of social change, but by concern for survival as a society. It must be stable enough to maintain the order necessary for the process of inquiry, trusting enough to encourage cooperative effort and acceptance of each other's "findings," [12] rich enough to educate its population in the modes of inquiry, dissatisfied or curious enough to want to know more.

Obviously this definition and these conditions raise more questions than can be answered easily: Who are these paragons? What power have they? What standards of knowledge qualify a man or a group or a society? How shall we deal with mystical and religious knowledge? With poetical and artistic knowledge? What about the basis for the epistemology itself—is not this the crudest act of faith? This is only an approximate definition of a model of a "knowledgeable society." The elements are present in some degree in every society; in the knowledgeable society they are present to the greatest degree.

COMMENTS ON THE DEVELOPMENT OF THE KNOWLEDGEABLE SOCIETY

I have defined the knowledgeable society in terms of an epistemology, a search for and quantity of knowledge, and a pattern of use of that knowledge. Any effort to examine in detail how such a society developed leads into theories of social change. Let us briefly consider the problem of development, partly as a means of showing, for contrast, what the knowledgeable society is not.

---

[8] Bernard Barber, *Science and the Social Order*, Glencoe, Ill.: Free Press, 1952; Florian Znaniecki, *The Social Role of the Man of Knowledge*, New York: Columbia University Press, 1940; Everett C. Hughes, *Men and their Work*, Glencoe, Ill.: Free Press, 1958; T. H. Marshall, *Class Citizenship and Social Development*, especially Ch. VI, Garden City, N.Y.: Doubleday, 1964; H. L. Wilensky, "The Professionalization of Everyone?" *American Journal of Sociology*, 70 (1964), pp. 137–58.

[9] Robert Gilpin and Christopher Wright, (eds.) *Scientists and National Policy Making*, New York: Columbia University Press, 1964; Don K. Price, *Government and Science*, New York: New York University Press, 1954.

[10] Daniel Bell, "The End of Ideology in the West" in *The End of Ideology*, Glencoe, Ill.: Free Press, 1960; Edward Shils, "The End of Ideology?," *Encounter*, 5 (November, 1955), pp. 52–58; S. M. Lipset, "The End of Ideology?," in *Political Man*, Garden City, N.Y.: Doubleday, 1960.

[11] Machlup, *op. cit.*, p. 13.

[12] Ithiel Pool holds that acceptance of statements on grounds other than the status of the source implies "an unusually high degree of mutual trust in interpersonal relations." See "The Mass Media and Politics in the Modernization Process," in Lucien Pye, ed., *Communications and Political Development*, Princeton: Princeton University Press, 1963, p. 242.

The growth and communication of experience. Malinowski gives us a starting point: "every primitive community is in possession of a considerable store of knowledge, based on experience and fashioned by reason." [13] He is here arguing against the view that primitive men are irrational and mystical; his argument takes the form of identifying the "profane" domain of life where cause and effect and the effectiveness of skill and reason are clearly perceived, in short, where "scientific thought" is dominant. Wherever the relationships between action and results are obscure, magic and religion have their domain, but the two may be kept separate and the latter need not dominate the former.[14] The knowledgeable society develops by an extension of an understanding of the cause-and-effect relationships in everyday experience and the withering away of the supernatural. Two further points can be made. Following Sumner part way,[15] we propose that philosophy and ethics, and perhaps a kind of low-level science, come to be the generalized and rationalized versions of everyday experience. To some extent they are emergent from and dependent upon the codification of everyday experience. The interaction between science, or at least technology, and philosophy and religion, is an interaction between experience of the mass and the reflections of the elite. In making experience conscious, modern society may, quite apart from the apparatus of science, increase the knowledge applied to everyday acts and decrease their magical and religious components. When experience is "lost" or when it is unconscious and traditional, the opportunities to build a knowledgeable society at this level are wasted.

The growth of culture and science. In *Configurations of Culture Growth,*[16] Kroeber employs the notion of episodic cultural "flowering," of typical phases of early growth, pause, maturity, perhaps revival, and then exhaustion. The reference is to all phases of culture—art, music, architecture and philosophy, as well as science and invention. The flowering of one field of culture is, according to Kroeber, likely to be accompanied by the flowering of others, but there are no necessary associations. "Flowering" and "growth," identified by historical recognition of excellence, represent the selection of a dominant "pattern," reinforced (or at least not opposed) by other social forces or areas, and selectively exploited until the possibilities of this particular specialized way of looking at things is exhausted, ending either in repetition or in rebellion, perhaps with an occasional revival.[17] Science, it seems, is no different. "It is . . . specifically to be noted that continuity is a proper quality only of the results of scientific activity. The activity itself is discontinuous." [18] Following Kroeber's line of thought on general culture growth, a knowledgeable society might be in a period of special "flowering," to be followed by decline; there have been many such in history. But since we are interested in exactly those areas of knowledge that are cumulative (and we have reason to believe that the policy sciences are beginning to show this property) we consider a trend to be more reasonable than a cycle.

The concept of growth in knowledge together with irregular scientific "flowering" is partly at odds with Ogburn's concept of an exponential growth of invention, due to the ever-expanding number of "elements" in a culture. Invention, for Ogburn, is the recombination of these elements into a new pattern. Since the invention of printing, "elements" are unlikely to be lost, and the communication process promotes increased diffusion; the possibility of new combinations is cumulative, and restrained only by friction, lag and disturbance. Ogburn seems less concerned with the "quality" of the invention, a property central to Kroeber's ideas, than with the quantity and the accelerated rate of innovations of all kinds, and with their capacity for creating social change and social strain.[19] Thus, the knowledgeable society is a strained society.

---

[13] Malinowski, *op. cit.*, p. 26.

[14] *Ibid.*, pp. 26–36.

[15] William Graham Sumner, *Folkways*, Boston: Ginn & Co., 1940 ed., pp. 1–74.

[16] See footnote (1) above. For a cyclical theory of cultural change, see P. A. Sorokin, *Social and Cultural Dynamics*, New York: American Book, 1937, 1941, Vols. I–IV.

[17] Kroeber, *op. cit.*, pp. 762–777.

[18] *Ibid.*, p. 204.

[19] Ogburn, *op. cit.*, pp. 17–32.

Derek Price has made some relevant observations, by considering the numbers of scientific journals, abstracts in specific sciences, and trained scientists, over time. Price finds something similar to Ogburn's cumulative curve. He says:

> It must be recognized that the growth of science is something very much more active, much vaster in its problems, than any other sort of growth happening in the world today. For one thing, it has been going on for a longer time and more steadily than most other things. More important, it is growing much more rapidly than anything else. All other things in population, economics, nonscientific culture, are growing so as to double in roughly every human generation or say thirty to fifty years. Science in America is growing so as to double in only ten years. . . . If you care to regard it this way, the density of science in our culture is quadrupling during each generation.[20]

But Price is explicit about the inhibition of science. Clearly such exponential growth cannot continue: "To go beyond the bounds of absurdity, another couple of centuries of normal growth of science would give us dozens of scientists per man, woman, child and dog of world population."[21]

According to Price, the curve of development is logistic in shape and we are nearing the point of deceleration of growth. One could then, at least on this quantitative basis, differentiate perhaps three periods: (1) accelerating rate of growth; (2) some intermediate constant rate; and (3) decelerating rate of growth. But this is still quantitative growth; it is at least possible that Kroeber's comments on "bursts" of greatness or eminence or quality apply. Nevertheless Kroeber's view that "there is no clear evidence of a tendency toward acceleration of growth as we pass from ancient to modern times"[22] seems to require reservations with respect to science.

Machlup is concerned with yet another kind of relevant data: the increase in personnel and expenditure on knowledge, and the implicit evaluation of worth by society relative to other pursuits. Machlup's concept of "the knowledge industry" is much broader than the focus on "science" or even "culture"; it includes those who distribute knowledge (in education and teaching) as well as those who produce it. He finds that knowledge-producing occupations have grown much more rapidly than others and that the salaries of knowledge-producers and distributors, relative to other occupations with equal education, have tended to increase. He also observes a changing emphasis within the knowledge industry: "While in the first part of this century growth was fastest in the clerical occupations, the lead was then taken by managerial and executive occupations, and more recently by professional and technical personnel."[23] The knowledgeable society encourages and rewards the "men of knowledge," compared to the "men of affairs."

The United States has been slow to recognize the importance of scientific knowledge, partly, as Machlup says, because of the "American idiosyncrasy in favor of the immediately practical and against the general-theoretical."[24] Although, in some ways, science grows out of technology,[25] it is often the other way around; even in technology the United States in the 19th Century tended to lag behind Europe.[26]

This is no longer so. Consider the following data on current American expansion in the sciences: (1) From 1940 to 1957, Federal government expenditures for research and development (excluding military pay) increased from $74 million to $2,835 million; from 1953 to 1963, total Federal expenditures for these purposes increased from $3 billion to $10 billion.[27] (2) In the ten-year period from 1953 to 1963, expenditures for research and development by colleges and universities increased from $420 million to $1,700 million.[28] (3) In the seven-year period

---

Price, *op. cit.*, pp. 107–108.

[21] *Ibid.*, p. 113.

[22] Kroeber, *op. cit.*, p. 842.

[23] Machlup, *op. cit.*, p. 396.

[24] *Ibid.*, p. 202.

[25] See Derek Price's discussion of "Renaissance Roots of Yankee Ingenuity" in his *Science Since Babylon*, pp. 45–67.

[26] Eugene Ayres, "Social Attitudes Toward Invention," *American Scientist*, 43 (1955), pp. 533–535; quoted in Machlup, p. 202.

[27] U. S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957* and *Continuation to 1962 and Revisions*, Washington, D.C.: U. S. Government Printing Office, 1960 and 1965, Series W 79, 80.

[28] U. S. Bureau of the Census, *Statistical Abstract*

from 1957 to 1964, the number of Ph.D.'s conferred annually increased as follows: [29] from 1,634 to 2,320 in the Life sciences; from 2,535 to 4,980 in the Physical sciences; and from 1,824 to 2,860 in the Social sciences. (4) In the period from 1950 to 1964 books in "science" published annually in the United States increased from 705 to 2,738 and books in "sociology, economics" from 515 to 3,272.[30]

The prodigious and increasing resources poured into research, the large and increasing numbers of trained people working on various natural and social "problems," and the expanding productivity resulting from this work, is, at least in size, a new factor in social, and (I shall argue) in political life. This "second scientific revolution," as it is sometimes called, reflects both a new appreciation of the role of scientific knowledge, and a new merger of western organizational and scientific skills.[31]

The knowledgeable society is a modern phenomenon; it has inherited a body of knowledge of man, nature, and society which has been continually created and reversed throughout a history of accelerating attention to systematic investigation, despite the irregularity of the production of "great" ideas. The scientific and technological component of culture is different from all other cultural products. Unlike "schools of art and literature," or religious movements, or political movements with their regional incidence, it is a world-wide phenomenon, with demonstrable success in implementing whatever purposes it is applied to.[32] All countries are devoting increasing resources to scientific investigation, and part of this increase is in the social or behavioral sciences. Like the rest of the "knowledge industry," to use Machlup's term, the social knowledge industry is expanding.

THE THOUGHTWAYS OF A KNOWLEDGEABLE SOCIETY

The knowledgeable society is characterized by a relative emphasis upon certain ways of thinking, a certain epistemology, or, at the very least, a certain knowledge about knowledge. Is this epistemological skill more characteristic of modern, particularly American, society, than of societies in previous periods? It is possible to speak of the development of epistemology somewhat as one speaks of economic and political development, and to construct a sketch of the "thoughtways" of a knowledgeable society. The view that these qualities are more widely distributed today than ever before (one of the themes of Whitehead's *Science and the Modern World*), and are more thoroughly understood by a governing elite of professional and managers, is reinforced by research showing the impact of modern education upon thinking processes.[33]

Anthropomorphic and analogical thinking. The first stage of thought is labelling, i.e., assigning things to classes—a more complicated process than at first appears.[34] Durkheim and Mauss argue that primitive classification was first developed according to social categories: "the first classes of things were classes of men, into which things were integrated. It was because men were grouped, and thought of themselves in the form of groups, that in their ideas they grouped other things, and in the beginning the two modes of grouping were merged to the point of being indistinct." So, also, "the unity of knowledge is nothing else than the very unity of the collectivity. . . . Logical relations are thus, in a sense, domestic relations."[35] This view has been criticized in detail,[36] but the general point, seen in animistic thought everywhere, is valid: Men classified the unfamiliar in terms of the

---

for the United States: 1965, Washington, D.C.: 1965 p. 545.

[29] *Ibid.*, p. 551.

[30] *Ibid.*, p. 527.

[31] See John J. Beer and W. David Lewis, "Aspects of the Professionalization of Science," *Daedalus*, 92 (1963), No. 4, pp. 764–784.

[32] "The Reformation, for all its importance, may be considered as a domestic affair of the European races. . . . Modern science was born in Europe, but its home is the whole world." A. N. Whitehead, *Science and the Modern World*, pp. 3–4.

[33] See Harold Webster, Mervin B. Friedman, and Paul Heist, "Personality Changes in College Students," in Nevitt Sanford, ed., *The American College*, New York: Wiley, 1962, pp. 811–846.

[34] See the discussion of "concept attainment" in Jerome S. Bruner, Jacqueline J. Goodnow, and George A. Austin, *A Study of Thinking*, New York: Wiley, 1956.

[35] *Primitive Classification*, pp. 82–84.

[36] See Rodney Needham's excellent introduction to Durkheim and Mauss, *ibid.*, pp. vii–xlviii.

homely, familiar concepts developed in daily living. Even in the history of science the use of familiar analogies dominated thought. For example, it is said that Aristotle thought of causal effects in terms of a horse drawing a cart and that Galileo thought of heavenly bodies as something like ships moving in an ocean without friction.[37] The knowledgeable society is one where, by successive approximation, categories and classes of things move from the immediate, personal and familiar, to more abstract concepts with a better fit (more adequate to account for the properties of the phenomena observed).

Differentiation of ego from inner world and from environment. The more a person responds "unselfconsciously" to his inner moods and fantasy life, without conscious thought, or to the stimuli of his environment, reactively, the less able he is either to schedule his drives and maximize his purposes or to master and control the environment. Somehow he must be "separate" from his inner world and his outer world; he must have ego strength to think through his problems, synthesize his desires and control his behavior. These qualities are said to be lacking in primitive man, as they are demonstrably lacking in a child. Indeed as one ascends the phylogenetic scale, the separation of ego from inner and outer worlds becomes more and more marked.[38] This is sometimes mistaken for alienation; in reality it is a necessary element in thought and a necessary ingredient of the knowledgeable society.

Imagining situations contrary to fact or beyond experience. In *The Passing of Traditional Society,* Daniel Lerner reports on surveys where Middle Eastern subjects are asked to imagine themselves as editors of a newspaper, Governor or President of their society, or residents in a foreign country. Those with the most limited experience and the most parochial orientations cannot do this; they boggle at the very thought. Lerner refers to this imagination of the self in the place of another as "empathy."[39] A more general notion is the "assumption of a mental set willfully and consciously,"[40] as distinguished from a capacity to respond only in terms of a given and familiar state of affairs and an inability to manipulate concepts in the mind so as to reconstruct them in an ungiven manner.[40] Any society which relies upon widely distributed initiative, ambition, and innovation must encourage these qualities of imagination: men must think of themselves and of elements of their situation as other than they are.

Holding simultaneously in mind various aspects of a situation. Primitive and uneducated people can learn a task, a creed, a message, a set of conventions, but it takes special qualities to grasp the "essential" parts, to see how they are put together, to compare them, in short, to analyze them. Comparison and contrast implies holding and bringing together at least two things at once.[41] Rote learning, as in many traditional schools, does not develop these special analytical qualities; it only teaches parts, or, perhaps, sequences. To analyze is to question, and questioning is regarded as dangerous.

The reflective abstraction of common properties and the formation of hierarchic concepts. The capacity to compare and analyze, to disintegrate a whole, is usually paired with the more difficult task of integrating and organizing parts into a new pattern: analysis and synthesis. In the most primitive societies, the concept of abstract numbers, in contrast to concrete instances, is sometimes missing.[42] Once possessed of the idea of, say, "fiveness" it is not difficult to assign groups with five discrete elements to this class; but to invent, from a multiplicity of objects, the concept of "five" or any other abstract number is the act of genius. The knowledgeable society is not only endowed with a great variety of useful concepts, it actively encourages concept formation to create classes and relationships which give a better account of observable phenomena. The pre-knowledgeable society employs the concepts given by tradition.

---

[37] Stephen Toulmin, *Foresight and Understanding,* New York: Harper Torchbook, 1961, pp. 52, 54.

[38] This point and several following are derived from Harvey, Hunt, and Schroder's explication and development of Goldstein and Scheerer's concept of a concrete-abstractness dimension of thought, originating from studies of children and brain-damaged patients. See Harvey *et al., Conceptual Systems,* pp. 24–49.

[39] *The Passing of Traditional Society,* New York: Free Press of Glencoe, 1958, pp. 47–52.

[40] Harvey *et al., Conceptual Systems,* p. 29.

[41] See the discussion of "conceptualizing in political discourse," in my *Political Ideology,* New York: Free Press, 1962, pp. 346–363.

[42] H. Werner, *Comparative Psychology of Mental Development,* New York: International Universities Press, 1957 rev. ed. quoted in Harvey *et al., Conceptual Systems,* p. 33.

The qualities discussed above fall along a dimension of concreteness-abstraction. The concrete style of thinking is stimulus-bound, unreflective, unanalytic, unsynthetic, and unimaginative. Present in all societies, it is most evident in primitive societies. Harvey, Hunt and Schroder believe it has social consequences, which can be summarized in these ways: [43] Greater concreteness tends to be accompanied by absolutism, categorical thinking, and stereotyping; it is likely to be expressed in attribution of external causality and "oughtness" to rules; it disposes toward catechisms and word magic; it tends to be accompanied by negativism and resistance to suggestion; and it encourages ritualism. In these ways, as readers of *The Authoritarian Personality* and of *The Open and Closed Mind* will recognize, the dimension of concreteness-abstraction has political and social implications: concreteness is related to authoritarianism and to dogmatic, rigid, and opinionated thinking; [44] The democratic society in contrast is marked by abstract thinking.

Employment of objective truth criteria. A knowledgeable society is not only one where more people value knowledge, but one where knowledge is more likely to be valued if it can be shown to be true by certain objective criteria. In the words of Ithiel Pool:

> To evaluate assertions primarily by a criterion of objective truth is not a natural human way of doing things; it is one of the peculiar features of the Graeco-Roman-Western tradition. . . . The Western criterion of truth-value . . . assumes that a statement has a validity or lack of it inherent in itself and quite independent of who says it and why. . . . In most societies facts must be validated by an in-group authority before they can be considered credible.[45]

In most societies, statements are true according to whether the spokesman is powerful and likely to dominate others, whether he is "one of us," whether it is expressed with appropriate politeness, and so forth. Moreover, in Eastern philosophies a thing can both be true and not true at the same time; there is no rule of the undistributed middle.[46]

In a sense, this is a facet of a much larger problem analyzed by Rokeach: the processing of information according to its "intrinsic merits." In his discussion of the open and closed mind, he says that the "basic characteristic that defines the extent to which a person's system is open or closed," is "the extent to which the person can receive, evaluate, and act on relevant information received from the outside on its own intrinsic merits, unencumbered by irrelevant factors in the situation arising from within the person or from the outside." By irrelevant internal pressures Rokeach means unconscious intruding habits and poses, irrational power needs or needs for self-aggrandizement, the need to allay anxiety, to create an impression, and so forth. The irrelevant outside factors are attitudes of dislike towards the source, conformity pressures, the rewards and punishments implied by acceptance and rejection.[47] The knowledgeable society screens out more of the irrelevant internal and external factors for more people.

Tolerance of dissonance and ambiguity. The authoritarian personality, according to Else Frenkel-Brunswik, is intolerant of ambiguous stimuli; he needs quick, sharp resolution of his doubts. Thus he likes sharply defined art, quick (and usually easy) answers, people who are decisive.[48] The person with a closed mind, according to Rokeach, does not bring together conflicting elements of his belief system; rather he compartmentalizes them, linking them only through the authority of the dogma or the party line or the dominant spokesmen's view.[49] Similarly the capacity and inclination to hold simultaneously in view opinions or attitudes each of which "implies" the reverse of the other (a favored message from a hated source) varies greatly in the population, and generally forces various kinds of reconceptualization to reestablish a consonant emotional posture.[50] More than others, the members of a knowledgeable society are endowed with the capacity to tolerate ambiguity, conflict and dissonance.

---

[43] Harvey *et al.*, *ibid.*, pp. 36–46.

[44] T. W. Adorno, Else Frenkel-Brunswik, Daniel J. Levinson, and R. Nevitt Sanford, *The Authoritarian Personality*, New York: Harper, 1950; Rokeach, see footnote 6.

[45] "The Mass Media and Politics," p. 242.

[46] *Ibid.*, pp. 242–244.

[47] Rokeach, *The Open and Closed Mind*, p. 57.

[48] "Intolerance of Ambiguity as an Emotional and Perceptual Variable," *Journal of Personality*, 18 (1949), pp. 108–143.

[49] Rokeach, *The Open and Closed Mind*, pp. 67–97.

Changed views of metaphysics and religion. Comte, writing in the early nineteenth century, held that social progress was produced by a changing epistemology and metaphysics. Societies pass (necessarily, he thought) through certain stages marked by the dominance in modes of thought and emphasis first, of theology, then of metaphysics, and finally of science associated with industrial development, centrally planned and controlled with the help of extensive sociological studies which give the controllers knowledge of the laws of society.[51] The agency of change is, however, unclear; there is no epistemological dialectic. Yet one can at least accept the idea that in the knowledgeable society theological and metaphysical modes of thought shrink in contrast to scientific modes.

Within this framework, however, certain kinds of religious thought seem to have encouraged the growth of science. Merton expressed his position as follows: "It is the thesis of this study that the Puritan ethic, as an ideal-typical expression of the value-attitudes basic to ascetic Protestantism generally, so canalized the interests of seventeenth-century Englishmen as to constitute one important *element* in the enhanced cultivation of science. The religious *interests* of the day demanded in their forceful implications the systematic, rational, and empirical study of Nature for the glorification of God in His works and for the control of the corrupt world."[52] A knowledgeable society, then, emerges from and is reinforced by religious beliefs which, however framed, focus attention upon this world, and allow for or encourage a scientific epistemology. Today, scientists and professional people are much less likely to be religious or believe in God than businessmen, bankers, and lawyers.[53]

Changed philosophy of knowledge. The mind vs. matter problem appears in many guises: as the contrast between "words and things", as rationalism vs. empiricism, as idealism vs. nominalism, and so forth. Whitehead considered that the great difference between the modern scientific age and all other periods was the wedding together of speculative and theoretical modes of thought with empirical and systematic modes of investigation.[54] Similarly Reichenbach has argued that the rise of scientific philosophy is grounded in a shift from "*transcendental* conception of knowledge, according to which knowledge transcends the observable things and depends upon the use of other sources then sense perception," to a "*functional* conception of knowledge, which regards knowledge as an instrument of prediction and for which sense observation is the only admissible criterion of nonempty truth."[55] There are not two worlds, an ideal and a real one, but one integrated world of thought and experience. The knowledgeable society is marked by an increased acceptance of this view. "The opium of the intellectuals" is not so much, as Raymond Aron thinks, Marxism,[56] as it is philosophical idealism.

One facet of this changed concept of knowledge has been an emphasis upon operationalism, *i.e.*, the position that concepts are related, however indirectly, to the operations which measure them, and by intersubjective testability. Knowledge in the knowledgeable society must be public, its sources indicated and its conceptual boundaries marked by something other than incommunicable experience.

From symptomatology to taxonomy to explanation. The history of the biological and behavioral sciences, reveals a tendency first to report observations on phenomena (the naturalist and the journalist), then, with greater care, to group these observations into classes and syndromes, and then, with experimental or controlled observational techniques, to attempt to understand causal relationships, to explain why the phenomena change as they do. When this latter phase is successful, control is more feasible and social policy is likely to be more adequate to the situation. In the knowledgeable society, the intellectual emphasis is more likely to be upon laws of behavior, change, and control. Attaching metrics to phenomena often improves our understanding and our control. The knowledgeable society increasingly employs mathematical modes of expression and thought.

---

[50] I think the most useful short account of the general phenomenon of cognitive dissonance and cognitive balancing is in Milton Rosenberg and others, *Attitude Organization and Change*, New Haven: Yale University Press, 1960, pp. 112–163.

[51] *A General View of Positivism*, passim.

[52] *Social Theory and Social Structure*, pp. 574–575. (Merton's emphasis).

[53] S. M. Lipset, *Political Man*, p. 314.

[54] *Science and the Modern World*, p. 3.

[55] *The Rise of Scientific Philosophy*, p. 252.

[56] *The Opium of the Intellectuals*, trans. by T. Kilmartin, New York: Norton, 1957.

The contribution of the philosophy of science. We have been discussing the complex of attitudes and skills which equip men to deal realistically with the events which impinge upon their lives—policy-formation at the micro-level. This is related to but separate from the ways of thinking of scientists and philosophers who are interested in social policy. This history of social thought reveals the importance of analogical thinking: If geometry yields results from axiomatic methods, so should sociology; in an age of mechanics the model may be clockworks or hydraulics to some (cf. Freud); organismic theories dominate certain periods: a primitive set of anatomical analogies seems to have been prevalent in the Middle Ages.[57]

It seems to me that the emergence of a coherent philosophy of science or logic of inquiry represents a crucial change in this groping toward a method of studying society, particularly as it has matured and increased the scope given to imagination. If one goes back no further than the beginning of this century, with the rise of analytic philosophy in Austria (the Vienna Circle) and in England, the development of the "unified science" group at Chicago, perhaps the general systems theorists, and the widespread teaching of the philosophy of science today, a change in intellectual posture toward man and society so great as to represent a watershed is evident. Other knowledgeable societies, marked by Kroeber's "bursts" of culture growth, have not sustained their performance. I believe the development and widespread acceptance of the philosophy of science as a basis for social inquiry represents a "take-off" phenomenon in social science, promising sustained growth in social interpretation.

PROFESSIONALISM AND THE "PRE-FORMULATION" OF POLICY

The discussion so far has sought to illuminate the development of the knowledgeable society, and to show its characteristic thoughtways. Now we turn to the application of this knowledge to public policy. The people who make this application are, in the first instance the professionals, organized in their own associations, governing and staffing institutions devised to develop and teach the new knowledge and apply it to current problems. Within the professions there are tendencies to allocate responsibility for knowledge domains and hence responsibilities for working out "solutions" of social problems relevant to these domains. One aspect of professionalization is the establishment of standards of performance well above actual performance. The gap between the actual and the idea creates within the profession a kind of strain towards remedial action. The consciousness of meeting or failing to meet standards enlists professional ambition, reputation, credit and blame. Staff conferences, annual meetings, and new research studies set up strains for better performance, better instruments, better laws, and new agencies to meet the new standards. In the knowledgeable society, much policy is made first through professional intercourse, concerning what solutions to press upon the government and what men to advance to positions of influence as well as what standards to impose. In some ways this is only a change of venue for political maneuvering, but in an important sense it implies a change in criteria for decision-makers from immediate political advantage to something within the professionalized domain of knowledge.

KNOWLEDGE IS ENCROACHING ON POLITICS

If one thinks of a domain of "pure politics" where decisions are determined by calculations of influence, power, or electoral advantage, and a domain of "pure knowledge" where decisions are determined by calculations of how to implement agreed-upon values with rationality and efficiency, it appears to me that the political domain is shrinking and the knowledge domain is growing, in terms of criteria for decisions, kinds of counsel sought, evidence adduced, and nature of the "rationality" employed. Some of the evidence for this direction of change may be suggested in the following sampling of recent events:

---

[57] Karl Deutsch has an interesting discussion of these analogies in his *The Nerves of Government*, New York: Free Press, 1963, pp. 24–38; The anatomical metaphor in medieval political thought is most explicit in Otto Gierke, *Political Theories of the Middle Ages*, trans. by F. W. Maitland, Boston: Beacon, 1958.

(a) With due allowance for political slippage, there has been a gradual expansion of the civil service based on competitive examinations from 23 per cent of personnel employed in the executive branch of the Federal government in 1891, to 87 per cent in 1962.

(b) The General Accounting Office, established in 1921, and the General Service Administration, established in 1949, supervise government business operations so as to encourage economic rather than political criteria.

(c) The Council of Economic Advisers was set up in 1946, symbolizing the introduction of economic criteria into the monetary and fiscal operations of government.

(d) The professionalization of the attack on poverty is illustrated by the contrast between the methods and programs of the Works Progress Administration (1933) and the Office of Economic Opportunity (1965).

(e) The growing use of extra-governmental organizations, like Rand and the university research centers, to study social and technical problems and formulate policy proposals, introduces a variety of less political (if not value-free) criteria for policy-making.

(f) Similarly the growing employment of Presidential Commissions and Committees and White House Conferences changes the nature of the criteria employed in policy-making.

(g) An enlarged governmental apparatus has been created to enlist scientific advice on a variety of topics (not just what is coming to be called the management of "science affairs"), as seen in the President's Scientific Advisory Committee and the Office of Science and Technology, and even the Office of Science Adviser to the Secretary of State.[58]

Moreover, the dominant scholarly interpretation of policy-making processes has changed in the direction of emphasizing the greater autonomy of political leaders and legislators: with respect to the role of pressure groups,[59] the power elite,[60] and the electorate.[61] If leaders and other legislators are less bound by the domain of pure politics than we had thought. Then they are freer to be guided by the promptings of scientists and findings from the domain of knowledge.

Studies of the legislative process reinforce this view. A massive literature documents four relevant points: (a) the rising influence of the bureaucracy is based in large part on bureaucratic command over the sources of knowledge; (b) state and national legislators respond to the growing importance of technical knowledge both with increased standards for their own mastery of subject-matter fields and with demands for greater staff resources to help them meet the challenge; (c) there is an increased reliance on the kind of professional help enlisted by the executive; and (d) the power of the lobby is less likely to be based on electoral sanctions than upon specialized information helpful (however self-interested) in formulating policy change.[62]

---

[58] Much of this is reported and commented upon in Gilpin and Wright, *Scientists and National Policy Making*. Here (on page 109) one will find Wallace Sayre making the point that "politics is inescapable." Nothing in this section should be read as implying anything contrary to this maxim.

[59] E. E. Schattschneider, *Politics, Pressures and the Tariff*, New York: Prentice-Hall, 1935; Peter H. Odegard, *Pressure Politics*, New York, Columbia University Press, 1928; Raymond A. Bauer, Ithiel de S. Pool, and L. A. Dexter, *American Business and Public Policy*, New York: Atherton, 1963.

[60] Floyd Hunter, *Community Power Structure*, Chapel Hill: University of North Carolina Press, 1953; Robert A. Dahl, *Who Governs?*, New Haven: Yale University Press, 1961.

[61] "Toward a More Responsible Two-Party System," Supplement to *American Political Science Review*, XLIV (1950) No. 3; and, see, for example, Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," *American Political Science Review*, LVII (1963), pp. 45–56.

[62] See, for example, John Wahlke, Heinz Eulau and others, *The Legislative System*, New York: Wiley, 1962; James D. Barber, *The Lawmaker*, New Haven: Yale University Press, 1965; Bauer, Pool, and Dexter, *American Business and Public Policy;*

THE CHANGED APPROACH TO PROBLEMS

Of course there will always be politics; there will always be rationalized self-interest, mobilized by interest groups and articulated in political parties. But, if political criteria decline in importance relative to more universalistic scientific criteria, and if the professional problem-oriented scientists rather than laymen come to have more to say about social policy, the shift in perspective is likely to occasion some differences in policy itself. What would these be?

In the first place there is the question of the very *consciousness* of a problem. The man in the middle of the problem (sickness, poverty, waste and especially ignorance) often does not know there is anything problematic about his state. He may accept his condition as embodying the costs of living: if one accepts his lot in life, one accepts lesions, hunger, overwork and unemployment. For this reason such people are often hard to reach. As Harrington says, "First and foremost, any attempt to abolish poverty in the United States must seek to destroy the pessimism and fatalism that flourish in the other America," the America of the poor.[63] Often it takes years of dedicated agitation to make people aware that they live in the midst of a problem. The curious thing about modern times is the degree to which government itself undertakes to do what, in the past, has so often been the task of the agitator. The New Deal helped to organize labor, and the New Frontier and the Great Society help Negroes to demand more of society, and help organize the poor to pursue their own interests. Admittedly there are political benefits in these acts, and they can be attributed only in minor part to the growing insights into the nature of poverty and apathy. Yet consciousness of a problem may come *first* to the authorities, scientific and governmental. People may have to be told, not that they are miserable, but that the conditions of their lives are, in some sense, remediable.

Beyond consciousness is something else, the analysis of the nature of the trouble: its causes, and what should be done about it. Here the main point is the environmentalism of the authoritative scientific or governmental view, in contrast to the personalism of the man involved. The problem as it presents itself to these two attentive persons is in each case different. For the unemployed worker, his problem is to find a job; for the economist, the problem is to analyze the causes of unemployment and sometimes to suggest remedial action. To the worker the "cause" of his plight is that he was let go; to the economist, the cause of the worker's plight may be insufficiency of demand due to higher interest rates and a budgetary surplus. What is cause to the worker is to the economist only a symptom, so different are their perspectives.

The view of a problem by scientific or governmental authorities is very often an analysis of the environment in which it occurs; the causes for the scientist are the factors which make the "problem" for the individual. In consequence, the political demands of the affected group and the demands of the professionals interested in the group's condition, may lead in different directions.

For people within a system (hospital, market, watershed, communication network), the boundaries and budgets seem fixed; they bargain for limited resources and more for them seems necessarily to imply less for someone else. An authoritative overview can change that perspective by introducing the possibility not of reallocating limited values but of generating an increase in values. Thus an economist today considers the problems of equity and efficiency in distribution in conjunction with the problem of growth. For the medical sociologist, the problem of the distribution of hospital facilities is paired with the problem of more and better facilities and better health; he is unsatisfied simply with a redistribution of untreated illness in a more equitable fashion. Political scientists have failed to understand this point, because their attention to "the authoritative allocation of values" has tended to obscure another facet of government: the generation of values.

Robert L. Peabody and Nelson W. Polsby, eds., *New Perspectives on the House of Representatives*, Chicago: Rand McNally, 1963; Donald R. Matthews, *U. S. Senators and Their World*, New York: Random House Vintage, 1960.

[63] Michael Harrington, *The Other America*, New York: Macmillan, 1963, p. 163.

KNOWLEDGE IS ENCROACHING ON IDEOLOGY

If we employ the term "ideology" to mean a comprehensive, passionately believed, self-activating view of society, usually organized as a social movement, rather than a latent half-conscious belief system,[64] it makes sense to think of a domain of knowledge distinguishable from a domain of ideology, despite the extent to which they may overlap. Since knowledge and ideology serve somewhat as functional equivalents in orienting a person toward the problems he must face and the policies he must select, the growth of the domain of knowledge causes it to impinge on the domain of ideology.

Silvan Tomkins has developed a theory of a basic ideological left-right dimension in virtually all domains of life, turning on the questions, "Is man the measure, an end in himself, an active, creative, thinking, desiring, loving, force in nature? Or must man realize himself, attain his full stature only through struggle toward, participation in, conformity to, a norm, a measure, an ideal essence basically independent of man." [65] He believes that arguments along these lines develop in passionate forms (in philosophy, mathematics, jurisprudence, etc., as well as in politics) wherever men are least certain of their ground. These arguments thrive on uncertainty and ignorance. "When the same ideas [that men have been arguing over in these ideological terms] are firmly established and incorporated into the fabric of a science or tested and found wanting, they cease to constitute an ideology in the sense in which we are using the term. At the growing edge of the frontier of all sciences there necessarily is a maximum of uncertainty, and what is lacking in evidence is filled by passion and faith and by hatred and scorn for the disbelievers. Science will never be free of ideology, though yesterday's ideology is today's fact or fiction." [66] The theory, then, is of an "ideo-affective" orientation toward the world directed towards subjects about which there is doubt. If the doubt is clarified by knowledge, this ideological orientation moves on to some other marginal and uncertain area. Increasing knowledge about man, nature, and society can be said to reduce the target area for ideological thinking.

A second way in which the characteristics of a knowledgeable society may be thought to reduce ideological thinking, is through the reduction of dogmatic thinking. Following Rokeach, we may conceive of dogmatic thinking as a selection and interpretation of information so as to reinforce a previously established creed, dogma, or political ideology. Information is used, not so much to understand the world as it really is, but as a means of defending against conflict and uncertainty.[67] The knowledgeable society is marked by a relatively greater stress on the use of information veridically, relying on its truth value and not on any adventitious defense, popularity, or reinforcement value. This should be associated with a decline in dogmatic thinking. The decline of dogmatism implies the decline of ideology, in the narrower sense of the term used here.

In the third place, consider the way in which knowledge may limit Mannheim's thesis that political thinking is inevitably biased. He says "all knowledge which is either political or which involves a world view is inevitably partisan"; and later "at the point where what is properly political begins, the evaluative element cannot easily be separated out," and still later, "the peculiar nature of political knowledge, as contrasted to the 'exact' sciences, arises out of the inseparability, in this realm, of knowledge from interest and motivation." [68] Mannheim has in view only the thinking of those who are themselves engaged in political strife; he does not envisage the possibility of such studies as *The Legislative System* and *The American Voter*,[69] which, although evaluative in many ways, nevertheless narrow the range of partisan, irrational and evaluative thought. Granting that interested parties form their ideas about politics into ideological constructs it seems likely that knowledge may constrict the scope of their ideology.

---

[64] See my *Political Ideology*, pp. 13–16; also footnote 10 above. Of course there are many definitions of ideology referring to a wider range of "mental products."

[65] "Left and Right: A Basic Dimension of Ideology and Personality," in Robert W. White, ed., *The Study of Lives*, New York: Atherton, 1963, pp. 391–392.

[66] *Ibid.*, p. 389.

[67] See Footnote 49 above.

[68] "The Prospects of Scientific Politics," in *Ideology and Utopia*, pp. 132, 168, 170.

[69] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter*, New York: Wiley, 1960.

This narrowing effect was, in fact, experienced by the participants at the conference of the Congress for Cultural Freedom in Milan in 1955 out of which the theme of "the end of ideology" developed. These scholars and scientists came expecting, indeed inviting, a great confrontation of world views. Under the pressure of economic and social knowledge, a growing body of research, and the codified experience of society, ideological argument tended to give way to technical argument, apparently to the disappointment of some.[70] The debate remained evaluative and partisan, but the domain of ideology was shrunken by the dominance of knowledge.

KNOWLEDGE AS DISEQUILIBRIUM

What happens when the scientific apparatus of the knowledgeable society produces some important findings: existential, causal, remedial, or whatever? Here are some examples from the social sciences:

> Among the nations of the world, the United States ranked 16th in rate of infant mortality in 1961.[71]
>
> "To raise every individual and family in the nation now below a subsistence income to the subsistence level would cost about $10 billion a year. This is less than 2 per cent of the gross national product. It is less than 10 per cent of tax revenues. It is about one-fifth of the cost of national defense." [72]
>
> "The reinforcing experience for convicted criminals while in jail results in high rates of recidivism: about three-fourths of those entering jail have been there before. And the younger the person at the time of first offense, the higher the rate of recidivism and the sooner it occurs." [73]
>
> "Today, more American school children die of cancer than from any other disease. So serious has this situation become that Boston has established the first hospital in the United States devoted exclusively to the treatment of children with cancer. . . . One of the earliest pesticides associated with cancer is arsenic. . . . In the United States the arsenic-drenched soils of tobacco plantations, of many orchards in the Northwest, and of blueberry lands in the East easily lead to pollution of water supplies." [74]
>
> "The more an individual engages in personal interaction with persons of different race, religious, or national background, the lower is his general level of prejudice. This result holds not only for majority group prejudices but also for minority prejudices against the majority group and other minorities. It is true of youths as well as their elders. It has been confirmed in 14 different samples, involving about 6000 persons." [75]

Such knowledge—discovered, organized, and communicated by professional men—creates a pressure for policy change with a force all its own. Knowledge (and what is regarded as knowledge) is pressure even without pressure groups, and without reference to an articulated forensic ideology. If the reader of these statements experiences some kind of policy-oriented speculation, so, I believe, do policy-making officials. The source of the tension is not difficult to discover. In skeleton form, the sequence may be as follows:

(a) A state of affairs is presented, conveying new or more precise information than that previously known (infants and children are dying at a "high" rate; poverty could be eliminated).

(b) A value is engaged (early death is bad; poverty is worse than prosperity).

(c) In some cases the information applies to particular groups whose needs and values are especially significant to an observer (Northwest apple growers, Southern Negroes and Whites, delinquent youth).

(d) Remedial action may be suggested (subsidies to the poor, policing of pesticides, enforced integration).

(e) Social, economic, and political costs are implied (taxation for subsidies to the poor, expensive re-education for prisoners, the opposition of tobacco growers).

---

[70] See footnote 10 above.

[71] *United Nations Statistical Yearbook, 1962*, New York: United Nations, 1963, p. 50.

[72] James N. Morgan *et al.*, *Income and Welfare in the United States*, New York: McGraw-Hill, 1962, pp. 3–4.

[73] Bernard Berelson and Gary Steiner, *Human Behavior*, New York: Harcourt, Brace, 1964, p. 630.

[74] Rachel Carson, *Silent Spring*, Boston: Houghton Mifflin, 1962, pp. 221–223.

[75] Robin M. Williams, quoted in Berelson and Steiner, op. cit., p. 519.

(f) Certain "pre-political ideological" positions on man and society are enlisted (Can man control his own fate? Is poverty "necessary"? Is human nature a constant?).

(g) Certain ideological postures toward the business of government are enlisted (Is every increment of government a bad policy in itself? Is government too corrupt an instrument to employ in changing conditions? Are tax dollars better spent by private organizations and individuals?).

Let us suppose, as seems likely, that knowledge like that presented above sets up a kind of "disequilibrium" in a person's mind. The restoration of equilibrium, then, is the problem-solving process, perhaps by questioning the data or the source, perhaps by changing one's own priorities of action, perhaps by selective inattention, perhaps by delegation (real or symbolic), perhaps by purely expressive as opposed to instrumental behavior, perhaps by rationalization, perhaps by scapegoating, perhaps by advocating simplistic solutions, and so forth.[76] But the point is that knowledge, with little more, often sets up a disequilibrium or pressure which requires compensating thought or action.

SUMMARY

In this article I have tried, first, to develop the idea of a knowledgeable society, with special attention to questions of growth and epistemology. Then, assuming that the concept applies to modern American society, I have suggested that the professionals and their associations have a role in the preformulation of policy, not all good, but generally responsive to the needs of society. Further, in comparing two "pure" domains, that of politics and that of knowledge, I have suggested that the criteria and scope of politics are shrinking while those of knowledge are growing. This has created a difference in perspectives of policy-makers: a different kind of consciousness, an environmentalist approach, and a concept of the generation of values. Like politics, ideology is declining as a *necessary* ingredient in change, partly because, given present values, knowledge sets up a powerful kind of attitudinal disequilibrium all its own.

---

[76] Dan Berlyne's *Structure and Direction in Thinking* illuminates the processes of "problematicity" and problem-solving, especially pp. 236–293.

Social Studies of Science, 5 (1975), 35-54

The Political Impact of Technical Expertise

Dorothy Nelkin

Technologies of speed and power – airports, power generating facilities, highways, dams – are often a focus of bitter opposition. As these technologies become increasingly controversial, scientists, whose expertise forms the basis of technical decisions, find themselves involved in public disputes. This 'public' role of science has generated concern both within the profession and beyond; for a scientist's involvement in controversial issues may violate the norms of scientific research, but have considerable impact on the political process. As scientists are called upon to address a wider range of controversial policy questions,[1] 'problems of political choice [may] become buried in debate among experts over highly technical alternatives'.[2]

This paper will discuss some of the implications of the increasing involvement of scientists in controversial areas. What is the role of experts in public disputes? How are they used by various parties to a controversy, and how do scientists behave once involved? Finally, what is their impact on the political dynamics of such disputes?

Author's address: Department of Urban Planning and Development, Cornell University, 614 Clark Hall, Ithaca, New York 14850, USA.

[1] See discussion of the increased demands for expert decision-making in Garry Brewer, *Politicians, Bureaucrats and the Consultant* (New York: Basic Books, 1973). Also, Dean Schooler, Jr., (in *Science, Scientists and Public Policy* [London and New York: The Free Press, 1971]) suggests that in the past, scientific influence has concentrated in government entrepreneurial areas such as space exploration, or in policy areas defined in terms of national security. The participation and influence of scientists has traditionally been rather minimal in policy areas with redistributive implications, e.g. social policy, transportation, and other issues subject to social conflict and competing political interests. As the public seeks technical solutions to social problems, and as scientists themselves become engaged in controversial public issues, this pattern is changing.

[2] Harvey Brooks, 'Scientific Concepts and Cultural Change', *Daedalus*, 94 (Winter 1965), 68.

THE ROLE OF EXPERTS

Scientists play an ambivalent role in controversial policy areas. They are both indispensible and suspect. Their technical knowledge is widely regarded as a source of power.

> The capacity of science to authorize and certify facts and pictures of reality [is] a potent source of political influence.[3]

Yet experts are resented and feared. While the reliance on experts is growing, we see a revival of Jacksonian hostility toward expertise, and of the belief that common sense is an adequate substitute for technical knowledge.[4]

The authority of expertise rests on assumptions about scientific rationality; interpretations and predictions made by scientists are judged to be rational because they are based on 'objective' data gathered through rational procedures, and evaluated by the scientific community through a rigorous control process. Science, therefore, is widely regarded as a means by which to de-politicize public issues. The increasing use of expertise is often associated with the 'end of ideology'; politics, it is claimed, will become less important as scientists are able to define constraints and provide rational policy choices.[5]

Policy makers find that it is efficient and comfortable to define decisions as technical rather than political. Technical decisions are made by defining objectives, considering available knowledge, and analyzing the most effective ways of reaching these objectives. Debate over technical alternatives need not weigh conflicting interests, but only the relative effectiveness of various approaches for resolving an immediate problem. Thus, scientific knowledge is used as a 'rational' basis for substantive planning, and as a means of defending the legitimacy of specific decisions. Indeed, the viability of bureaucracies depend so much on the control and monopoly of knowledge in a specific area, that this may become a dominant objective.[6] Recent technological disputes, however, suggest that access to knowledge and expertise has itself become a source of conflict, as various groups realize its growing implications for political choice.

---

[3] Yaron Ezrahi, 'The Political Resources of American Science', *Science Studies*, 1 (1971), 121. See also Don K. Price, *Government and Science* (New York: New York University Press, 1954).

[4] For a discussion of the historical tradition of resentment of experts in the United States see Richard Hofstadter, *Anti-intellectualism in American Life* (New York: Knopf, 1962).

[5] See Robert Lane, 'The Decline of Politics and Ideology in a Knowledgeable Society', *American Sociological Review*, 31 (October 1966), 649-62, and Daniel Bell, *The End of Ideology* (Glencoe, Ill.: The Free Press, 1960).

The past decade has been remarkable for the development of 'advocacy politics';[7] consumer advocates, planning advocates, health care advocates and environmental advocates have mobilized around diverse issues. Key slogans are 'accountability', 'participation', and 'demystification'. These groups share common concerns with the 'misuse of expertise', the 'political use' of scientists and professionals, and the implications of expert decision making for public action. Table 1 presents some statements of these concerns by various groups: radical scientists who have organized to develop 'science for the people'; consumer advocates concerned with corporate accountability; advocacy planners who assist communities in expressing their local needs; and environmentalists and health professionals who demand 'demystification of medicine'.

Their criticism reflects a dilemma. The complexity of public decisions seems to require highly specialized and esoteric knowledge, and those who control this knowledge have considerable power. Yet democratic ideology suggests that people must be able to influence policy decisions that affect their lives. This dilemma has provoked a number of proposals for better distribution of technical information; expertise, it is argued, is a political resource and must be available to communities as well as to corporations, utilities or developers.[8] The

---

[6] See discussion in Michel Crozier, *The Stalled Society* (New York: Viking Press, 1973), Chapter 3. A vivid example of the importance of this tendency to monopolize knowledge occurred during the 'energy crisis' with the realization that the large oil companies had nearly exclusive knowledge on the state of oil reserves.

[7] I am using this term to describe a phenomenon that Orion White and Gideon Sjoberg call 'mobilization politics', in 'The Emerging New Politics in America', M. D. Hancock and Gideon Sjoberg (eds.), *Politics in the Post Welfare State* (New York: Columbia University Press, 1972), 23.

[8] Note for example the system of 'scientific advocacy' proposed by John W. Gofman and Arthur R. Tamplin, *Poisoned Power* (Emmaus, Penn.: Rodale Press, 1971). A similar system is suggested by Donald Geesaman and Dean Abrahamson in 'Forensic Science – A Proposal', *Science and Public Affairs (Bulletin of the Atomic Scientists)*, 29 (March 1973), 17. Thomas Reiner has proposed a system of community technical services in 'The Planner as a Value Technician: Two Classes of Utopian Constructs and Their Impact on Planning', in H. Wentworth

Table 1. Public Concern with Expertise

Radical Scientists	*Consumer Advocates*	*Environmentalists*	*Advocacy Planners*	*Medical Critics*
		On The Misuse of Technology		
[we] feel a deep sense of frustration and exasperation about the use of [our] work. We teach, we do experiments, we design new things – and for what? To enable those who direct this society to better exploit and oppress the great majority of us? To place the technological reins of power in the hands of those who plunder. . .	What is needed is a sustained public demand for a liberation of law and technology to cleanse the air by disarming the corporate power that turns nature against man.	Many believe that the advantages of our technology compensate for environmental degradation . . . . Some have faith that the laboratories that have delivered miracles can also provide the tools to remedy any problems man may face. But with technology's gifts to improve man's environment has come an awesome potential for destruction.	Advocacy planning may be one of the channels of action through which people may try to humanize their technical apparatus; to prevent the exercise of bureaucratic power from leading to a new diffuse despotism, in which power appears in the image of technical necessity.	Psychiatry and psychology are used as direct instruments of coercion against individuals. Under the guise of 'medical methods', people are pacified, punished, or incarcerated.
		On the use of Expertise		
Skills and talents of potentially enormous usefulness have been bent to destructive ends to guarantee expansion and	Too many of our citizens have little or no understanding of the relative ease with which industry has or can obtain	It doesn't require special training to keep a broad perspective and to apply common sense. Thus, for every technically	Even without administrative power, the advocate planner is manipulator . . . The planner may not be the	Professionals often regard themselves as more capable of making decisions than other people, even when their

protect imperialism. For the sake of the ruling class scientists and engineers have been turned into creators of destruction by the mechanisms of an economic and social system over which they can exert no control.	the technical solutions. . .	knowledgeable [person] there is a layman activist. . . In fact, the technologist's training can stand in his way. There is a growing awareness that civilized man has blindly followed the technologists into a mess.	first to identify 'problems' of an urban area, but he puts them on the agenda and plays a large part in defining the terms in which the problem will be thought about – and those terms in effect play a large part in determining the solution.	technical knowledge does not contribute to a particular decision. . . Professionalism is not a guarantor of humane, quality services. Rather it is a code-word for a distinct political posture.
		On Expertise and Public Action		
What we can hope is not that scientists can provide the people with an objective approach to build a better world, but that in the better world built by the people, scientists will be able to work in a more objective fashion, unfettered by elitism and the worst competitive aspects of present-day science.	An action strategy must embrace the most meticulous understanding of the corporate structure – its points of access, its points of maximum responsiveness, its specific motivational sources and its constituencies.	The importance of the environmental movement's potential rests not only in what tangible results it can accomplish, but in its acting as a catalyst to start people working together. Alliances possible by organizing around environmental concerns stagger the mind of the seasoned community organizer.	Any plan is the embodiment of particular group interests . . . any group which has interests at stake in the planning process should have those interests articulated. . . Planning in this view becomes pluralistic and partisan – in a word, overtly political.	Medicine should be demystified. . . When possible, patients should be permitted to choose among alternative methods of treatment based upon their needs. Health care should be deprofessionalized. Health care skills should be transferred to worker and patient alike.

Sources: These statements are quotations from editorials and the popular literature circulated by such groups as SESPA, Nader's Raiders, Earth Day groups and the Health Policy Advisory Centre.

increasing importance of technical information has also prompted analyses of the behaviour of scientists as they are diverted to applied and controversial work.

For example, Allan Mazur suggests that the political (i.e. non-scientific) context of controversies crucially affects the activities of scientists, the way they present their findings, and thus their ultimate influence on decisions. Despite norms of political neutrality, claims Mazur, scientists behave just like anyone else when they engage in disputes; their views polarize and as a result the value of scientific advice becomes questionable. Thus, disputes among experts may become a major source of confusion for policy makers and for the public.[9] Guy Benveniste, focusing on the use of scientists by policy makers, suggests that 'technical' decisions are basically made on political or economic grounds. Expertise is sought as a means of supporting particular policy programmes; the selection of data and their interpretation are thus related to policy goals.[10] Similarly, King and Melanson argue that when knowledge is employed in the resolution of public problems, it is shaped, manipulated, and frequently distorted by the dynamics of the policy arena.[11]

These analyses emphasize the politicization of expertise. Details of two recent disputes in which 'experts' were used by both project developers and critics provide an opportunity to develop these arguments, and then to explore the impact of experts on the political process. One of the disputes concerns the siting of an 830 megawatt nuclear power plant on Cayuga Lake in upstate New York; the other is the proposed construction of a new runway at Logan International Airport in East Boston, Massachusetts.

The power plant siting controversy began in June 1967, when the New York State Electric and Gas Company (NYSE&G) first

---

Eldridge (ed.), *Taming Megalopolis*, I (New York: Anchor Books, 1967). Based on systems similar to legal advocacy and expert witness in the courts, such proposals are intended to make technical advice more widely available to citizens' groups – usually through provision of public funds to underwrite the cost of expertise.

[9] Allan Mazur, 'Disputes Between Experts', *Minerva*, 11 (April 1973), 243-62.

[10] Guy Benveniste, *The Politics of Expertise* (Berkeley, Calif.: Glendessary Press, 1972). See also Leonard Rubin, 'Politics and Information in the Anti-Poverty Programs', *Policy Studies Journal*, 2 (Spring 1974), 190-5.

[11] Lauriston R. King and Philip Melanson, 'Knowledge and Politics', *Public Policy*, 20 (Winter 1972), 82-101.

announced its intention to build Bell Station.[12] Groups of scientists and citizens, concerned with the thermal pollution of Cayuga Lake, organized themselves to oppose the plant, and demanded that NYSE&G consider design alternatives that would minimize the damage to the lake caused by waste heat. They forced the utility to postpone its application for a construction permit, and to contract for additional research on the environmental impact of the plant. In March 1973, following consultants' recommendations, NYSE&G announced a power station plan that was essentially the same as its earlier controversial design. The company, however, was now armed with data from one and a half million dollars' worth of environmental research supporting its claim that the heat from Bell Station would not damage the lake. Yet once more there was concerted and well-informed public opposition, this time focused on radiation hazards. Four months later the company was forced to abandon its plan.

The proposed new 9200-foot runway at Logan Airport was part of a major expansion plan that had been a source of bitter conflict in East Boston for many years.[13] Located only two miles from the centre of downtown Boston in an Italian working-class community, this modern convenient airport is a source of extreme irritation, fear, and community disruption. The expansion policies of the Massachusetts Port Authority (Massport) have been opposed, not only by airport neighbours but also by Boston's city government and by state officials concerned with the development of a balanced transportation system. Here, as in the Cayuga Lake power plant siting debate, knowledge was used as a resource both by Massport, seeking justification for its expansion plans, and by those opposed to such plans. Massport's staff was backed by consultants who claimed that without expansion the airport would reach saturation by 1974, and that the new runway would cause no environmental damage. The opponents, primarily from the adjacent working-class neighbourhood of East Boston, used technical advice provided by the city of Boston. Following pressure from the governor as well as from the mayor, Massport eventually deleted the proposed runway from the master plan for future airport development.

---

[12] For a history and analysis of this controversy see Dorothy Nelkin, *Nuclear Power and its Critics* (Ithaca, N.Y.: Cornell University Press, 1971); 'Scientists in an Environmental Controversy', *Science Studies*, 1 (1971), 245-61; and 'The Role of Experts in a Nuclear Siting Controversy', *Science and Public Affairs*, 30 (November 1974), 29-36.

[13] Documentation of this conflict can be found in Dorothy Nelkin, *Jetport: The Boston Airport Controversy* (New Brunswick, N.J.: Transaction Books, 1974).

While this paper will focus on similarities in the dynamics of these two disputes, it is necessary first to point out important differences. The community opposed to the power plant was a college town; the dispute was a middle-class environmental conflict, sustained by expertise from scientists in a nearby university who also lived in the area. In contrast, the opposition to the airport came primarily from a working-class neighbourhood dependent on expertise provided by government officials who, for political and economic reasons, chose to oppose the airport development plans.

The technical aspects of the two disputes were also quite different. The power plant issue was embedded in a set of vague uncertainties and intangible fears about radiation; airport expansion posed the concrete and direct threat of increased noise and land purchase. The main area of technical conflict in the former case was the potential environmental impact of the new power plant and the experts involved were mostly scientists and engineers. In the latter case the controversial issue was the validity of projections – whether the runway was really necessary at all – and the dispute involved economists and lawyers as well as engineers.

Despite such differences, the two cases have a great deal in common: the use of expertise, the style of technical debate, and the impact of experts on the political dynamics of the dispute are remarkably similar.

THE USE OF EXPERTISE

Opposition to both the power plant and the airport developed in several stages. The developers (utility manager, airport manager) contracted for detailed plans on the construction of their proposed facility. As they applied for the necessary permits, affected groups tried to influence the decision. The developer in each case argued that plans, based on their consultants' predictions of future demands and technical imperatives concerning the location and design of the facility, were definitive, except perhaps for minor adjustments necessary to meet federal standards.

In the power plant controversy, scientists from Cornell University who lived in the community were the first to raise questions about the NYSE&G plan when it was announced in 1967. By mid-1968, their activity had built up sufficient political support to persuade NYSE&G to postpone its plans, and to undertake further environmental research.

A new sequence of events began in March 1973, when NYSE&G again announced its intention to build the plant and claimed that it was imperative to begin construction promptly. The company's consultants, Nuclear Utilities Services Corporation, had prepared a five-volume technical report. NYSE&G placed copies in local libraries, circulated a summary to its customers, and invited comments. The report supported NYSE&G's earlier plan for a plant involving a General Electric boiling water reactor with a once-through cooling system. The study concluded that cooling towers (which had been recommended by power plant critics in 1968) were economically unfeasible in the size range required for the plant, unsuited to the topography of the area, and would have a tendency to create fog. To develop an optimum design for a once-through cooling system, consultants designed a jet diffuser to provide rapid mixing of the heated discharge with the lake water. With this system, they argued that the plant would have an insignificant effect on the aquatic environment of Cayuga Lake. The consultants only briefly concerned themselves with the issue of radioactive wastes on the grounds that this was not a problem unique to Cayuga Lake; the report only stated that the effect would be substantially below current radiation protection standards.

NYSE&G organized an information meeting attended by 1,000 citizens, and for two hours summarized the highly technical material supporting its plans. This, however, was followed by two and one-half hours of angry discussion, and the utility's president announced that if public protest was likely to cause delay, they would build the plant at another site. He hoped, however, that the decision would be 'based on fact and not on emotion'.

The first organized response came from twenty-four scientists who volunteered to provide the public with a review and assessment of the utility's massive technical report.[14] Their review was highly critical and NYSE&G's consultants responded in kind (see below). Meanwhile, citizens' groups formed and the community polarized, as the company posed the issue in terms of 'nuclear power *or* blackouts'.

The airport case also involved experts on both sides of the controversy. Opposing forces mobilized in February 1971, at a public hearing required by the Corps of Engineers in order to approve

---

[14] Two hundred copies of the critique were sent to libraries, citizens' groups, faculties at universities and colleges in the area, officials in state and federal agencies, political representatives in local, state and federal government, and newspapers.

Massport's request to fill in part of Boston Harbour. One thousand people attended and for ten hours scientists, politicians, priests, schoolteachers and others debated the priorities which they felt should govern airport decisions. Massport's staff was backed by consultants, who claimed that without the runway the airport would reach saturation by 1974. Consultants provided a brief environmental statement arguing that the new runway would have no direct detrimental effects of ecological significance. The only environmental costs would be the elimination of ninety-three acres of polluted clam flats and two hundred and fifty acres of wildlife preserve – which constituted a hazard in any case because birds interfere with jets. Furthermore, because of the added flexibility, the runway would relieve noise and congestion caused by an expected increase in aircraft operations. Massport's claims were later buttressed by an environmental impact statement commissioned from Landrum and Brown, Airport Consultants, Inc. at a cost of $166,000. The study documented Massport's contention that the new runway was essential for safety and would be environmentally advantageous; it emphasized the positive contributions of Logan Airport – its economic importance to the City of Boston, and the reduction of noise that would result from increased runway flexibility.

The opposition was organized by a coalition of citizens' groups called the Massachusetts Air Pollution and Noise Abatement Committee. The issues raised were diverse. Neighbourhood people spoke of the discomfort caused by aircraft operations, and of Massport's piecemeal and closed decision-making procedures. Environmentalists feared the destruction of Boston Harbour, and planners related airport decisions to general urban problems. Legal, economic, and technical experts became involved as the Mayor's office and the Governor evaluated Massport's claims. As in the power plant case, the conflict polarized as Massport posed the issue in terms of 'airport expansion *or* economic disaster'.

THE STYLE OF TECHNICAL DEBATE[15]

In both cases the technical debate involved considerable rhetorical licence, with many insinuations concerning the competence and the biases of the involved scientists.[16] NYSE&G emphasised that the need for a nuclear power plant on Cayuga Lake was 'imperative', that there would be a serious energy shortage if they did not proceed immediately with the plan, and that the impact of the plant on the local environment would be 'insignificant'. NYSE&G insisted on their unique technical competence to make this decision.

---

[15] Unless otherwise noted, the quotations that follow are from local environmental reports, memos, letters and public hearings. They are statements by the opposing scientists involved in the controversy.

> Our study is the most comprehensive study ever made on the lake. Opponents can create delays but are not required to assume responsibility.

However, the Cornell critics called NYSE&G's data 'inadequate', 'misleading', 'non-comprehensive', and 'limited in scope and inadequate in concept'. Some of the critics provided data from other research that contradicted NYSE&G's findings. They emphasized that there was simply not enough known about deep-water lakes to assess the risks.

NYSE&G consultants countered by claiming that Cornell critics were unfamiliar with the scope and requirements of an environmental feasibility report; in particular, that the critics' review failed to distinguish between the goals of pure and applied research.

> From an academic position a complete ecological model that predicted all possible relationships would be desirable, but this was neither feasible nor necessary for assessing the minor perturbations caused by one plant.

In fact, each group used different criteria to collect and interpret technical data. The two studies were based on diverse premises which required different sampling intervals and techniques. NYSE&G consultants, for instance, claimed that their water quality studies focused on establishing base-line conditions to predict the changes caused by the power plant; Cornell studies focused on limiting factors, such as the impact of nutrients on lake growth.

Scientists attacked each other with little constraint. Cornell reviewers accused NYSE&G consultants of value judgments that led to 'glaring omissions', 'gross inadequacies', and 'misleading interpretations'. Consultants referred to the Cornell report's 'confusion resulting from reviewers reading only certain sections of the report', and 'imaginative, but hardly practicable suggestions'. The NYSE&G president accused the Cornell reviewers of bias:

---

[16] Mazur, *op. cit.* note 9, also documents the use of rhetoric in technical debates.

> It is of some interest that many of the individuals who participated in the Cornell review have taken a public position in opposition to nuclear plants. Philosophical commitment in opposition to nuclear generation may have made it difficult for these reviewers to keep their comments completely objective.[17]

A similar style of debate characterized the technical dispute over the airport runway. Expansion of Logan was recommended by consultants as 'the best opportunity to realize a reduction of current social impact'. Failure to expand the airport as proposed would cause delays, increase air pollution, reduce safety margins and have a 'drastic' and 'immeasurable' impact on the local economy – 'an impact which the Boston area could not afford'. Massport's environmental report described and rejected, one by one, alternatives proposed by airport opponents. Banning specific types of aircraft 'interferes with interstate commerce'. Limiting maximum permissible noise levels is 'legally questionable', since the airport functions as part of a coordinated national system. A surcharge for noisy aircraft would be 'useless' as economic leverage, since landing fees represent a negligible percentage of total airline expenses. Setting night curfews is 'precluded' by the interdependence of flight schedules and aircraft utilization requirements: it would relegate Boston to a 'second-class' airport and have 'disastrous effects' on service to sixty five percent of the 267 cities served by Boston. Moreover, seventy percent of the cargo business would be 'negatively affected'. Soundproofing neighbouring houses and building would be 'economically prohibitive' and have little effect. The *only* feasible solution to noise and environmental problems, according to the consultants' report, was an expanded runway system that would permit increased flexibility. Massport insisted on the validity of its expertise:

> We are closer and more knowledgeable than any other group no matter what their intention may be, on what Logan Airport . . . what Metropolitan Boston, what the entire state of Massachusetts and New England needs.[18]

And Massport consultants suggested their agreement with their client when, in a technical analysis of the airport's economic impact, they stated:

---

[17] William A. Lyons, 'Recommendations of the Executive Offices of New York State Electric and Gas Corporation to the Board of Directors' (13 July 1973).

[18] Edward King, Massport Executive Director, Testimony at U.S. Corps of Engineers' 'Hearings on the Application by the M.P.A. for a Permit to Fill the Areas of Boston Harbour' (Boston, 26 February 1971), mimeograph, 101.

> It is inconceivable that an enterprise of this magnitude can be treated other than with the most profound respect.[19]

Airport opponents called the Massport technical reports 'the logical outcome of efforts directed toward narrow objectives'. City consultants contended that authority to restrict aircraft noise was in fact limited neither by the FAA nor by the Massport enabling act, and that the FAA actually encouraged airport operators to restrict airport noise independently. They argued that Massport's assumptions concerning anticipated demand for increased airport capacity were questionable and in any case were subject to modification by consolidating schedules and dispersing general aviation flights. Massport's own raw data suggested that with a reasonable adjustment Logan Airport could accommodate a considerable increase in actual business, for aircraft were operating at an average of just under half capacity. Moreover, projections were based on the growth pattern of the 1960s. The decrease in air travel demand in 1970 could have been regarded either as a new data point or as an anomaly. Massport chose the latter interpretation, ignoring the 1970 slump. Their projections also ignored the possibility of competitive alternatives to air travel.[20]

Massport's figures concerning the economic impact of expansion and the consequences of a moratorium on expansion were debunked by critics as 'blatant puffery'. As for Massport's contention that the new runway would be environmentally advantageous, city representatives concluded that an expanded airfield would only expose new populations to intolerable noise. Instead, they recommended measures to increase capacity at Logan through scheduling adjustments and efforts to distribute the hours of peak demand by economic controls such as landing fees.

Differences were to be aired at a second round of public hearings scheduled for July 10, 1971. However, on July 8, following a task-force study that recommended alternatives to expansion, Governor Sargent publicly opposed the construction of the new runway. Under these circumstances, the Corps of Engineers was unlikely to approve the project, so Massport withdrew its application for a permit and temporarily put aside its plans for the runway. A year and a half later, in February 1973, Massport deleted the proposed runway from the master plan for future airport development. Citing projections that were close to those used by airport opponents two years earlier, the Port Authority claimed that re-evaluation of future needs indicated that the new runway was no longer necessary.

---

[19] Landrum and Brown, Inc., *Boston-Logan International Airport Environmental Impact Analysis* (11 February 1972), section ix, 3.

[20] A systematic critique of Massport's data was made by a commission chaired by Robert Behn (Chairman of Governor's Task Force on Inter-City Transportation), 'Report to Governor Sargent' (April 1971).

Both disputes necessarily dealt with a great number of genuine uncertainties that allowed divergent predictions from available data. The opposing experts emphasized these uncertainties; but in any case, the substance of the technical arguments had little to do with the subsequent political activity.

THE IMPACT OF EXPERTISE ON POLITICAL ACTION

In both the airport and power plant controversy, it was the *existence* of technical debate more than its *substance* that stimulated political activity.[21] In each case the fact that there was disagreement among experts confirmed the fears of the community and directed attention to what they felt was an arbitrary decision-making procedure in which expertise was used to mask questions of political priorities.

This relationship between technical disputes and political conflict was most striking in the power plant case. Cornell scientists assessed the NYSE&G report with the intention of providing technical information to the public. They focused almost entirely on the issue of thermal pollution – the effect of the plant's heated effluent on Cayuga Lake. The citizens' groups, however, were most concerned with the issue of radiation. They had followed the considerable discussion in the press and in popular journals about the risks associated with the operation of nuclear reactors – risks that had not been as widely publicized at the time of the first controversy in 1968. Thus, the thermal pollution issue (which had dominated earlier controversy) became, in 1973, a relatively minor concern. Citizens, in contrast to the scientists who were advising them, focused on problems of transporting and disposing of nuclear wastes, on the reliability of reactor safety mechanisms, on reactor core defects that would allow the release of radioactive gases, and on the danger of human error or sabotage.

---

[21] For further discussion of this point, see Nelkin, 'The Role of Experts in a Nuclear Siting Controversy', *op. cit.* note 12.

When the citizens' committee first met to establish a position on the issue, its newsletter concentrated entirely on the reactor safety issue.[22] This set the tone of subsequent discussion, in which three possible courses of action were considered: that the committee oppose construction of any nuclear plant on Cayuga Lake until problems of reactor safety and disposal of radioactive wastes were resolved; that it take up its 1968 position and oppose only the *current design* of Bell Station; or that it support NYSE&G plans. The first proposal, one of total opposition, won overwhelming support. The emphasis of citizens' groups thereafter was on the risks associated with nuclear power, despite the fact that the technical debate dealt mainly with the problem of thermal pollution.

The disputes between scientists, however, served as a stimulus to political activity. In the first place, the criticism by Cornell scientists neutralized the expertise of the power company. Simply suggesting that there were opposing points of view on one dimension of the technical problem increased public mistrust of the company's experts, and encouraged citizens to oppose the plant. Second, the involvement of scientists gave moral support to community activists, suggesting that their work would be effective. The citizens' groups called attention to NYSE&G's statement that if there were concerted opposition, the company would not go ahead with its plans. The ready support of local scientists led to substantial expectation in the community that the effort involved in writing letters and going to meetings would not be wasted.

As for the details of the technical dispute, they had little direct bearing on the dynamics of the case. Citizens trusted those experts who supported their position. People who supported NYSE&G voiced their trust in the consultants employed by the power company:

> Let us allow the professionals to make the decisions that they get paid to make.

And power plant critics used expertise only as a means to bring the issue back to its appropriate political context. The case was one of local priorities, they claimed; it was not a technical decision:

---

[22] CCSCL (Citizens Committee to Save Cayuga Lake), *Newsletter*, 6 (April 1973). This newsletter reprinted in full a selection of well-informed articles – notably those by Robert Gillette in *Science*, 176 (5 May 1973); 177 (28 July; 1, 8, 15 and 22 September 1972); and 179 (26 January 1973).

> To say that our future is out of our hands and entrusted to scientists and technicians is an arrogant assumption. . . We suggest that the opinions of area residents who care deeply about their environment and its future are of equal if not greater importance.[23]

In the airport case, the technical arguments served primarily to reinforce the existing mistrust of Massport among those opposed to airport expansion, and they were virtually ignored by those who supported Massport. Opinions about the necessity of the runway were well established prior to the actual dispute. In East Boston, Massport employees and local sports clubs which were supported by an airport community relations programme defended the Port Authority's plans for a new runway and maintained their trust in Massport's competence.

> In terms of efficient and competent operation, Massport is head and shoulders above other agencies.

Airport opponents, while benefitting from the advice provided by experts from the City of Boston, claimed the issue was a matter of common sense and justice. They defined the problem in terms of values (such as neighbourhood solidarity) which are not amenable to expert analysis.

> We need no experts. These people will verify themselves the effect of noise. . . . Massport is extremely arrogant. They do not have the slightest conception of the human suffering they cause and could not care less.[24]

Airport critics pointed out various technical errors and problems of interpretation in Massport's predictions and environmental impact statements; but this simply re-confirmed the community's suspicion of Massport, and further polarized the dispute. Later, these same experts who were sympathetic to East Boston's noise problem failed to convince the community to accept a Massport plan for a sound barrier. Despite advice that this would help to relieve their noise problem, the community chose to oppose construction of the barrier. Local activists feared that this was a diversion, and that if they accepted this project the community would somehow lose out in the long run. Thus, they disregarded expert opinion that this was a favourable decision, and the old mistrust prevailed.

---

[23] Statement by Jane Rice cited in the *Ithaca Journal*, (14 May 1973), 1.

[24] These statements are from testimony at U.S. Corps of Engineers' Hearings, *op. cit.* note 18. The ultimate expression of this kind of sentiment was, of course, the remark alleged to have been made by former vice president Spiro Agnew, responding to the report by the U.S. Presidential Commission on Pornography and Obscenity: 'I don't care what the experts say, I *know* pornography corrupts!'

SUMMARY AND CONCLUSIONS

The two conflicts described above, over the siting of a power plant and the expansion of an airport, have several aspects in common. One can trace parallels, for instance, in the way the developers used expertise as a basis and justification of their planning decisions; how experts on both sides of the controversy entered the dispute and presented their technical arguments; and how citizens affected by the plan perceived the dispute. Similarities are evident in public statements, as developers, experts and citizens expressed their concerns about various aspects of the decision-making process. These are compared in Table 2. These similarities, especially with respect to the use of scientific knowledge, suggest several related propositions which may be generalizable to other controversies involving conflicting technical expertise:

First, *developers seek expertise to legitimize their plans and they use their command of technical knowledge to justify their autonomy.* They assume that special technical competence is a reason to preclude outside public (or 'democratic') control.

Second, *while expert advice can help to clarify technical constraints, it also is likely to increase conflict,* especially when expertise is available to those communities affected by a plan. Citizens' groups are increasingly seeking their own expertise to neutralize the impact of data provided by project developers.[25] Most issues that have become politically controversial (environmental problems, fluoridation, DDT) contain basic technical as well as political uncertainties, and evidence can easily be mustered to support or oppose a given proposal.

Third, *the extent to which technical advice is accepted depends less on its validity and the competence of the expert, than on the extent to*

---

[25] For further discussion of the tactics of using expertise within the fluoridation controversy, for example, see Robert Crain *et al.*, *The Politics of Community Conflict* (Indianapolis: Bobbs Merrill, 1969); and H. M. Sapolsky, 'Science, Voters and the Fluoridation Controversy', *Science*, 162 (25 October 1968), 427-33.

Table 2. Perspectives on Decision Making and Expertise

	Power plant dispute	*Runway dispute*
Developers		
On responsibility and competence for planning	Our study is the most comprehensive study ever made on the lake. Opponents can create delays but are not required to assume responsibility.	We are closer and more knowledgeable than any other group no matter what their intention may be, on what Logan Airport . . . what Metropolitan Boston. . . . what New England needs.
On public debate	We have adopted a posture of no public debate.	We have competent staffs . . . I can't see any sense in having a public hearing. . . If it is to be by consensus that the authority operates . . .
Experts (consultants)		
On impact of project	Alternate approaches would have undesirable effects on the human environment . . . the proposed design should produce no significant impact. Actual individuals would be exposed to much lower doses than that due to normal habits.	Adverse environmental impact will result from failure to undertake this project as contrasted with the impact if the Authority proceeds. Noise measurements of typical urban noise conditions . . . show that street level background noise overshadows taxi-way noise.
On planning	Although an ecological model might be desirable from an academic viewpoint it is not felt to be necessary to provide an adequate assessment of the impact of the minor perturbation introduced by the proposed plant.	A master plan would be nothing more than an academic exercise . . . a study of this magnitude could never be justified for a small project of this nature.
Experts (critics)		
On developers' data	Statements and conclusions were not justified and must therefore be regarded as nothing more than guesses. . . The data base is not only inadequate, but misleading.	Analysis of the economic impact of Logan Airport shows demonstrated 'blatant puffery' in the figures appearing in the report.

Table 2 contd. **Perspectives on Decision Making and Expertise**

Citizens (project supporters)		
On decision-making responsibility	Let us allow the professionals to make the decisions that they get paid to make.	In terms of efficient and competent operation, Massport is head and shoulders above other agencies.
Citizens (project opponents) *Citizens (project*		
On decision-making responsibility	To say that our future is out of our hands and entrusted to scientists and technicians is an arrogant assumption. . . We suggest that the opinions of area residents who care deeply about their environment and its future is of equal if not greater importance.	We need no experts. These people will verify themselves. . . Massport is extremely arrogant. They do not have the slightest conception of the human suffering they cause and could not care less.
On decision-making process	Were they using the power the people gave them to support their own feelings or those of private concerns? There is representative government in our country, but it sure isn't in our county.	What is really on trial here is not just the Port Authority, it is really the American system. Will it listen to spokesmen for the people and the people who speak for themselves?

Sources: These perspectives are direct quotations from public hearings, letters or transcripts of meetings.

which it reinforces existing positions. Our two cases suggest that factors such as trust in authority, the economic or employment context in which a controversy takes place, and the intensity of local concern will matter more than the quality of character of technical advice.[26]

Fourth, *those opposing a decision need not muster equal evidence.* It is sufficient to raise questions that will undermine the expertise of a developer whose power and legitimacy rests on his monopoly of knowledge or claims of special competence.

---

[26] The relation between beliefs and the interpretation of scientific information is analyzed in S. B. Barnes, 'On the Reception of Scientific Beliefs', in Barry Barnes (ed.), *Sociology of Science* (Harmondsworth, Midx.: Penguin Books, 1972), 269-91.

Fifth, *conflict among experts reduces their political impact.* The influence of experts is based on public trust in the infallibility of expertise. Ironically, the increasing participation of scientists in political life may reduce their effectiveness, for the conflict among scientists that invariably follows from their participation in controversial policies highlights their fallibility, demystifies their special expertise and calls attention to non-technical and political assumptions that influence technical advice.[27]

Finally, *the role of experts appears to be similar regardless of whether they are 'hard' or 'soft' scientists.* The two conflicts described here involved scientists, engineers, economists and lawyers as experts. The similarities suggest that the technical complexity of the controversial issues does not greatly influence the political nature of a dispute.

In sum, the way in which clients (either developers or citizens' groups) direct and use the work of experts embodies their subjective construction of reality – their judgments, for example, about public priorities or about the level of acceptable risk or discomfort. When there is conflict in such judgments, it is bound to be reflected in a biased use of technical knowledge, in which the value of scientific work depends less on its merits than on its utility.

---

[27] See discussion of how controversy among scientists influences legislators in Barnes, *ibid.*

EXPERTISE AND POLITICS—PROBLEMS AND TENSIONS

HARVEY BROOKS

Professor of Applied Physics, Harvard University

(Dinner Address, April 25, 1975)

A PROFOUND CHANGE has taken place in the relation between the expert community and the political community in the last ten years. This change is much more complex than the customary clichés about anti-intellectualism and anti-technology muckraking, though that is a component. The participation of experts of all kinds in the political process is much greater and far-reaching today than it was when the Russian launching of Sputnik catapulted science advisers into the White House. Scientists today are listened to much more but believed much less than they were in those heady days. There is still a great respect for learning among politicians and policy makers, but there is also much greater skepticism and suspicion, and the image of objective, "value-free" science and scholarship is severely tarnished. Yet by and large science and technology are still looked to to solve many of our most pressing problems from food and population to energy and environment, from housing to child care.

What is happening, and why? Where is the science-polity relationship heading? Are we about to witness battles between opposing politicians flanked by their captive experts, or are we headed for a revival of Jacksonian hostility towards expertise and, in the words of Dorothy Nelkin of Cornell University "the belief that common sense is an adequate substitute for technical knowledge."[1]

A few days ago the House of Representatives adopted an amendment to the National Science Foundation authorization which requires that the director transmit to both houses of Congress every thirty days a list of all grants proposed for award by the Foundation, together with complete justification for each. Within thirty days either house may disapprove any or all of these grants simply by passing a resolution of disapproval—the ultimate in the substitution of lay judgment for the traditional scientific "peer review."[2] This amendment is not expected to survive conference, but is a warning shot across the bows, a harbinger of what scholars and professionals may anticipate more and more frequently in the future. Yet within the last year the Congress has passed dozens of bills pertaining to energy, environmental questions, or health care which mandate an objective study by the National Academy of Sciences, prior to the implementation of any legislation. One or another of the academies has been deeply involved in input to the national decision process on automobile emission standards, on offshore drilling, on strip-mining of western coal, on the Alaska pipeline, and prospectively on nuclear safety, on the United States response to the world food problem, possibly on banning the production of freon because of its effects on the stratosphere, and so on through a growing number of public issues, in which the technical community is looked to for guidance and hopefully "neutral" advice.

Part of what has happened is well summarized by Professor Dorothy Nelkin in a footnote to an article on "The Political Impact of Technical Expertise."[3] "In the past," she says,

> scientific influence has concentrated in government entrepreneurial areas such as space exploration, or in policy areas defined in terms of national security. The participation and influence of scientists has been traditionally rather minimal in policy areas with redistributive implications, e.g., social policy, transportation, and other areas subject to social conflict and competing political interests. As the public seeks technical solutions to social problems, and as scientists themselves become engaged in controversial public issues, this pattern is changing.

That is indeed the nub of the matter. Scientists first became engaged at the high policy level of the federal government in defense policy, and

---

[1] D. Nelkin, "The Political Impact of Technical Expertise," *Social Studies of Science* 5, 1 (January 1975): p. 36.

[2] "House Vote to Control Grants Scored by Head of Science Unit," *New York Times*, April 23, 1975: p. 45; also D. Shapley, "Congress: House Votes," p. 5.0.

[3] D. Nelkin, *ibid.*, p. 35.

PROCEEDINGS OF THE AMERICAN PHILOSOPHICAL SOCIETY, VOL. 119, NO. 4, AUGUST 1975

later in the design of the space program, where their role was generally seen as evaluating alternative means to generally agreed upon ends, such as greater national security for the dollar, or beating the Russians to the moon.[4] Nobody's ox was gored, except for a few contractors or bureaucrats. But many of today's issues involve intense conflicts or apparent conflicts of values—energy supply versus environmental purity, clean air versus cheap personal transportation, equity versus excellence in education or health care. The distinction between means and ends is often blurred, and technical uncertainties leave a wide-open door for the entry of personal values and political predilections. The more scientists and experts participate in the political process, the more they find themselves deploying their expertise on different sides of political issues, and this tends to highlight the fallibility of science, calling attention to the non-technical premises that often underlie technical or professional advice, especially in situations where the evidence is not all in. And, of course, on political questions of high technical content the evidence is never all in; otherwise the subject would not be so controversial. If we could really quantify the health effects of automobile emissions, or the risk of nuclear accidents, or the educational benefits of Head Start, political controversy would not disappear, but it would be much less confused by expertise. There might still be some differences in judgment as to how much more automobile purchasers should be required to pay to avoid several hundred premature deaths due to respiratory ailments each year. Even if the recent Atomic Energy Commission report turns out to be precisely correct in its estimate of the probability of a nuclear reactor accident,[5] there will be some people who feel that the assurance of a cheap and reliable source of electric energy is not worth even that tiny risk. But by and large the potential for political controversy in technical areas increases rapidly with technical uncertainty and incomplete evidence. We no longer see great national debates on vaccination or on the desirability of administering polio vaccines to schoolchildren, for example.

One of the problems that the scientist has in participating as an expert in the political decision process relates to the different role of tentative evidence and provisional conclusions. Such conclusions put forward within the scientific community are relatively reversible. If advanced with scientific caution, the conclusions are accepted in that spirit, and are progressively revised by criticism and the search for new evidence and analysis. Provisional conclusions in basic science have few consequences outside the scientific community, and hence can be bandied about rather freely; they are thus functional within science, speeding convergence on the truth. But in the political arena tentative propositions or data can be highly disfunctional when drastic political action may be taken in consequence. The situation is further complicated by the fact that political consequences can be strongly affected not only by the conclusions themselves, but by the particular way they are presented to the public. This is becoming an increasing problem as science is able to discern more and more subtle effects on the basis of very complex chains of reasoning, often extremely abstract and technical.

For example, my colleague at Harvard, Professor Michael McElroy, has encountered this dilemma in most acute form. He and his associates have been making model calculations on the effects of freon[6] (used in spray cans and refrigerants) and of methyl bromide[7] (used extensively as a crop preservative in agriculture) on the destruction of the ozone layer in the stratosphere. If production of freon and methyl bromide continues to increase at current rates for the next decade or so, his computations indicate the possibility of significant though uncertain consequences both to health and to agriculture by the end of the century. Since his calculations were published he has been beset by congressional committees and television talk shows, not to mention the research directors of industries that produce freon or methyl bromide. Cautions about the uncertainties in the present evidence, and the need for more research, are brushed aside by one group as self-serving pronouncements aimed at getting research grants or blackmailing the chemical industry; this group wants an immediate ban on freon production, an $8 billion a year industry employing 250,000 workers. The other group dismisses McElroy's work as mere abstract speculation, an unproved theory which he is irresponsible to publish. McElroy argues that one could wait two or three years to find out more before taking drastic political action; the delay would make a negligible difference in the final outcome. But this is an unpopular view in many quarters.[8]

---

[4] R. R. Nelson, "Intellectualizing about the Moon-Ghetto Metaphor: A Study of the Current Malaise of Rational Analysis of Social Problems," *Policy Science* **5** (1974): pp. 375–414, *cf.* esp. p. 381.

[5] N. Rasmussen, *et al.* *Reactor Safety Study: (WASH-1400)*, draft of August, 1974, USGPO, Washington, D. C., 1974.

[6] M. B. McElroy, N. D. Sze, and S. C. Wofsy, "Freon Consumption: Implications of Atmospheric Ozone," *Science* **187** (February 14, 1975): pp. 535–537.

[7] M. B. McElroy, S. C. Wofsy, and Y. L. Yung, "The Chemistry of Atmospheric Bromine," to be published in *Geophysics Letters*.

This is a prototypical case of a situation which scientists of integrity are facing increasingly. It is a situation in part created by the increasing power of science to detect or predict ever more subtle and esoteric environmental effects, often decades before they would become noticeable in terms of any detectable human impact. Many carcinogenic (cancer-producing) chemicals in low concentrations may not lead to cancer for thirty or forty years, but once their presence can be detected in the environment the scientist and the politician are both faced with dilemmas and choices which could have been ignored if the knowledge had not been available. Mutagenic (mutation-producing) chemicals may have no detectable effects among humans for generations. Yet if we were to adopt an ultraconservative approach in each one of these situations, humanity would be deprived of many benefits whose cumulative effect would probably far outweigh the tiny risks involved. It is not enough to say to the scientist, just tell us the facts and let the chips fall where they may. The particular facts which are selected and the way they are presented to the public may have much greater political impact than the mere facts by themselves, so that the scientist holds enormous power in his hands.

Furthermore, the politician naturally wants clear-cut answers: if possible he wants to be told what course to follow, so that he can throw the onus of difficult decisions having large redistributive effects onto objective scientific considerations. Scientists inexperienced in the political arena, and flattered by the unaccustomed attentions of men of power, are often inveigled into stating their conclusions with a confidence not warranted by their evidence, and at the same time their statements in a political arena are not subject to the same sort of prompt corrective process that they would be if confined within the scientific community.

There is an interesting parallel between the scientist's desire to establish priority for a discovery or invention, and politician's search for new issues on which he can make a name for himself—issues which differentiate him from other politicians in the eyes of his constituents, just as a new finding differentiates a scientist in the eyes of his professional peers. I have never seen this basis for affinity of motivation between scientists and politicians discussed anywhere. It is particularly important in relations with the legislative branch, since it is legislators that tend to be one-issue people: it is much more difficult for an executive to make his name on a single issue. Recently technical questions have provided very fertile ground for politicians to dig in in their search for distinctive issues. This has been especially true in the environmental area. The potential alliance between individual politicians and scientists, though often beneficial, can also be dangerous because neither side is subject to the normal checks and balances of peer groups. Once a politician has staked out scientific territory for himself, his colleagues tend to stay away. At the same time the scientist is speaking in a forum to which opposing scientific views are not more or less automatically accorded equal access. The politician is free to select his own experts to develop an issue in the way that has maximum political utility to him. Truth may be only incidental.[9]

The technical issues which have high political utility in this sense depend strongly upon the current political climate. Right now environmental issues and anti-big-technology issues have great political clout. Solar energy and windmills, even though they are also technology, are acceptable because they are "little technology." But the phenomenon I am talking about was not unknown in what we now look back on as the earlier era of technological euphoria—approximately 1955–1965. In this climate the same sort of alliance between politician and scientist could take place in relation to the promotion of big technology, especially in the space-defense area. Some of the dramatic technology fiascos of the fifties and sixties had their origins in this kind of alliance, usually reinforced by the fact that the proposed project would be located in some congressman's own district. One needs only to mention Aircraft Nuclear Propulsion, Project Orion, Project Mohole, Project Dynasoar (now revived as the space shuttle), Project Rover and many acronyms that I shall not try to explain.

---

[8] For a review of this controversy see Allen L. Hammond, "Ozone Destruction: Problem's Scope Grows, Its Urgency Recedes," *Science* **187** (March 28, 1975): pp. 1181–1182.

[9] D. Nelkin, *ibid.*, p. 54.

But in recent years it has become easier to stop new technology than to start it. The launching of a new technological project does require a major commitment of resources, and hence the recruitment of a significant political consensus. This seems to occur only when the country is frightened, whether by a sputnik or an oil embargo. In more normal times the negative seems to possess much more political utility than the positive. One critic can neutralize the advocacy of many proponents.

A review of scientific inputs to policy over the last few years may lead the cynic to the view that science has had very little substantive influence on policy. The scientific "facts" which have actually been used are simply those which selectively support the political values of those who use them, and the "objective" input of science to policy appears as an illusion. Recently I had the experience of serving as a member of the so-called advisory board of the Ford Foundation Energy Policy Project, whose output was published in the catchily titled book: *A Time to Choose: America's Energy Future*. To me the most interesting part of this book is the series of commentaries or reviews by individual members of the advisory board, published at the end of the book. Written by individuals all with some measure of expertise in the subject matter, they remind one of nothing so much as the story of the blind men and the elephant.[10] It is hard to believe that advisers are talking about the same problem. One adviser accuses the staff of irresponsible timidity because it failed to call for an immediate moratorium on nuclear power and a reduction by a factor of three in per capita energy consumption by the end of the century. Several others accuse the staff of irresponsibly sabotaging the "proven" technology of nuclear energy and of committing mayhem on the standard of living of the American people. The whole thing reveals a divergence of values and political prejudices which is truly depressing. If experts are so at loggerheads, how can the public and the politicians fail to be confused? The expert views seem to leave almost unlimited scope for all possible national energy policies.

Yet I am not so pessimistic as this account would indicate. The progress of science itself is an inherently disorderly, even irrational process, even though we are very quick to rationalize it in retrospect, and make it appear highly logical *after* the fact, as indeed it is. Perhaps in a longer perspective the interplay of science and politics will also appear more rational than it seems when one is immersed in the process. Both processes proceed by evolution and natural selection.[11] At any one time errors and false starts far outnumber those ideas that eventually win through. This is true to a considerable extent in science, and it is equally true in democratic politics. The costs of these errors may be larger in the political than in the scientific arena, but they may also be the costs we have to pay to achieve a better direction of evolution in the long run. Many people have remarked that highly centralized organizations and decision processes avoid small mistakes but are much more prone to really large ones, without any feedback.

The history of automobile emissions legislation may prove to be a case in point. After several years of frustration about lack of progress in the control of urban air pollution Congress passed the clean air amendments of 1970. Largely the result of a complex interplay of political forces, these required a very drastic roll-back of automobile emissions by 1975, using standards originally designed to be reached in 1982, in order to achieve certain air quality objectives with the automobile population expected by 1990 according to then current projections of auto production.[12] The legislation has been modified twice since, largely in response to recently discovered technical evidence summarized in reports of the National Academy and other groups.[13] It will probably be modified subsequently at least once again in the near future. Many experts deplored the approach of the 1970 law not because they disagreed with the ultimate goals, but because they feared that the compressed time scale would lock the automobile industry into a clumsy, complex, and expensive "technological fix" of the internal combustion engine and delay or foreclose more fundamental, orderly, and less expensive approaches to the problem, in particular radically different auto power plants.[14] A few weeks ago, for reasons which none of the scientific studies had clearly foreseen the administrator of EPA announced that the application of the original 1975 standards for carbon monoxide and hydrocarbons would be postponed to 1979, and that new legislation would probably be sought for an even larger postponement.[15] The reason was evidence that the catalysts developed to meet the original standards would generate sulfates, whose health effects could be more serious than those of the pollutants the catalysts were designed to remove. This was not certain because the discovery was too recent and too little research had been done to assess its significance. Thus we seem to have gone full circle, and we may now be forced back on the road towards new types of power plants that do not require catalysts. Indeed this was apparently the intent of the research on which the original standards were based.

---

[10] D. Freeman, *et al.*, *A Time to Choose: America's Energy Future* (Combridge, Mass., 1974), pp. 349–412.

[11] H. Brooks, "Technology Assessment as a Process," *Internat. Social Science Jour.* 25 (1973): pp. 247–256.

[12] For an account of the technical basis of the 1975 standards see W. R. Ahern, Jr., "Health Effects of Automotive Air Pollution" in H. D. Jacoby, and J. D. Steinbruner, *Clearing the Air: Federal Policy on Automotive Emissions Control* (Cambridge, Mass., 1973), esp. pp. 162–174.

[13] *Air Quality and Automotive Emission Control.* A Report by the Coordinating Committee on Air Quality Studies, NAS/NAE, Summary Report, 1 (Washington, D. C., September 1974).

It is tempting to believe that with more constructive use of science and the advice of scientists we could have avoided this zig-zag, confused performance and saved some of the billions of dollars it has cost the consumer and the industry. Perhaps. But perhaps also the draconian and scientifically ill-founded 1970 law was necessary to get the auto industry and the great government bureaucracy to take the auto pollution problem seriously; at least that is what the defenders of the law argue. To some extent the technical events which have taken place since 1970 have confirmed the implicit belief of the rabid environmentalists that if you just held the engineers' feet to the fire, they would make more progress in a short time than they claimed they could. They did, but without considering the second and third order consequences of this progress.

Many scientists, particularly the younger generation of scientists who are the major supporters of so-called public interest science feel that you can only get things done if you exaggerate and dramatize scientific findings. Yet I fear that the long range effect of this strategy may be to undermine the credibility of science not only in the political process, but in its own right as an institution which has earned the right to self-governance and autonomy within the society.

On balance I am not that pessimistic, and as in most situations I think self-corrective action will take over. But I think it is time that the scientific and technical community examined itself and took a more realistic view of the long-range costs of advocacy in the public arena, both as proponents and as critics of technology.[16] The same, of course, applies to other areas of expertise and is not unique to science. It is just that public confidence in the natural sciences is higher, and they may therefore have further to fall in public estimation than, shall we say, economics or sociology.

---

[14] Cf. Jacoby and Steinbruner, *ibid.*, chap. 4.

[15] C. Holden, "EPA Grants Reprieve on Auto Emissions," *Science* **187** (March 28, 1975): p. 1179.

[16] Tom Alexander, *Fortune*, April, 1975: pp. 146–150; *cf.* esp. p. 150.

Utopian and Pragmatic Rationalism: The Political Context of Scientific Advice

YARON EZRAHI

TOWARDS the end of the 1970s, disillusionment with the power of scientific knowledge to transform and rationalise the decisions and actions of governments led to a more sober reassessment of the role of science in public life. This shift was accompanied by awareness of the preponderance of political considerations in the formulation and execution of public policies. The process of democratic politics consists of endless conflicts, negotiations and temporary compromises; legislatures and public bureaucracies which, unlike private firms, are typically insulated from market-like external tests of performance, and are not under compelling pressure to integrate scientific knowledge and technical standards into their activities. Nevertheless, scientific knowledge and techniques do enter into political decisions and governmental programmes, where they have a variety of effects. These effects are, of course, often quite different from the effects expected from the use of knowledge to enhance the rationality and effectiveness of governmental actions. It is our task to understand how scientific knowledge does become woven into the political and bureaucratic formulation and execution of policies. Despite the various constraints, the integration of scientific knowledge remains a factor the weight of which varies in different contexts.

Two Approaches to the Role of Science in Public Policy

Problems such as poverty, crime, inflation and automobile accidents are conditions which policies are, at least manifestly, intended to change through measures initiated by governments. There is a distinction between the conditions which constitute the "problems" with which policy is concerned, and the complex of decisions and actions in relation to these conditions which constitute the "policy process". The most common view is that when knowledge relevant to treating problems of policy is available, the process of making policy should, and in principle can, be adjusted to ensure the maximum assimilation of that knowledge into the decisions made and actions taken to carry them out. This view has become so pervasive that it is still hard to imagine an alternative to it. Nevertheless, there is another view of the matter which assumes that the assimilation of relevant scientific and technological knowledge into the process of making and executing policy is limited, first by the fact that the technological elements of problems of policy are rarely clearly separable from the political ones, and second by the fact that the making and execution of policy regarding problems are not exclusively means of achieving technically rational goals but are also, and often incompatibly, the means of legitimating decisions politically, of furnishing publicly defensible records for the makers of policy, and of attaining other ends which are primarily political.

The first view regards any political aims and considerations in the making of policy to be obstacles to be overcome on the way to the progressive rationalisation of decisions and actions; this is the utopian rationalist view. The point of view of pragmatic rationalism, in contrast, considers the political components of the process of public policy-making as inherent ingredients which neither can nor—in so far as one is committed to a democratic political order—should be eradicated. For pragmatic rationalism the problem is not how to substitute knowledge for politics, but how knowledge can be best incorporated into political decisions, and how the knowledge appropriate to the rational attainment of substantive ends can be combined with the objectives of maintaining and increasing political power.

The pragmatic rationalist accepts, within limits, the inevitability of political ingredients in the making of policies; he sees that political considerations are not confined to the substance of the policies chosen, but are present in the decisions about what problems or conditions are to be dealt with. The identification of problems, like the policies which are devised to deal with them, are seen as part of the pursuit of political objectives which is rarely entirely in harmony with the scientific and technological approach to problems. The very selection and definition of problems, are often means to win support, divert public attention from other problems, and express certain political commitments. Even in situations where moderate expectations could enhance the likelihood of small changes, political considerations may produce grandiose promises which might elicit immediate support while diminishing the probability of success in meeting even the more modest objectives. Especially in fields such as education, welfare and energy, where intervention has produced, at best, only mixed results, politicians prefer to espouse policies which are politically effective even when they are technically irrelevant or inadequate.

> [The] evaluators of social action programmes often complain that the programmes lack any clear and concise statement of aims, a condition which they deplore because it muddies up evaluations. Their response generally has been to bemoan the imprecision and fuzzy-mindedness of the politicians and administrators, who establish the programs and then to choose a summary measure of program accomplishment which satisfies their more precise approach [but which simplifies the objectives of the programme].[1]

---

[1] Cohen, David, "Politics and Research: Evaluation of Social Action Programs in Education", in Weiss, Carol H. (ed.), *Evaluating Action Programs* (Boston: Allen and Bacon, 1972), pp. 136–137.

Dr. David Cohen is suggesting in this statement that these experts fail "to grasp the diverse and conflicting nature of social action programs". His objection is typical of the pragmatic rationalist critique of utopian rationalism. Utopian rationalism attributes the imprecision in the definition of objectives to intellectual deficiencies which can be removed by enlightenment or to parochial interests which can be unmasked by objective social-scientific inquiry. For the pragmatic rationalist, ambiguities and contradictions are necessary to the politician who addresses diverse audiences and who seeks to form coalitions of supporters without which no actions can be carried out.[2] Considering the vital role of political considerations and rhetoric in the mobilisation of support, the pragmatic rationalist tries to insert as much scientific analysis as possible into the process. For the utopian rationalist, the equals who possess scientific knowledge are always fighting a war against "politics", whereas for the pragmatic rationalist, politics is a reality to which he must learn to accommodate himself.[3] The results of sophisticated research in areas such as the production of energy, or military technology or the use of intelligence tests in education, are often integrated into the political arguments for particular policies without regard to their intellectual merit. The "careers" of the Moynihan and the Coleman reports, or the effects of the "I.Q. controversy" on the uses of intelligence tests, are illustrative.[4] They indicate how the political use of the knowledge of experts influences its application in the handling of problems.[5]

The Limits of the Two Approaches

The utopian and pragmatic rationalist approaches are not mutually exclusive. Nevertheless, by comparison with pragmatic rationalism the utopian rationalist approach to the assimilation of scientific knowledge into the making and execution of policy would be instructive only in very rare and limited cases.

The pragmatic rationalist knows that the "scientific knowledge" which is used by politicians and others engaged in contentions over policy might not be what qualified scientists would be willing to acknowledge as valid. It is, moreover, often the prestige of scientific knowledge rather than scientific knowledge itself that politicians and others draw upon to justify their positions and to discredit their adversaries.

---

[2] See Page, Benjamin I., "The Theory of Political Ambiguity", *The American Political Science Review*, LXX, 3 (September 1976), pp. 742–752.

[3] For an exceptionally instructive discussion of the conduct of individual scientific advisers, see Jones, R. V., "Temptations and Risks of the Scientific Adviser", *Minerva*, X, 3 (July 1972), pp. 441–451.

[4] See Rainwater, Lee and Yancey, William L., *The Moynihan Report and the Politics of Controversy* (Cambridge, Mass.: MIT Press, 1967); Cronbach, Lee J., "Five Decades of Public Controversy over Mental Testing", and Ezrahi, Yaron, "The Jensen Controversy: A Study in the Ethics and Politics of Knowledge and Democracy", in Frankel, Charles (ed.), *Controversies and Decisions* (New York: Russell Sage Foundation, 1976), pp. 123–170.

[5] The political uses of the authority of scientists are illustrated, for example, in the controversy about the role of nuclear physicists in the foreign and defence policies of the United States after the Second World War. See Stern, Philip M., *The Oppenheimer Case: Security and Trial* (Evanston: Harper and Row, 1969); and also the selection of papers and documents in Grodzins, Morton and Rabinowitch, Eugene (eds.), *The Atomic Age: Scientists in National and World Affairs* (New York: Simon and Schuster, 1965).

In contrast with the utopian rationalist, according to whom the validity of scientific knowledge should compel its acceptance and application where it is technically relevant, the pragmatic rationalist sees that the structure and complexity of scientific knowledge rarely permits completely unambiguous application and that the progress from intellectual diagnosis to practical action is full of uncertainty and an unavoidable mixture of scientific and non-scientific judgements. The pragmatic rationalist is more likely to appreciate the fact that the uncertainty and the non-scientific judgements which are involved in the uses of knowledge in public policy tend to facilitate the exploitation of the prestige of science to make assertions which lie beyond what is permissible by established professional standards. Such extensions of scientific authority are in fact very common. The conduct of the antagonists in the controversy about intelligence tests in the early 1970s is a case in point.[8]

But even when the validity of the particular bit of scientific knowledge is well established among scientists and the recommendations of experts are widely accepted, politicians are often more interested in the political value of the consensus of the scientists as a shield against external criticism, than in the intellectual and practical value of such a scientific consensus for effective treatment of the problem under consideration. Scientific validity, then, is often respected as a major ingredient of the politically usable authority of science, rather than because it meets scientific standards.

The degree of agreement or disagreement within the respective groups of scientists and policy-makers is important in determining the roles and uses of scientific knowledge in public policy. The exponents of the utopian rationalist outlook limit themselves unwittingly to only one of the possible relationships between politics and scientific knowledge, namely, the situation in which both political objectives and the relevant scientific knowledge are unambiguous and agreed upon. Such situations encourage belief in the feasibility of replacing science for politics. This, however, is only an illusion created by the unusual circumstances where consensus among politicians about what is desirable leaves the field free to scientific analysis of the most effective means of reaching the agreed-on political objective.

Agreement on Goals with Scientific Consensus

There are policies the ends of which are relatively clear and agreed upon and policies with ends which are both ambiguous and controversial. And there are problems of policy on the factual aspects of which scientists agree and others on which they are not in scientific agreement. Where both goals of policy and scientists' opinions are consensual, the agreement on goals tends to remove the process of making policy from the arena of political contentions, and the task of experts is mainly to give advice on how to execute it most effectively and economically.

---

[8] Cronbach, L. and Ezrahi, Y., in Frankel, C. (ed.), *op. cit.*, pp. 123–170.

Such situations, where the objectives of policy are, at least temporarily, settled and the focus of attention is on execution, tend to generate a demand for expert scientific and technological advice and to be relatively favourable to their application. When scientific advisers agree on the relevant facts and courses of action, the problem can be defined as technological; it is a matter of applying established knowledge to achieve accepted ends. Instances of the role of science in public policy which approximate these conditions, like the Manhattan project or the Apollo programme, are usually cited as the most dramatic illustrations for its success. But they are often illegitimately construed as a guiding standard for the use of scientific knowledge in the formation and execution of policies which usually lack these congenial conditions. Policies such as those pursued in dealing with education, welfare, the economy or crime, do not have available to them the scientific and technological knowledge and the agreement on objectives which were available or attained in the construction of the atomic bomb or during the first years of the space programme. Professor Arthur Jensen's equation of the task of promulgating and carrying out effective educational programmes with engineering tasks like building sound bridges and aircraft is a typical extension of this scientistic approach to areas where agreement on objectives and agreement on the scientific knowledge of the conditions to be dealt with are either lacking or partial.[7] Such a scientistic or engineering approach to social problems is reasonable only in the rare cases where there is consensus about both political objectives and the means of their attainment. The development of military technology has produced several such cases. The field of public health, where political consensus about the desirability of preventing or treating certain, mainly epidemic, diseases often coexists with scientific agreement on the best medical procedure, is another example. In much of social and economic policy, such a combination of political and scientific consensus is by no means common.

Agreement on Objectives without Scientific Consensus

Another type of situation is that in which there is consensus about the right objectives of policy but no agreement among scientists as to the facts of the situation or the means to be employed. The proper role of the scientists in this case is to make clear the range of reasonable scientific judgements. Disagreements among scientists on scientific questions give more freedom to politicians and administrators to interpret scientific opinion and to select measures to be used. At the same time, these scientific uncertainties leave room for potential political opponents to revive political contention over the objectives of policy. If time were not a consideration, further scientific research and further discussion among scientists might produce scientific consensus. In most cases, however, political and administrative schedules do not leave time for further scientific and technological research. One question then is how scientists can assist politicians and administrators to take into account scientific and technological considerations, despite unsettled differences among scientists on important matters.

---

[7] Jensen, Arthur R., "How Much Can We Boost I.Q. and Scholastic Achievement?", *Environment, Heredity and Intelligence*, Reprint Series no. 2 (June 1969); also *Harvard Educational Review*, XXXIX, 1 (Winter 1969), p. 3.

The controversy in the United States regarding the distribution of the Salk vaccine for immunisation against poliomyelitis illustrates this state of agreement about objectives in combination with scientific dissensus. The threat of poliomyelitis and the need to take preventive measures were increasingly recognised in the United States with the rise in the incidence of the disease in the early 1950s.[8] The question of politically and organisationally acceptable methods for producing and distributing the vaccine was subject to considerable debate, but this was largely resolved by the Poliomyelitis Vaccination Assistance Act of 1955. Although the Act signified consolidation of support for the objective of preventing poliomyelitis epidemics with the necessary organisational measures, qualified scientists were still divided over the purely scientific questions of the efficacy and safety of the Salk vaccine. During the controversy, which ended with the introduction of the Sabine vaccine, the execution of the policy of large-scale immunisation was constrained by the absence of clear scientific consensus on the best methods of treatment.

The "battery additive" controversy and the early phases of the discussions on the relations between smoking and health in the United States represent similar cases of disputes among scientists which have delayed or otherwise affected the execution of policy.[9] In the "battery additive" controversy, the appearance of conflict among scientists in the interpretation of the effectiveness of a certain product was sufficient to complicate the task of governmental agencies which had to decide whether a producer was engaged in deceptive or honest advertising. Similarly, during the late 1950s and early 1960s, in the early phases of the debate concerning the effects of smoking on health, differences among scientists on the precise sense in which smoking could be said to be a " cause " of cancer and other diseases influenced governmental action.

---

[8] "Congressional Response to the Salk Vaccine for Immunization Against Poliomyelitis", *Technical Information for Congress*, Report to the Subcommittee on Science, Research and Development of the Committee on Science and Astronautics, U.S. House of Representatives, 92nd Congress, prepared by the Science Policy Research Division of the Library of Congress (Washington, D.C.: U.S. Government Printing Office, April 1971), pp. 309–336.

[9] See Lawrence, Samuel, A., "The Battery Additive Controversy", in Bock, E. A. and Campbell, A. K. (eds.), *Case Studies in American Government* (Englewood Cliffs, N.J.: Prentice Hall, 1962), pp. 325–368; and Reiser, Stanley Joel, "Smoking and Health: The Congress and Causality", in Lakoff, Sanford A. (ed.), *Knowledge and Power: Essays on Science and Government* (New York: The Free Press, 1966), pp. 293–311.

Although the scientists involved could not meet the requirement of certainty, they agreed on assigning sufficiently high probability to the connection between smoking and various diseases—especially cancer—to warrant public action. But to laymen, who typically identify science with certainty of knowledge, even high probability was not persuasive enough. Such doubts made it possible for the opponents of governmental efforts to reduce smoking to claim that the assertion that smoking is dangerous to health lacked a sufficient basis in scientific knowledge.

Scientific consensus cannot be mechanically measured and complete dissensus is not the only alternative to complete consensus. Public misunderstanding of the nature of scientific knowledge can easily lead to situations where even what scientists regard as a fairly high degree of consensus among themselves is construed by laymen as a damaging manifestation of disagreement among experts. These unrealistic expectations of absolutely complete consensus among scientists can easily be exploited by politicians and publicists who wish to discredit their political opponents by pointing out that the scientific evidence upon which they base some of their claims is ambiguous and that the agreement among the scientists is only partial.

If scientists agree that of several alternative courses of action the anticipated effectiveness of any one is not clearly superior to the rest, politicians can interpret the situation as granting them freedom to ignore available scientific knowledge altogether. In doing so, they may fail to recognise which alternatives the scientists have agreed to reject, and they may regard the uncertainty which was restricted to a choice from a range of superior alternatives as a much broader uncertainty, which permits them to choose any course of action which they prefer for political reasons.

Scientific Consensus and Disagreements about Objectives

There is a third pattern of the relationship between political objectives and the state of scientific knowledge. Scientists might be in agreement regarding the conditions which can be dealt with by governmental action and about the probable consequences of each of several different courses of action, while the politicians might in the same situations disagree about the objectives which ought to be pursued. In the class of situations in which politicians agree on the objectives of policy and scientists agree on relevant scientific matters, the principal role of scientists is to assess the prospective efficacy of measures. In the class of situations where politicians agree on objectives while scientists disagree about the relevant scientific matters, scientists should, if they confine themselves to what they really know, explain the points of their agreement and disagreement. In the third pattern, where political objectives are controversial and scientific matters are agreed upon by the scientists, their main task should be to assist the politicians to become aware of the costs and consequences of each of the policies proposed. But in such cases, where the choice of policy is still under contention, even if the scientists are guided by strictly scientific standards, a climate of political partisanship affects attitudes towards even what is put forward as uncontroversial scientific knowledge.

Scientists can assist in the task of choosing policies in various ways. They themselves can be partisans of the competing political groups, making it appear that their scientific knowledge supports their partisan position, or they can try to be neutral experts not siding with any party to the debate but trying to enlighten the entire discussion of alternative policies and trying to make decisions better informed. Even as partisans, scientists might think that they are not being political; they might think that they are acting on behalf of ends given by their scientific knowledge and they might think that the politicians with whom they are in agreement are acting under the guidance of a scientific point of view. They are, in these conditions, espousing the standpoint of utopian rationalism in a situation which is quite inappropriate to it.

In so far as scientists in these situations are themselves on different sides of the issue, the disagreements are political and not scientific. The scientists might agree on the assessment of evidence and on the factual consequences of each of the preferred alternatives of policy, but they would disagree on the policy to be chosen. The disagreements of scientists as citizens might be a function of their different conceptions of the right policy, while being in agreement with other scientists about the facts of the situation and about the consequences of each of the alternative policies. They could in principle be very partisan in the espousal of their political views while not allowing such views to affect their scientific assessment of the factual or scientific aspects of an issue.

The interaction between science and policy during the controversy on fluoridation in the United States is an instance of the pattern discussed here. Concerning the effects of fluoridation on the human body, there was a considerable consensus among scientists that the artificial adjustment of the fluoride content of the public water supply would significantly reduce dental caries and, if done at the proper levels, would constitute both a medically effective and reasonably safe treatment of a major problem.[19] Fluoridation was endorsed in these terms by the National Academy of Sciences, the American Dental Association and the American Medical Association.

Despite this impressive scientific consensus there was considerable political controversy over the issue. In addition to the question of dental decay, questions were raised concerning the right of an individual to choose what he consumes, and the level at which this decision should be taken. In 952 referenda held on the issue in different communities between 1950 and 1966, fluoridation was rejected in 566 cases.[11]

---

[19] See "Fluoridation: A Modern Paradox in Science and Public Policy", in *Technical Information for Congress*, p. 616. See also Sapolsky, Harvey, "The Fluoridation Controversy: An Alternative Explanation", *The Public Opinion Quarterly*, XXXIII, 2 (Summer 1969), p. 241.

There are, of course, numerous further examples of cases where scientists agree that the application of a certain technique would constitute the most effective means to produce certain results, but where action is either ruled out or delayed because of disagreement about the desirability of the intended and foreseeable consequences. Although amniocentesis techniques which can be used, among other things , for prenatal identification of sex are generally agreed upon by scientists as being reliable, there is much resistance to a large-scale application of this technique because of the possible uses of such knowledge in parental decisions concerning abortions. The history of birth control pills is also an example of a treatment the technical effectiveness of which was a subject of considerable consensus, while its application was delayed because of disagreements in the moral evaluation of its effects on sexual behaviour. Politicians are sensitive to such attitudes in the electorate and they are not necessarily moved even by the unanimous views of scientists.

In July 1832, the Board of Health of New York City resisted the urging of the Medical Society of New York that it take action against a spreading cholera epidemic. Despite considerable medical agreement concerning the diagnosis and treatment of the problem, action by the Board was delayed by political disagreement which was fed, in part, by the fear that any official acknowledgement of the existence of a cholera epidemic could hurt the commerce of the city, thus having an adverse influence on its welfare. Only when the conditions of the epidemic became more visibly acute was a political consensus generated to approve the policy recommended by the physicians. This was an instance in which the scientific profession agreed about an existing state of affairs and about its remedy, but where action was delayed because a sufficient number of politicians tried to forestall as long as possible acknowledgement of the facts unanimously reported by the scientists [12]

When the credibility and influence of experts depend not on the scientific validity of their recommendations but upon the seldom realisable condition that the anticipated consequences of acting upon their recommendations will either improve the position of all involved interests, or will not alter the relative distribution of material or political resources among the contending groups, even a perfect adherence to professional standards cannot prevent political contention.

---

[11] Sapolsky, Harvey, "Science Voters and the Fluoridation Controversy", *Science*, CLXII, 3852 (25 October, 1968), pp. 427–432.

[12] Rosenberg, Charles E., *The Cholera Years* (Chicago: University of Chicago Press, 1962), p. 22.

Disagreement about Objectives Coupled with Scientific Dissensus

The expectations of the utopian rationalist are entirely inapplicable to a situation in which the goals of policy are unsettled, ambiguous or contradictory, and the scientists are also in disagreement about the things of which they purport to be able to speak with the authority of science. The absence of both the political and the scientific conditions for a technical solution is not congenial to the tasks of the scientist as a technical adviser on the best means to carry out a particular policy, or as an expert in the assessment of the anticipated consequences of alternative courses of action. Under these conditions, political and scientific disagreements tend to interpenetrate and to aggravate the controversy. Moreover, scientists who, without being supported by a consolidated scientific consensus, argue for politically controversial policies, become particularly vulnerable and tend to lose the credit previously given to them for dispassionate objectivity. Without the support of their peers, they cannot ensure the appearance that their judgements are sufficiently disinterested and that their recommendations are based on modes of analysis which are in themselves neutral with respect to the objectives of contending parties.

The controversies in the United States over the production and deployment of anti-ballistic missiles to defend American land-based nuclear weapons emplacements, and the uses of intelligence tests in the educational system are striking illustrations of the type of situation where simultaneous disagreements over political and scientific or technical issues accentuate each other.

The difficulties inherent in such situations for assuming the strict code of professional standards necessary for guiding and judging professional conduct are most clearly illustrated in the controversy around the decision to develop anti-ballistic missile systems. A leading participant in the controversy, Mr. Albert Wohlstetter, made formal appeal to the Operations Research Society of America to inquire into the professional conduct of some of his adversaries; he thus initiated an explicit consideration of the matter.[13] The Society moved to establish a committee for this purpose in November 1969, and in the report which it released in September 1971 the committee took the position that some of the scientists who testified before a congressional committee had violated professional standards and confused the ethically distinct tasks of analysis and advocacy.[14]

Although the committee did not claim that compliance with specified scientific standards would have resulted necessarily in a scientific agreement, it did insist that:

---

[13] See "The Obligations of Scientists as Counsellors: Guidelines for the Practice of Operations Research", Report and Documents, *Minerva*, X, 1 (January 1972), pp. 151–152.

[14] *Ibid.*, p. 119.

the calculations are sufficiently simple that in principle it should have been possible to reach agreement among the professionals if not on their results, at least on where their assumptions differ; and then to conduct a debate over the validity of one or the other set of assumptions.[15]

The committee—by insisting that the method and calculations of the critics of the anti-ballistic missile, and especially of Professor George Rathjens, were less reliable than those of supporters of the anti-ballistic missile, like Mr. Wohlstetter—implied that, at least according to the technical standards of operations research, the appearance of scientific disagreement produced by the critics of the anti-ballistic missile was not fully warranted and that their divergent conclusions might reflect the influence of non-scientific considerations. By regarding Professor Rathjens and other critics as unrepresentative of professional opinion, the committee in fact implied that a scientifically tenable consensus was possible in the situation under consideration. The existence of such a consensus among scientific experts—or at least, the presumption of its possibility—is necessary, if a scientific profession wishes to distinguish between acceptable and unacceptable professional practices and if it wishes to censure unprofessional conduct.

Mr. Wohlstetter's adversaries in the debate on the anti-ballistic missile did indeed respond to the attempt to evaluate their performance in the light of standards of operations research by declaring that " there never was any general agreement on the technological facts underlying the debate ".[16] While Mr. Wohlstetter and the committee of the Operations Research Society allegedly questioned the validity of the scientific statements made by the critics of the anti-ballistic missile, the latter justified their conduct by insisting that there was no scientific consensus, that scientific assertions were bound to become mixed with political ones and that no deviation from professional standards could therefore be pointed to.[17] Critics of the anti-ballistic missile, like Professors Rathjens, Weinberg and Wiesner, implied that since the operations research component of the assessment of complex technical systems like the anti-ballistic missile is not the only technically relevant basis of evaluation, even an agreement among professional operations research specialists would not warrant the claim of scientific consensus in the matter.[18]

Professor Paul Doty, a critic of the society's report, took a similar view.[19] His insistence on the "complexity of the issue" [20] and on the wide scope of relevant professional considerations suggests that he would oppose the application to this case of the norms and ethics of scientific advice which would be appropriate in situations where a professional consensus on a preferred course of action exists. Doubting that the techniques of operations research could be extended to strategic problems with the hope of narrowing " the uncertainties of assumptions and parameters ",[21] Professor Doty defined the relevant technical judgements in the matter much more widely. He required, among other things, a comprehensive evaluation of political consequences which is clearly beyond the competence of operations research.

---

[15] *Ibid.*, p. 121.

[16] *Ibid.*, p. 153.

[17] *Ibid.*, p. 153.

[18] *Ibid.*, pp. 153–154.

[19] Doty, Paul, " Science Advising and the ABM Debate ", in Frankel, C. (ed.), *op. cit.*, pp. 185–203, and " Can Investigations Improve Scientific Advice? The Case of the ABM ", *Minerva*, X, 2 (April 1972), pp. 280–294.

[20] Doty, P., *op. cit.*, 1972, p. 293.

Moreover, by pointing out that the proponents of the anti-ballistic missile had not considered what he regarded as important types of questions, Professor Doty attempted to demonstrate that his adversaries were guilty of a selective focus of attention. He insisted that it was wrong to start from one definition of expertise and to declare that those who dealt with topics beyond the competence of operations research were unprofessional in their conduct. Instead, he implied that in such complex matters the range of scientific judgements which become relevant to a choice of policy might be much wider than the supporters of the anti-ballistic missile claimed. To balance the common—although inescapable—bias of attention, which leaves out potentially important aspects of a complex issue that do not lend themselves to a particular narrowly defined professional treatment, Professor Doty praised the virtues of a comprehensive approach. But he did not consider the deficiencies of the comprehensive approach when praising it as a remedy for the bias of selective focus. As long as scientists were called upon only to assess those aspects of an issue about which there was a solid body of scientific knowledge and procedures of assessment at least one necessary condition for scientific consensus was within reach. By extending the range of things about which scientific judgement is required into areas where such knowledge is either lacking or is scanty, although such extensions may be necessary, the probability of scientific consensus and the concomitant opportunity for the influence of scientists on the making of policy are sharply reduced.

The controversy about the anti-ballistic missile and the debate around the report of the committee of the Operations Research Society of America suggest that conflicts between narrow and comprehensive definitions of the roles of scientists as experts are themselves, at least in some respects, a part of the " political game ".[22]

---

[21] *Ibid.*, p. 293.

[22] For the discussion of the uses of science in politics see Ezrahi, Yaron, " The Authority of Science in Politics ", in Thackray, Arnold and Mendelsohn, E. (eds.), *Science and Values* (New York: Humanities Press, 1974), pp. 215–251, and " The Political Resources of American Science ", *Science Studies*, I, 2 (April 1971), pp. 117–133. See also Nichols, K. Guild, " The De-Institutionalization of Technical Expertise ", in Skoie, H. (ed.), *Scientific Expertise and the Public*, conference proceedings (Oslo: The International Council for Science Policy Studies and The Institute for Studies in Research and Higher Education of the Norwegian Research Council, 1979), pp. 34–48.

The controversy over the validity of intelligence testing, which was renewed at the end of 1969 following claims made at that time by Professor Arthur R. Jensen, is another instance of the confusion engendered when there is a combination of scientific and political disagreements. In the controversy, each side claimed that its opponents had poor scientific grounds for their contentions, and argued that its own position represented a genuine scientific consensus. While the " environmentalists " typically insisted on the hypothesis of " environmental deprivation " as the valid ground for conducting compensatory educational programmes for the disadvantaged such as " Head Start ", the " hereditarians " insisted that research demonstrated that genetic-hereditary barriers to raising the intelligence quotient and the academic achievements of " disadvantaged " children warranted an entirely new approach to the issue. The controversy over intelligence tests shows that, despite the tendency of each side to describe its own position as scientifically sound while ascribing political motives to the other side, strategies of both " narrowing " and " extending " the scientific referents of the issue, even if they are guided by strictly scientific considerations, can arouse sentiments which have to be taken into account by politicians. Professor Jensen, in trying to " narrow " the range of scientifically relevant features of the issue in order to demonstrate the weight of genetic factors in accounting for average group differences in the scores on tests of intelligence, was of the opinion that his adversaries' attempts to give much weight to other factors were unscientific or, more precisely, were politically motivated.[23] But some of Professor Jensen's opponents, using the strategy of " extension ", similarly found unscientific overtones in his selective concentration on genetic factors.[24] Professor Jensen, arguing that there is a scientific consensus, insisted, along with the hereditarian thesis, that " points of disagreement [are] less fundamental and much narrower in scope than the points of agreement ".[25] For him, much of the disagreement should have been regarded as " misunderstanding ".[26] His critics who were mostly but not exclusively " environmentalists ", emphasised those uncertainties and disagreements about the " facts " which weakened his claim to represent an alleged scientific consensus. Some of them claimed that the authoritative scientific opinion was environmentalist, thus asserting that there was a genuine scientific consensus but that it was the opposite of what Professor Jensen said it was. A more moderate claim was made by the editors of the *Harvard Educational Review.* They decided to cease circulation of the original issue containing the article, regardless of the financial loss to the journal. In a letter replying to requests for offprints, they wrote:

---

[23] Jensen, A. R., *op. cit.*, p. 28.

[24] See Hunt, J. M., " Has Compensatory Education Failed? Has It Been Attempted?", Reprint Series no. 2, from *Harvard Educational Review*, XXXIX, 1 (Winter 1969), p. 149.

[25] *Ibid.*, p. 214.

[26] *Ibid.*

"The Jensen article . . . presents a view of intelligence that we feel must be read in the context of expert discussion from other psychologists and geneticists the Spring issue will contain. It is imperative that our readers be given access to the entire debate." [31] They were, in other words, pointing to a still unresolved scientific dissensus. They also required an enlargement of the range of the scientifically relevant expertise on the matter beyond the professional outlook of genetic psychology which Professor Jensen claimed to represent.

In both the controversy about the anti-ballistic missile and the controversy about intelligence testing, the attempts to delineate the extent of scientific consensus also involved claims that such a consensus of qualified scientists actually existed but was being disregarded by the opposite group. Whereas in internal scientific discourse, such groups may compete with each other in presenting what each regards as the most effective analysis, when the discussion takes place in the context of contention about public policy, their divergent views acquire a political significance which inevitably invites political assessment of competing scientific positions. But the political ramifications of scientific and technical recommendations do not necessarily warrant the attribution of political motives to those scientific advisers who happen to disagree with each other about scientific matters.

The Relative Weights of Scientific Knowledge and the Authority of Scientists

In so far as patterns of the relations between science and the making of policy are analysed as functions of the extent of consensus within the respective groups of scientists on the one side and among politicians and administrators on the other, changes in the extent of consensus should obviously alter the patterns. (The substance of the judgements of the scientists and the makers of policy may change without altering the facts of consensus or dissensus. Our discussion is limited, however, to those cases in which such changes in the extent of consensus occur and therefore transform the interaction between the scientific and political elements in the making of decisions about public policy.)

The history of the use of intelligence tests in the making and execution of educational policy is instructive. The relatively uncontroversial use of the results of Binet tests for the early identification of retarded children in Parisian schools around 1900 developed by the second part of the twentieth century into a heated controversy about the use of such tests for the racially heterogeneous school population of the United States. In the latter situation, the production and use of data on average group differences in the scores on intelligence tests appeared to some persons to undermine arguments for racial integration and equality.[28] There has been a shift from a situation in which both the objectives of policy and the validity and interpretation of intelligence tests were agreed upon, to a situation where the objectives of applying intelligence tests became controversial and finally, to one where the consensus on educational objectives as well as that on the validity and interpretation of the intelligence tests employed have both broken down.

---

[31] The letter quoted was sent to the author in a private communication. On the position of the editorial board, see also, *ibid.*, pp. 1-2.

The extension of the use of intelligence tests from Parisian schools of 1900 to the American army in 1916, and later, to American schools and universities and selective immigration agencies, was accompanied by increased disagreement concerning objectives.[29] Later on, especially after Professor Jensen's paper where average group differences in scores on intelligence tests were interpreted at least in part as indicating the hereditary transmission of traits the question of the scientific validity of intelligence tests was raised. The scientific consensus had broken down, partly under pressure of further analysis and partly under pressure of the political partisanship of the scientists themselves. Intense scientific controversy aggravated already existing political disagreement. These shifts corresponded with changes in the social definition of the role of psychologists. They started as scientific experts engaged in applying an obviously valid method to a widely acknowledged problem; then they became preoccupied with defending and reinterpreting the use of intelligence tests in the face of mounting political criticisms, which in the latest stage has split the scientific community and tainted most of the participating experts as partisan advocates.

The decision to change the hand-gun ammunition of the Denver Police Department in 1[illegible]4 presents yet another example, more limited in scope of the consequences of change in the degree of agreement and consensus.[30] The issue was raised when the police decided to cease using a certain kind of bullet which had proven insufficiently effective in impeding the person shot at from shooting back at policemen or others. The proposed bullet was to have superior "stopping effectiveness", thus improving the security of policemen. The proposed shift from the one type to the other was opposed, however, by partisans of the civil rights movement on the grounds that an increase in "stopping effectiveness" involved here also increase in probable injury to the persons shot at, as well as in higher probability of risk to bystanders. The scientific validity of statements about the properties of the alternative types of bullets and the political views regarding the relative values of "stopping effectiveness", "minimal injury", "reduced threat to bystanders", were each subject to much disagreement. The clash between the contending parties, each of whom enlisted its own scientific experts, produced an impasse; the problem was resolved by persons who were introduced as "experts on human judgment" and who attempted to analyse the assertions of experts in ballistics and medicine and used certain analytical procedures to replace the deadlocked adversarial procedure in ordering the preferences of the different groups involved in the controversy. The analysts quickly discovered that no analytical methods could uncover in the multiplicity of the politicians' preferences unambiguous criteria for a political decision. They were unable to proceed further before obtaining from the municipal council an agreement to a formula giving equal weight to the three competing criteria involved. Only then, on the basis of this exceptional political decision to suspend political procedures of choice and compromise, were the analysts able to produce a recommendation of a type of bullet which appeared to balance the requisites of "stopping effectiveness", minimal necessary injury and minimal danger to bystanders.[31] This is therefore not a case where scientific advice resolved a political disagreement. It is rather one where the readiness of politicians to suspend their disagreement by adopting an arbitrary formula furnished a condition for the acceptance of "scientific advice". Contrary to the impression given by the experts upon whose report of the case we rely here, this is an exceptional example not generalisable to the typical situation where experts have to advise genuinely divided politicians and an equally divided public. Nevertheless, regardless of whether such shifts from political disagreement to political agreement are brought about by experts or not, the case of the hand-gun ammunition of the Denver Police Department does illustrate how the suspension of political conflict changes the terms and role of the expert in relation to policy.

---

[28] See Cronbach, L., and Ezrahi, Y., in Frankel, C. (ed.), *op. cit.*, pp. 123–170.

[29] See Cronbach, L., *ibid.*, pp. 139–142. For a more complete documentation of the Lippermann-Terman debate, see Block, N.J. and Dworkin, Gerald (eds.), *The IQ Controversy* (New York: Pantheon Books, 1976), pp. 4–44.

[30] Hammond, Kenneth R. and Adelman, Leonard, "Science, Values and Human Judgment", *Science*, CXCIV, 4263 (22 October 1976), pp. 389–396.

The utilisation by politicians of the general prestige of scientists in situations where the scientists themselves gravitate towards politically partisan positions is clearly illustrated in the controversies over the nuclear test ban treaty and the supersonic transport in the United States. During the congressional hearings on the treaty in the summer of 1963, one of the congressional committees was informed that "in a poll of Nobel prize-winning scientists, there had been no opposed views recorded".[32] One observer of the controversy noted that:

> the major importance of scientific advice lay not so much in that it provided the basis of decision as that it helped create a political consensus in favor of the decision. In the process of acquiring scientific advice, the prestige and presumed objectivity of scientists were mobilized to ascribe to the treaty a certain aura of technical legitimacy.[33]

---

[31] *Ibid.*, pp. 393–395.

[32] Uyehara, Cecil H., "Scientific Advice and the Nuclear Test Ban Treaty", in Lakoff, S. (ed.), *op. cit.*, pp. 152–153.

[33] *Ibid.*, p. 153.

In the controversy over the supersonic transport, as in the controversy over the test-ban treaty, there was disagreement over political objectives as well as scientific disagreement about the technical aspects of the issues; the general prestige of science was exploited for political purposes. Here, too, scientists for the most part either played the role of partisan advocates or allowed their names to be invoked by political partisans. In March 1971, two lists of economists, one favouring and the other opposing supersonic transport, were produced by opposing senators and invoked in the political contention over the policy to be adopted.[34]

Utopian rationalists do not seriously consider the possibility of such misuse of the general prestige of science because they themselves are prone to it, and also because they think that the scientific knowledge presented to politicians and civil servants always consists of statements of demonstrated facts, which only the uninformed or the corrupt can ignore.[35] The pragmatic rationalist outlook sees that political considerations are integral to decisions which concern not only the choice of technical means to attain certain substantive ends but also serve as the means to balance competing interests and legitimate compromises without which no policy is possible in a democracy. Pragmatic rationalism acknowledges the existence of unavoidable constellations of political interests and seeks to enhance the impact of knowledge on decisions; its exponents may therefore try to explore how politicians' responsiveness to scientific authority can be used to enhance the effective role of relevant scientific knowledge.

The fact that scientific knowledge can be drawn upon to serve political ends in contentions about public policy suggests, however, that the increasing prominence and influence of scientists in the making of policy do not necessarily indicate a corresponding increase in the assimilation of scientific knowledge into the handling of problems of policy. It may very well mean, particularly when there is disagreement about the objectives of policy, that science is utilised for its political rather than for its intellectual value. It is often, rather, a political assimilation of the general prestige, not the scientific knowledge, of scientific advisers as the representatives of science.

Correspondingly, efforts to improve the scientific validity of the " scientific component " of public policy, as for example in the case of the Consumer Price Index, are resisted, sometimes precisely because frequent changes and adaptations could weaken the authority which constitutes much of the utility of such technical instruments for politicians and administrators. Thus, the publication of studies on the probable validity and margin of error in the statistics used to compute the index, although it would render the index sounder scientifically, could, at the same time, weaken its credibility as a reliable, stable reference in disputes between trade unions, business men and the government.[36]

---

[34] Clark, Ian D., " Expert Advice in the Controversy About Supersonic Transport in the United States ", *Minerva*, XII, 4 (October, 1974), pp. 416–432.

[35] A typical expression of the desire to restrict the role of scientific knowledge in public policy to the provision of reliable factual statements can be found in " The Science Court Experiment ", interim report of the Task Force of the presidential Advisory Group on Anticipated Advances in Science and Technology, *Science*, CXCIII, 4254 (20 August, 1976), pp. 653–656.

Obscuring the Extent of Dissensus

There are situations in which scientists who actually disagree on the appropriate method of dealing with a problematic condition may present to politicians the outward signs of a scientific consensus. Even when they agree that the knowledge available in certain areas contains many uncertainties, scientists sometimes wish to minimise the impression that their knowledge is uncertain; they are apprehensive lest the appearance of inadequate knowledge or the disagreements among themselves will lower their status and weaken their influence in the discussion of public problems. Politicians and civil servants for their part might be concerned that a favourable political constellation might be very transient and that conflicts beween scientists on the factual aspects of decisions about policy would be injurious to their cause. It would therefore be to their advantage to do whatever they can do to sustain an outward show of scientific consensus. Sometimes, therefore, the preferences of both scientists and politicians can encourage the concealment or understatement of disagreements among scientists, or of the inadequacy of knowledge, which would be relevant. It is the politically usable prestige of scientific knowledge and the reputation of scientists for objectivity and integrity which are drawn upon in such circumstances, at the price of actually disregarding these very values.

Examples of the attempts to create an impression of scientific consensus by administrative means are numerous. During the controversy over the nuclear test ban treaty, the then Secretary of Defense, Mr. Robert McNamara, summoned the experts to a seminar partly in order:

> to " persuade " the scientists to agree to the use of a common set of figures.... In the technical hearing sponsored by the JCAE [Joint Committee on Atomic Energy], scientists and engineers tended to use slightly different sets of figures for their respective purposes. This made comparison very difficult for policy-making purposes. The administration, therefore, tried to persuade the scientists to agree to a common set of figures which could be used as a base. This set of figures was known as " the bible "...[37]

During the debate concerning the relations between smoking and health, the Surgeon-General of the United States, confronting at an early stage diverse and incongruent scientific opinions, established an advisory com-

[36] See Ezrahi, Yaron, " The Political Context of Science Indicators ", in Elkana, Y., Lederberg, J., Merton, R. K., Thackray, A. and Zuckermann, H. (eds.), *Towards A Metric of Science* (New York: John Wiley, 1978), especially the section on the " Institutionalization of Science Indicators ", pp. 305–310. See also Kruskal, W. H. and Telser, L. G., " Food Prices and the Bureau of Labor Statistics ", *Journal of Business*, XXXIII, 3 (July 1960,) pp. 258 and 279, and " Comment " by Ewan Clague, *ibid.*, p. 280.

[37] Uyehara, C., *op. cit.*, p. 146.

mittee with the intention of precipitating a "majority" scientific opinion, which would declare smoking a serious health hazard. The Surgeon-General was keenly aware of the importance of engendering the outward signs of consensus among important scientists; he hoped that the prestige of science would bring about the support of the laity.[28] In the battery-additive controversy, the director of the United States National Bureau of Standards tried to resolve apparent inconsistencies between the judgements of various scientific experts by a prior agreement on a "public experiment" which would be binding on all parties regardless of the outcome.[29]

Even as a device against political opposition, however, the employment of the appearance of scientific consensus where it is actually absent is a dubious strategy. Experts who were not a party to such processes of adjustment are usually available to politicians who disagree with the proposed policy. Such strategy can, therefore, ultimately diminish the authority and influence of science as a political as well as an intellectual resource.

Another type of problem arises when a political change which eludes the advising scientists makes controversial policies which were formerly fixed by political agreement. When scientific advisers fail to adjust their role to such shifts in the political status of the goals of policy, and where they continue to take for granted goals which are no longer agreed upon, they can unintentionally become advocates. Instead of experts who advise on the execution of accepted policy, they implicitly strengthen one as against other competing goals. This kind of situation does not require that the scientists themselves change. They continue to adhere to strictly scientific standards while the change in the political constellation alters the political significance of their scientific contributions.

Another kind of confusion results from the utopian rationalist belief that more research can help resolve a political conflict which is resolvable only by political means. In such cases scientific advisers may try to persuade policy-makers with the zeal of missionaries instead of paying more careful attention to the complex task of making sure that scientific facts are not ignored in arriving at political compromises.

In many situations, adherence to the scientific norm of objectivity is strained by the presence of genuine disagreement among scientists with regard to the relevant facts and their interpretation. The danger here is that, instead of furnishing an adequate representation of divergent scientific opinions, the experts advocate one alternative which is not scientifically superior to the others.

In other cases where a scientific agreement on an order of alternatives ranked by their efficacy co-exists with political disagreement on the order of objectives, the adequacy of advice depends not—as in the former kind

[28] Reiser, S. J., *op. cit.*, pp. 295–297.
[29] Lawrence, S. A., *op. cit.*, pp. 13–14.

of situation—on accurate representation of diverse scientific opinions, but rather on the accessibility of the same uncontroversial body of knowledge to the partisans of the various political positions and interests.

Another constraint on the integration of scientific advice into policy-making arises from the common clash between the political need for stable and reliable knowledge and the scientific urge for constantly revising knowledge in the light of new scientific advances. Politicians, in so far as they arrive at compromises and acquire support from complex clusters of expectations and desires, are reluctant to allow changes which may disrupt the delicate political conditions necessary for particular policies and programmes, even when these changes are rational from a scientific point of view.

Contrary to the views of utopian rationalists, pragmatic rationalists would not, therefore, attempt to integrate into policies and programmes all new knowledge which is relevant. Instead they would explore the conditions under which the gains of adjusting the programme to new knowledge could be shown to offset the political costs of changing the scientific statements which they put forward.

The prestige of science will differ in accordance with differences in the extent of scientific consensus achieved in the various disciplines. In comparison with the other disciplines, the natural sciences appear more likely, though by no means uniformly, to achieve some measure of scientific consensus. Not only are they better off intellectually, but their premisses and idiom are less likely to become intertwined with the rhetoric of politicians, civil servants, and publicists than those of the social sciences. They are therefore less amenable, though they are by no means invulnerable, to abuse for political purposes. The subject-matters of the social sciences are almost always immediately and obviously the objects of political contention; this is not true of the subject-matters of the natural sciences, although in some areas the relative advantage of the latter has been deteriorating in recent years.

Conclusion

Utopian and pragmatic rationalists share a common commitment to the rationalisation of public policy by increasing the incorporation of scientific knowledge in the definition and treatment of problems about which policies are sought. They differ not so much in their aims as in their conceptions of how far these aims can be realised. The utopian rationalist is always at war with politicians and with politics. Viewing political contentions and considerations as irrational and otherwise unjustifiable constraints upon the fullest use of scientific knowledge, he seeks to overcome these constraints. For the utopian rationalist, the complete "scientificisation" of public policy-making is the ultimate good. The pragmatic rationalist is not committed to the sufficiency of scientific knowledge. He simply would like political actions to draw upon scientific knowledge where it is appropriate and not to fly in the face of established scientific knowledge. He acknowledges the limits set by politics on the uses of scientific knowledge in public affairs. He thinks that a world without competing interests and desires is impossible and he does not think it is possible to establish such a world by subjecting it to the dominion of scientific knowledge.

The attitudes of the utopian rationalist and those of the pragmatic rationalist agree in their high evaluation of the benefits of the application of scientific knowledge. The former, however, believes that the " values " which he thinks are inherent or implicit in scientific knowledge should dominate all others. Now, within the scientific community, it is possible to identify a hierarchy of values in which the pursuit and application of knowledge is conceivably the highest good. But to assume that the value of scientific knowledge and political decisions and actions based on scientific knowledge can be fixed as the highest value of the entire society is incompatible with the wisdom and the ineluctable facts of democratic politics. Such an assumption ignores the diversity of the objectives which are pursued in society, and disregards the task of political activity, which is the creation and preservation of order while pursuing practical objectives which are usually incommensurate, inconsistent and ambiguous.

Hence the very idea that the scientific adviser should act as manipulator of political arrangements to enhance the influence of knowledge is unacceptable, even in the service of the ideal of rationality. Even where the accepted role of the scientist requires that he advance the application of knowledge in politics and administration, it does not extend to all those decisions which have to be taken in political institutions regarding values such as the maintenance of order and the realisation of justice. The justifiable partial autonomy of politics *vis-à-vis* the pretensions of scientific experts rests upon the premise that there is no legitimate alternative to the publicly chosen procedures for the democratic ordering and compromise of diverse values, desires and interests.

The wisdom and prudence of the pragmatic rationalist should deter him, therefore, from being a Machiavellian in the service of truth and rationality. If from the scientific point of view, political reasons are often technically invalid reasons for action, from the political point of view the reasons of scientists and those who invoke the name of science are often equally arbitrary and much too indifferent to the social values, desires and interests to which politicians must attend. The task of scientists and politicians is neither to substitute the one for the other, nor to subordinate one to the other. It is rather to find in each case the way to fuse knowledge and policy within the limits set by the political and moral requirements of legitimacy and feasibility and by the standards of scientific truth and rationality.

Experts in Policy Processes: A Contemporary Perspective

Ben L. Martin
University of Missouri-Kansas City

An expert has typically been conceived as one with certain personal qualities of knowledge or skill. Martin suggests an alternative to the empiricist mode of viewing experts (with its faith in the ability of scientists to discover an objective truth), abandoning the notion of an expertise which confers expertness apart from the social setting. Instead he views experts and expertise in more relativistic terms. In the relativist mode, expertness is an ascribed quality, conferred by those who accept one as an expert. Partisanship enters: clients may be seen as choosing from among candidate-experts those who share their policy position, to assure receiving the right kind of advice. The marketing of supportive experts becomes part of our political life. In this very useful look at the literature of experts in policy-making, Martin observes how generally we have been dazzled into thinking naked pundits are wearing fine scientific garments.

Ben L. Martin is assistant professor of political science at the University of Missouri-Kansas City. He taught at the College of William and Mary after completing graduate studies at the Fletcher School of Law and Diplomacy, Tufts University.

In an era of accelerating complexity, "experts" are increasingly found as participants in public policy processes; this participation raises fundamental questions concerning the recruitment and roles of experts in opinion and policy processes. How does one become an expert? What conditions affect modes of expert participation? Systematic work on these topics at the level of general analysis is relatively rare and recent,[1] but case studies abound. Those of book length are introduced and concluded by analyses of varying scope,[2] while the less ambitious fill several edited volumes[3] along with studies of relationships among science, state, and society. These relationships have also been examined at length by Price and others.[4] A survey of work on the place of intellectuals in bureaucracy[5] and in the political culture[6] leads to the literature on professions and the meaning of professionalism.[7]

---

[1] D. Schooler, Jr., *Science, Scientists, and Public Policy* (New York: Free Press, 1971); R. S. Friedman, "Professionalism: Expertise and Policy Making" (New York: General Learning Press, 1971).

Throughout this literature, one is struck by the decisive importance of assumption and definition in shaping descriptive and analytical patterns. One is further struck by the inability of these patterns to account for the universe of expert-client relationships. This suggests that the search for more comprehensive analytical patterns must begin with a search for alternative assumptions and more productive definitions. This paper addresses itself to these tasks. After describing standard ways of viewing experts and after identifying the assumptions on which they are based, we shall note the pervasiveness of these views and assumptions in scientists' self-images, in the political culture, in notions of professionalism, and in analyses of experts' roles in policy processes. We shall then explore the heuristic potential of an alternative philosophical assumption, that of "relativity," defend the respectibility and logic of that choice, and employ that alternative to suggest a new definition of "expert." This definition will then be employed to cast new light on the recruitment and roles of experts in opinion and policy processes.

---

[2] E. S. Flash, Jr., *Economic Advice and Presidential Leadership* (New York: Columbia University Press, 1965); R. Gilpin, *American Scientists and Nuclear Weapons Policy* (Princeton: Princeton University Press, 1962); R. Hilsman, *Strategic Intelligence and National Decisions* (Glencoe: Free Press, 1956); R. S. Kirkendall, *Social Scientists and Farm Politics in the Age of Roosevelt* (Columbia, Mo.: University of Missouri Press, 1966); H. L. Wilensky, *Intellectuals in Labor Unions* (Glencoe: Free Press, 1956).

[3] S. A. Lakoff, ed., *Knowledge and Power* (New York: Free Press, 1966); E. T. Crawford and A. D. Biderman, eds., *Social Scientists and International Affairs* (New York: John Wiley & Sons, 1969); J. S. Dupre and S. A. Lakoff, eds., *Science and the Nation* (Englewood Cliffs, N.J.: Prentice-Hall, 1962); G. M. Lyons, ed., "Social Science and the Federal Government," *The Annals* (Philadelphia: The American Academy of Political and Social Science, 1971), Vol. 394, March, 1971; R. Gilpin and C. Wright, eds., *Scientists and National Policy-Making* (New York: Columbia University Press, 1964); P. J. Piccard, ed., *Science and Policy Issues* (Itasca, Ill.: F. E. Peacock, 1969); D. Lerner, ed., *The Human Meaning of the Social Sciences* (Cleveland: World Publishing, 1959).

[4] D. K. Price, *Government and Science* (New York: New York University Press, 1954); Price, *The Scientific Estate* (Cambridge, Mass.: Harvard University Press, 1965); C. P. Snow, *Science and Government* (Cambridge, Mass.: Harvard University Press, 1961).

[5] H. A. Kissinger, "The Policymaker and the Intellectual," *The Reporter*, 20 (March 5, 1959), 30–35; R. K. Merton, *Social Theory and Social Structure*, Rev. ed. (Glencoe: Free Press, 1957), pp. 207–224.

[6] R. Hofstadter, *Anti-Intellectualism in American Life* (New York: Alfred A. Knopf, 1963); S. M. Lipset, "American Intellectuals: Their Politics and Status," *Daedalus*, 88 (Summer, 1959), 460–486, E. Shils, *The Intellectuals and the Powers and Other Essays* (Chicago: University of Chicago Press, 1972).

[7] K. S. Lynn, ed., *The Professions in America* (Boston: Houghton Mifflin, 1965); H. L. Wilensky, "The Professionalization of Everyone?" *American Journal of Sociology*, 70 (September, 1964), 137–158; S. P. Huntington, *Soldier and the State* (New York: Random House, 1957).

Except for sociologists of knowledge (including sociologists of science), philosophers of science of the historical school, and very few other analysts,[8] writers have tended to take experts very seriously, on their own terms. That is, before and after sundry qualifications and references to nonsubstantive potential sources of the dimensions of experts' roles, most analysts take experts at their face value. By far the most common definition of "expert" is in terms of knowledge and skill, acquired through education, training, or experience. Standard reference is to personal qualities of the expert himself, regardless of setting. *Expertness* (that which distinguishes experts from others) is equated to *expertise* by definition.[9] In these terms, expertness is neither a status nor a role, for a man's knowledge and skill do not depend upon the presence of clients or the regard of others. Credentials are rarely discussed as instruments conferring expertness in and of themselves; rather, they are required as *evidence* of knowledge gained and skills acquired.

The impact of this definition of expertness in terms of knowledge upon notions of experts' roles is a result of the continued importance of a relatively pure, "natural," or "radical" empiricism in the epistemological lunges of most of us, intellectuals and nonintellectuals alike. Karl R. Popper has shown that at the heart of both empiricism and Cartesian rationalism, over which empiricism has triumphed even on the European continent as the basic of scientific knowledge, lies the doctrine that *truth is manifest*. Because nature does not lie, the truth is knowable through induction and reliance upon the senses, a process by which Aristotle, and also Bacon, meant "not so much the inferring of universal laws from particular observed instances as a method by which we are guided to the point whence we can intuit or perceive the essence or the true nature of a thing" in a consciously Platonic sense.[10] This optimistic view of the possibility of grasping reality, along with analogies from Newtonian mechanics and astronomy, has produced the notion of the expert (typically a scientist or one who "has it down to a science") as a man with access to the blueprints of reality, "dis-covering" the laws of the cosmos and society.

---

[8] Including R. C. Wood, "Scientists and Politics: The Rise of an Apolitical Elite," in Gilpin and Wright, op. cit., pp. 41–72; and perhaps P. J. Piccard, "For some purposes this sense of community among scientists is the best definition of the terms *science* and *scientists*." Op. cit., p. 9; and R. Gilpin, op. cit., pp. 3–38, 262–342.

[9] Cf. the treatment of "specialized knowledge" as a commodity produced by experts and consumed by policymakers (L. R. King and P. H. Melanson, "Knowledge and Politics: Some Experiences from the 1960's," *Public Policy*, 20 [Winter, 1972], 84). However, I. Gerver and J. Bensman distinguished between "objective" and "socially recognized" expertness in "Toward a Sociology of Expertness," *Social Forces*, 32 (March, 1954), 226.

Most philosophers of science and many of the more introspective leaders of the generation of physicists, biologists, and mathematicians that witnessed the revolution in science of the early twentieth century have identified mechanistic determinism as utopian myth. Yet the clarity and power of the empiricist vision have been sufficient to sustain it as the basis of most scientists' views of themselves, their definitions of science, and their prescriptions as to the norms and histories of their disciplines. Normal textbook histories of the disciplines portray linear progressions of accumulating knowledge, Promethean epics of expanding insight.[11] Methodological and ethical norms reinforce the image of the scientist as one achieving progressive mastery over the mysteries of the book of nature.

There is some disagreement as to the modesty of scientists facing problems of public policy. Price has emphasized the uncertainty of many, yet earlier in the same work he quoted prominent scientists' ridiculing politicians' inability to solve political problems in the same open, rational way in which scientists solve theirs.[12] Most scientists seem to share the public faith in themselves, extolling their contributions, the innocence of scientific inquiry, the beneficial sweep of science, and scientists' special endowments for bringing order and sense to the political process.[13]

An empiricist thread runs not only through scientists' views of themselves but also through the American political culture which includes attitudes toward knowledge and action, problems and solutions as well as attitudes toward politics and the political system. Despite the minor backwash of do-it-yourself fads, in an age of technological complexity, the natural impulse of bewildered "Everyman" is to call in an expert. The Jacksonian notion of the omnicompetence of the common man has had to learn to coexist with a newer respect for experts. Cultural traditions assuming the possibility of, and placing high value upon, rational understanding and manipulation of the world, reinforced by appreciation of the practical fruits of science, have led often to popular views of scientists as magicians and miracle men, tempered however by suspicion that the magicians may be fakers and by resentment that they are so necessary and important.

---

[10] K. R. Popper, *Conjectures and Refutations* (New York: Basic Books, 1963), p. 12.

[11] T. S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago, University of Chicago Press, 1970), pp. 1–3.

[12] Price, *The Scientific Estate*, pp. 102–119, 9–10.

[13] Wood, op. cit., p. 62. But see Gilpin, op. cit., pp. 28–34; and W. R. Schilling, "Scientists, Foreign Policy, and Politics," *American Political Science Review*, 56 (June, 1962), 291–295 for criticism of scientists' dispositions.

As experts are commonly defined as men of knowledge, distinctions are made according to the sources of their knowledge. The more negative elements of the cultural ambiguity with respect to experts are usually reserved for intellectuals, men who live for ideas, deriving their knowledge from formal education, reflection, imagination, and intuition. Whatever the sources, few deny the phenomenon of anti-intellectualism in American culture. Evidence is abundant, particularly from Congressional and business sources, where theorists, planners, and basic researchers are commonly ridiculed as utopian and impractical.[14] Social scientists are much more likely to be put in these categories than physical scientists, especially the latter engaged in applied research.

In contrast to these attitudes is the more nearly unqualified deference offered experts deriving their knowledge not from book learning, or book learning alone, but from practical experience, and using that knowledge to alter the physical world for progress. Here is the residue of the idea of the worth of the common man using common sense focused and fused into expertise. Several have described the dominant American problem-solving modes as the engineering approach, skill thinking, and the operator's way.[15] Because of the nature of our national experience, ends and "the facts" are normally taken as given; and expertise is applied to the problem of means. Faith in a single kind of rationality projects goals as part of nature and denies the necessity and inevitability of politics. This is the "procedural illusion," [16] or, alternatively, the growth of a "knowledgeable society," increasingly employing objective standards of truth.[17] The American ideal looks to engineer-kings, not to philosopher-kings, for superior access to the blueprints of reality.

---

[14] Kirkendall, op. cit., pp. 94–96; H. S. Hall, "Scientists and Politicians," in B. Barber and W. Hirsch, eds., *The Sociology of Science* (New York: Free Press, 1962), p. 274; Merton, op. cit., pp. 219–222; Hofstader, op. cit., pp. 3–19; E. Shils, "Intellectuals and the Center of Society in the United States," in Shils, op. cit., pp. 160–165.

[15] S. Hoffmann, *Gulliver's Troubles, or the Setting of American Foreign Policy* (New York: McGraw-Hill, 1968), pp. 146–148; W. W. Rostow, "The Fallacy of the Fertile Gondolas," in A. M. Scott and R. H. Dawson, eds., *Readings in the Making of American Foreign Policy* (New York: MacMillan, 1965), p. 302.

[16] Hoffmann, op. cit., p. 122.

[17] R. E. Lane, "The Decline of Politics and Ideology in a Knowledgeable Society," *American Sociological Review*, 31 (October, 1966), 649–662.

Empiricism infuses notions of professionalism, as well. A body of systematized, generalized knowledge is always cited as the first attribute of professions. The professional is one with access to specialized realms of reality, mysteries beyond the ken of laymen; and "the theoretical aspects of professional knowledge and the tacit elements in both intellectual and practical knowing combine to make long training necessary and to persuade the public of the mystery of the craft." [18] Professionals are also required to provide knowledge-based services so esoteric that clients usually cannot choose professionals "by a measurable criterion of competence, and after the work is done, the client is not usually competent to judge if it was properly done." [19] Medicine and law are the prototypical professions; their leavening of theory with practical knowledge, experience, and application produces a mix of intellectualism highly valued in the American cultural context. Virtually all of the other attributes of professions usually cited—corporateness, responsibility, self-regulation, and selflessness—derive from the special nature of professional expertise.

Arguments over the meaning of professionalism center upon the problem of the roles of experts in society. The problem is more focused if no less difficult when the client is the state or its officers, and concerns relationships among politicians and such professionals as military officers, economists, and public health physicians. Politicians' responses can range from Secretary of Defense Charles E. Wilson's leaving "all that military stuff" to the generals to Robert McNamara's regarding the uniformed military as his blue-collar force. Even more complex is the problem, also stemming from the empiricist tradition, of intelligence/operator relations, which finds experts with different sources of knowledge of the same area pitted against each other. Hilsman found that both operators and intelligence people subscribed to notions of knowledge and reality fully consistent with the empiricist cultural mode outlined earlier. Both felt that operators have especially relevant ways of knowing, through their "sixth sense," their "feel" for the area, that stem from practical experience and long years on the job. These were felt to be superior to the bookish sources of the intelligence man's knowledge. If all the "facts" are gathered without prejudice, reality will be well represented, and policy "solutions" will present themselves to an operator prepared by experience to receive them. There is inconsistency in that facts ordering themselves like the pieces of a broken vase ought to be manageable even by an intelligence man in an ivory tower; yet these are the intellectual sources of the familiar organizational prescription separating intelligence agencies from operating arms, visible not only in the foreign policy sub-system but also in economic and agricultural policy sub-systems and in labor union policy processes as well.[20]

---

[18] Wilensky, "The Professionalization of Everyone?" op. cit., p. 150.

[19] W. J. Goode, "Community within a Community: The Professions," *American Sociological Review*, 22 (April, 1957), 196.

The empiricist tradition, defining "expert" in terms of expertise, has informed most of the literature on science and government as well. Expert, usually scientific, input is commonly summarized in terms of three overlapping roles, those of advisor, researcher, and administrator. Entwined with history and example, analysis usually amounts to exploration of dilemmas facing experts, clients, and the political system as a whole. Surveying work on intellectuals, Wilensky found one group of writers, including Weber, Veblen, Laswell, Laski, De Man, and Burnham, which saw intellectuals acquiring increasing power and influence, and a second, including Mills, Znaniecki, and Merton, which perceived intellectuals increasingly becoming tools, servants of the powerful.[21] Both groups took experts seriously, in terms of expertise; the latter merely lamented a process of intellectual prostitution.

The problems of researcher-experts are usually expressed in terms of the impact of federal funding, secrecy requirements, and guidelines on originality, innovation, and basic research. The central problem of advisor-experts and administrator-experts is that of speaking truth to power without compromising expertise. The expert is pictured as being drawn in two opposite directions, both of which would debase his commitment to independence and truth. On the one hand are pressures for adopting the limited role of technician, abandoning those of critic and innovator. On the other hand are the attractions of the political arena, tempting the expert to forget the limits of his substantive competence and skill.

Clients' dilemmas begin with the series of decisions continuously required to delineate the boundaries of experts' roles. The decision-maker has responsibility for policy, but he faces an expert with access to elements of truth relative to the issue. Insisting that the client should set goals and the expert should develop means is rarely helpful in practice, where the two are inextricably entwined. The problem is especially troublesome

[20] Hilsman, op. cit., pp. 40–80, 99–122; Flash, op. cit., p. 306; Kirkendall, op. cit., p. 208; Wilensky, *Intellectuals in Labor Unions*, p. 264; Sorenson has said that the President will "pay more attention to the advice of the man who must carry out the decision than the advice of a mere 'kibitzer.'" *Decision-Making in the White House: The Olive Branch or the Arrows* (New York: Columbia University Press, 1963), p. 79.

[21] Wilensky, *Intellectuals in Labor Unions*, pp. 15–18.

when ends are thought to be obvious or given, requiring no difficult decisions. Next is the client's difficulty in knowing whether his expert has given him the whole truth or whether advice has been seasoned by predisposition and bias. Confusion and frustration result as well when clients receive conflicting advice.[22]

Systemic dilemmas are usually viewed in organizational terms. The search is for organizational forms which relate knowledge and power in a way that maximizes rationality and efficiency, defined as systemic adaptation to the opportunities and difficulties presented by post-industrial, "scientific society." [23] The characteristically public-administration standards are symptomatic, for intellectuals first moved into the several American governments in force with the Progressive movement early in this century, especially in Wisconsin, where Governor LaFollette employed academics from the University of Wisconsin extensively, setting a precedent which was followed in the invasion of Washington by intellectuals and academics during the New Deal. Intellectuals involved in that movement erected justifications for their participation in the reform, good government mold, exalting experts and administration at the expense of politicians and politics.

Macro knowledge-power problems are usually treated as a compound of the several expert and client dilemmas noted earlier. There is both prescriptive and descriptive disagreement concerning experts' impact on policy processes. C. P. Snow has extended one perspective by suggesting that great decisions are made secretly by a handful of men "who cannot have a first-hand knowledge of what those choices depend upon or what their results may be." [24] To the contrary, however, are views of science as an "establishment," not subject to control, and as one of the four "estates," views in which science is deeply involved in policy processes.[25] Adding to the latter perspective are frequent references to the need to keep experts "on tap and not on top" and occasional warnings about experts' tendencies to miss the forest for the trees. Disagreements as to experts' and clients' relative abilities to grasp the big picture produce contrasting suggestions for dealing with the systemic problem of ends/means determination.[26]

---

22. Schilling outlined seven client procedures which could be followed in this situation, but he noted that "they appear to have about the same promise, for better or for worse, regardless of the kind of experts involved." (Op. cit., p. 289.)

23. S. A. Lakoff, "Scientific Society: Notes toward a Paradigm," in Piccard, op. cit., pp. 51–72.

24. Snow, op. cit., p. 1.

25. Price, *The Scientific Estate*, pp. 12, 163–207..

26. H. Laski, "The Limitations of the Expert," *Harper's*, 162 (December, 1930), 106; M. Millikan suggests that social science can illuminate bases of decision even

Having traced the philosophical antecedents of standard ways of viewing experts, as reflected in scientists' views of themselves, in popular culture, and in social and political processes in a series of personal and organizational dilemmas, we may inquire whether there are not alternative ways of defining "expert" which may be useful in highlighting patterns of recruitment and role prescription obscured by the traditional, empiricist mode. Venturing from the empiricist definition requires abandonment of the notion of the ideal expert, whose expertise confers expertness regardless of social setting. There may be utility in refusing to take experts and expertise seriously, either on their own terms or on empiricist terms, that is, by adopting relativistic attitudes toward knowledge. This is probably neither as startling nor as obnoxious as it first appears, baldly stated. Behaviorism has long required skepticism when dealing with men's own versions of their affairs. Structural-functional analysis seeks patterned behavior that is not necessarily conscious on the part of the actors. Applying these perspectives to the analysis of experts both allows and requires skepticism in dealing with experts' claims of access to a graspable reality, manifest truth in the empiricist image.

There are, however, other grounds for treating experts' claims to knowledge of a neatly patterned reality with less solemnity than has usually been their due in the past. A conceptual revolution in the physical sciences occurred early in this century, in which the theretofore prevailing deterministic, mechanistic construct of Newton was abandoned in favor of contemporary *scientia* focusing more on process, action, and interaction than on objects in space.[27] The clear lines of the earlier neat blueprint of reality have wavered, blurred, and run off the page. These conceptual changes have become a deep-flowing (but not yet popular) cultural current, affecting the arts—originally considered techniques for expressing the structure of reality—and contributing to a "modern mentality which makes quantum theory acceptable to the physicist, abstract art to the artist, metaphysical poetry to the poet, atonal music to the musician, or abstract spaces to the mathematician." [28] "Relativity" has suffused the more rarified intellectual atmospheres, becoming a core perspective for theories in many fields.

---

if it cannot provide answers. ("Inquiry and Policy: The Relation of Knowledge to Action," in Lerner, op. cit., p. 167). Lakoff feels that much of the appeal of systems analysis is a result of its being perceived as a way to avoid uncertainty and hard choices. ("Scientific Society: Notes toward a Paradigm," in Piccard, op. cit., p. 67).

[27] J. S. Ackerman, "On *Scientia*," *Daedalus*, 94 (Winter, 1965), 17–18.

[28] H. Brooks, "Scientific Concepts and Cultural Change," *Daedalus*, 94 (Winter, 1965), 71.

For example, when the relationship between retinal impressions and "seeing" became problematic, psychologists' study of sensation shifted toward relativism, joining the study of perception which had always been discussed in terms of past associations, unnoticed sensations, and unconscious inference. Students of sociolinguistics began to view language as a prerational function, the structure of which influences the way one understands his environment, pictures his world, and develops his notions of causation and other relationships. The axiomatic method in mathematics no longer assumes the self-evident character of axioms or any connection to the "real world." Philosophers of science have largely abandoned the concepts of "validation" and "verification" in favor of comparative and quantitative, statistical concepts of "confirmation." Other philosophers of science of the historical school have found the concept of relativity of knowledge helpful in reappraising history of science and the scientific enterprise; and sociologists have traced interactions between science and society, suggested impacts of social forces upon scientific change, and examined the social structure of scientific communities.

These examples suggest the respectability and utility of a relativistic treatment of knowledge and notions of reality in several fields and disciplines. Yet relativism (in this context, the notion that, *for purposes of political analysis,* it is useful to assume that there is no absolute truth or that there might as well be none) can also be defended logically in terms of decision-making. For example, empiricist analyses uniformly hold that the "facts," accessible to experts if to no one else, can and should present the baseline for policy. Yet the number of facts conceivably relevant to virtually any policy issue is infinite; pure induction can only confuse and immobilize, Selection based on theory is inevitable. This is sometimes acknowledged but rarely allowed to inform analysis of experts' recruitment and roles. Facts do not speak for themselves; they do not order themselves; by themselves they imply no particular policy imperatives. Policy implications flow only from interpretation, but the same facts admit different interpretations.

The past provides no benchmark, for versions of history vie with each other. Debates about the past are politically relevant only as they figure in debates about the present, and neither is qualitatively different from guesses about the future. Moreover, it would be surprising if time were to tell, for no process need be accepted as crucial experiment, no event as decisive counterinstance. Neither does technicism offer relief from the logic of relativism. There *are* Democratic and Republic ways of paving a street, ways that differ as to financing, locating, and contracting, for example. Debates over security policy issues offer particularly instructive examples of the way in which scientists, employing differing images of the

Soviet Union, of world politics, and of the role of force, can offer conflicting, *ostensibly technical* advice concerning nuclear test bans and ballistic missile defenses.[29] Facts, trends, patterns, and policy implications are all in the eye of the beholder; invention and discovery are one.

This train of logic points to the primacy of mind sets, world views, as units of analysis of policy debates, ordering facts, definitions, values, problems, and arguments. This is slightly less startling when applied to social sciences than when applied to physical sciences because of the prevalence of vying schools of thought among most of the social sciences, but we have noted the insights that flow from this approach applied by historians even to the "hard" sciences. "Indeterminacy" has been suggested as a particular characteristic of social sciences, stemming from the greater number of contingencies facing social scientists than engineers and from the greater difficulties of predicting and simulating social reality and of testing one's plan.[30] The assumption of relativity extends indeterminacy to the whole policy process; this is consistent with consensus-building theories and with the notion of decisions as temporary, vulnerable truces among competing groups and movements.[31]

If the relativist approach is accepted, therefore, does it suggest a definition of the quality of expertness more useful than the standard empiricist definition, encountered earlier, which was expressed ultimately in terms of access to graspable truth, the blueprint of reality? One promising possibility, consistent with functional analysis, would look not to innate knowledge or skills of "experts" themselves but rather to others' notions of expertness. This reference to extrastimulus factors is consistent with developments in the study of sensation and perception noted earlier. Like all statuses, that of "expert" is a social artifact. Expertness is an *ascribed quality,* a badge, which cannot be manufactured and affected by an expert himself, but rather can only be received from another, a client. In this view, therefore, for purposes of political analysis, it is improper to speak of experts in the absence of clients. Thus, one is an "authority" on a subject especially and specifically because he enjoys *acceptance* as such. Conceiving authority as legitimacy in this context encourages focusing on expert-client relationships as units of analysis.

---

[29] Gilpin, op. cit., pp. 15–23, 262–267; C. H. Uyehara, "Scientific Advice and the Nuclear Test Ban Treaty," in Lakoff, op. cit., pp. 149–150; A. Wohlstetter, "Strategy and the Natural Scientists," in Gilpin and Wright, op. cit., pp. 202–203; "Guidelines for the Practice of Operations Research," *Operations Research,* 19 (September, 1971), 1123–1258; P. Doty, "Can Investigations Improve Scientific Advice? The Case of the ABM," *Minerva,* 10 (April, 1972), 280–294.

[30] Merton, op. cit., p. 210.

[31] It is also consistent with theories of decision-making as "satisficing" and as "successive limited comparisons." H. A. Simon, "A Behavioral Model of Rational Choice," *Quarterly Journal of Economics,* 69 (1955), 129–138; C. E. Lindblom, "The Science of Muddling Through," *Public Administration Review,* 19 (1959), 79–88.

From a pool of candidate-experts, acceptance by clients creates experts in opinion and policy processes. More than others, the size of some pools is limited by expectations of formal credentials and therefore by the structure of specialties; these may be even more limited intellectually by the distribution of orthodoxies. Still, of the one hundred thousand listed in *American Men of Science* and of the more than two million members of the American Association for the Advancement of Science, it has been estimated that perhaps two hundred to a thousand scientists are politically influential.[32] The size of many other pools of candidate-experts is more flexible because of anti-intellectualism and the cultural value placed on practical experience. For example, in the Overseas Branch of the OWI during World War II, social scientists had to compete with journalists and foreign nationals as area experts. Academic credentials were not particularly preferred over either a "nose for news" or even mere nationality and language competence.[33]

In hierarchical settings, primary acceptance of a candidate-expert by a client is sufficient. But where policy choices must be defended in several arenas, the acceptance process is more diffuse. In horizontal settings, both executive and nonexecutive, primary ascriptions are followed by efforts at multiple acceptance, for which different merchandising strategies are available. Consensus-building processes usually include clients' efforts to sell their experts to other clients.

Alternative bases for ascription of expertness vary according to the *client's image of the subject matter*. Is it science? Are problems essentially technical, admitting "solutions." Is it riddle, mystery, esoterica? The client's image of the subject matter not only shapes the recruitment process but also determines the content of the expert's role. This is the factor so often neglected in the literature on experts and professionalism.[34]

When a client's answers to the questions above are in the affirmative, that is, when partisanship potential is not perceived, the credentials of candidate-experts are likely to be taken at face value. When the client views the subject matter as value-free science, a matter for administration rather than politics, he becomes particularly dependent upon the labels of competence provided by professional societies and universities. It is for this reason that "the 'expert' not uncommonly is the person who elaborates the existing framework most ably, rather than the individual charting new paths."[35] This sense of awe is more likely to be displayed by clients toward physical scientists than toward social scientists.

---

[32] Wood, op. cit., pp. 47–48.

[33] L. W. Doob, "The Utilization of Social Scientists in the Overseas Branch of the Office of War Information," *American Political Science Review*, 41 (1947), 654–655.

[34] One of Friedman's five variables is a professional group's prestige, which is related only partly to the "perception held by laymen" of the "objective knowledge base." (Op. cit., p. 4.) This insight is neither developed thereafter nor allowed to suffuse the rest of the essay.

It is when clients take expertise seriously that those designated as "experts" perform the textbook functions noted earlier in empiricist analyses and the several attendant dilemmas ensue. More thoughtful clients may try to limit their risks and uncertainties by tending closely the conditions under which experts are used, defining issues and assumptions, taking advantage of differences among experts, and determining the limits of expertise, but as long as experts are thought to have unique access to esoteric reality, these endeavors cannot remove residues of doubt nor change the essential relationship of dependence based upon faith.

The congressional role in the military policy process during and since World War II can be understood in these terms. Because Congress has usually treated military strategy as "technical" (that is, nonpolitical), it has remained dependent upon the uniformed military for advice and alternatives consistent with military "science." Taking claims of military professionalism seriously, congressmen have not usually been inclined to raise questions concerning high-level military policy, and armed services committees have become quasi-judicial bodies trying to judge expert claims but trapped ultimately by self-imposed limitations. Just as nonscientists in the executive sometimes refer only half-jokingly to "doctor worship" and the "doctor cult," congressmen have at times resented their dependence upon the military for alternatives and have welcomed the periodic interservice squabbles as opportunities to "get the facts."[36]

Because of the congressional assumption that the intricacies of military strategy reveal themselves only to military professionals, the Armed Services Committees have usually concerned themselves with more peripheral matters and have been called "real estate committees." This is Huntington's familiar strategy/structure pattern, which should be expected to appear whenever clients are immobilized in awe of expertise.[37]

[35] Kissinger, op. cit., p. 33.

[36] L. A. Dexter, "Congressmen and the Making of Military Policy," revised in R. E. Wolfinger, ed., *Readings on Congress* (Englewood Cliffs, N.J.: Prentice-Hall, 1971), pp. 372–384.

[37] S. P. Huntington, *The Common Defense* (New York: Columbia University Press, 1961), pp. 123–135; D. S. Greenberg, "Mohole: The Project That Went Awry," in Lakoff, op. cit., p. 110.

McNamara's "revolution" in the Pentagon was based in the first instance upon his view of the uniformed military as his blue-collar force; from that primary perspective, all else could follow. The recent senatorial unwillingness to accept without question executive initiatives in military policy has been accompanied by a markedly diminished reverence toward professional military judgment.

Although a client may be able to exploit divisions among his experts, uncertainty and immobility are more likely to be his reactions when he believes that these men deal in truth, for is not truth one? In practice, consensus among one's experts serves as truth, and clients are likely to welcome it. They are vulnerable when they ignore scientific unanimity, and clients are usually eager to have scientific "solutions" to tough problems presented. This vulnerability and eagerness often combine to offer advantages to scientists, especially physical scientists, in political processes where they can capitalize upon their reputation for unqualified commitment to truth, naïveté in political affairs, and lack of experience in compromise.[38] Social scientists are probably less likely to be allowed to be "wild men," upsetting the bureaucratic routine and taking advantage of administrators' awe. Yet even when clients take experts completely seriously, they sometimes resist results of expert labors which do not positively reinforce clients' expectations and value systems.[39]

This suggests an alternative basis on which ascriptions of expertness may be made. This basis is employed whenever clients fail to be awed by a subject matter, when they think they know the subject, and when they are sensitive to partisanship. In these instances, experts are accepted who have mind sets and policy positions similar to those of the client. Partisanship is the basis for ascription. Clients take advantage of the distribution of schools of thought within specialties to find sympathetic candidate-experts, raise them up, and inject them into the policy process. In extreme cases in which an orthodoxy is particularly well entrenched in a well-organized specialty, this may prove difficult, even when clients are sensitive to partisanship. For example, in 1929 all ten professors of economics in Australia predicted disaster if the country abandoned the gold standard. Although unhappy, the Labour Government did not feel it could ignore the "experts" designated by others.[40] There is no problem, of course, when clients are already sympathetic to the orthodoxy to which a tightly organized, hierarchical specialty is committed. Usually, however, in most specialties policy perspectives are sufficiently well distributed that clients can discover sympathetic partisans suitable for the expert role. Moreover, when such a distribution is lacking in one specialty, there are usually related specialties available from which actors for the expert role can be drawn.

---

[38] Wood, op. cit., p. 61.

[39] R. K. Carter, "Clients' Resistance to Negative Findings and the Latent Conservative Function of Evaluation Studies," *American Sociologist*, 6 (May, 1971), 118–123.

[40] Lipset, op. cit., p. 470.

When it is taken to indicate that science is not at hand, or when a client is sure of himself intellectually, a sense of controversy surrounding an issue encourages sensitivity to partisanship. This sensitivity in turn affects receipt of information and relationships with others. Criticism is often brushed off as "purely political," implying that the analysis is not "scientific" and that its content need not be taken seriously. Interest group representations are commonly perceived as partisan communications. Lobbyists try to establish reputations as experts; but, with the exception of "public interest" groups like the League of Women Voters, they are rarely able to simulate complete disinterestedness. Several studies of interest groups' impact have concluded that officials are relatively free in their interactions with both lobbyists and constituents, that through mechanisms of selective attention and perception officials hear what they want to hear, that positions are rarely changed by these communications, and that officials use lobbyists more often than they follow their bidding.[41] Lobbyists have usually found it a waste of time trying to sway committed opponents and have relied upon cultivating and supporting already sympathetic officials.

Examples of the importance of clients' images of policy-relevant realities are numerous, often appearing even in sources committed to the sanctity of science, scientists, experts, and expertise. In Snow's story of Tizard and Lindemann, it is clear that the expert roles of both depended upon shifting party fortunes, Tizard rising and falling with Labour Governments and Lindemann depending upon Churchill. This is far from the empiricist moral of Snow's story, however, which is that men of truth (like Tizard) are often disastrously ignored by those in power.[42] Flash has found that the role of the Council of Economic Advisors "tends to be more peripheral and superficial and its impact less significant in cases (a) where its expertise is not dominant but only one of a number, (b) where institutional positions on possible alternatives are fixed, and (c) where decisions reflect explicit mandates to, or commitments of, one or more agencies."[43]

---

[41] R. A. Bauer, I. Pool, and L. A. Dexter, *American Business and Public Policy* (New York: Atherton Press, 1963), pp. 415–424; L. W. Milbrath, "Interest Groups and Foreign Policy," in J. N. Rosenau, ed., *Domestic Sources of Foreign Policy* (New York: Free Press, 1967), p. 250.

[42] Snow, op. cit., pp. 4–53.

The bases of clients' partisanship may be intellectual, political, organizational, or personal. These are likely to be complementary and mutually reinforcing rather than mutually exclusive, as men affiliate politically, organizationally, and personally with others with whom they are comfortable intellectually. The application of partisanship on any of these bases depends, however, on a primary perception that scientific esoterica is not at hand. In a context of interdependence, legislators often rely for advice on colleagues with similar positions, politically and intellectually. Congressional awe with respect to military strategy changes to assertiveness when domestic political and economic impact is seen to be at stake. In these "structural" cases, congressmen ignore military advice freely and handle the issues as domestic political ones, usually in patterns of distribution. Personal loyalties to a particular service can affect a congressman's willingness to accept advice from military professionals. Similarly, divisions among military men awaken a sense of controversy and invite congressional assertiveness. The spectrum of opinion among atomic scientists on the H-bomb issue in 1949–1950 also suggested to clients the applicability of partisanship, and the Truman administration was able to use and support the more hawkish scientists while ignoring and excluding the more dovish. At least implicitly, the dissenters used their scientific credentials to support their mostly nonscientific, moral, strategic arguments against the bomb. Because of the potency of credentials of this sort in an empiricist culture, they had to be tarnished and were, qualified by accusations of poor judgment and of having pushed claims to expertise beyond acceptable limits.

Some analysts have suggested that experts' influence varies inversely with saliency of issues for clients. It has been said that expert influence decreases to the extent that redistributive and regulatory patterns are operative. Experts' influence is said to increase in non-zero sum policy contexts and decrease in zero sum situations.[44] These observations point to conditions in which partisanship is likely to be operative. Yet they cannot be considered independent variables, for they depend upon prior client perceptions of the nature of the subject matter. One's health is usually of the highest saliency, yet patients facing dread diseases deliver themselves on faith into the hands of their chosen physicians, submit to prescriptions and regimens, and pay whatever is required. It is the client's estimate of the nature of his need, his problem, that determines his choice and use of experts and his willingness to sacrifice and change.

---

[43] E. S. Flash, Jr., "Macro-Economics for Macro-Policy," in Lyons, op. cit., p. 51.

[44] Schooler, op. cit., pp. 73, 233–234; Friedman, op. cit., pp. 4–5, 13.

When clients are unsure of themselves, they tend to defer to already-designated experts in recruiting new experts. This process allows the established experts the luxury and power of applying partisanship in recruitment and perpetuates and strengthens the uncertainty and loss of control of the original clients. It is in this way, apart from deliberate nepotism, that cliques of advisors appear, as experts call on others with whom they are compatible personally and intellectually. Several observers have noted this process at work in the science advisory system in the executive branch, where older scientific advisors look for younger scientists with "political sophistication," "administrative skills," and "broad-guaged views" in addition to "technical competence" and "reputation." [45]

"From the intrinsic natures of its facts, from our own natures as observers of its facts, and from the peculiar relation in which we stand towards the facts to be observed, there arise impediments in the way of sociology greater than those in the way of any other science." [46] These impediments arise in the way of all the social sciences and often color clients' perceptions of social scientists and their research. The cultural respect for and faith in science does not extend uniformly to the social sciences. Clients often consider themselves seat-of-the-pants experts on human affairs, and anti-intellectualism is more likely to be directed toward social scientists than physical scientists. The notion that physical reality is and should be considered infinitely malleable does not always find its counterpart with respect to social realities. Value assumptions and implications of social research are likely to be more obvious to clients. Theories and perspectives are less likely to pass unnoticed and unquestioned; they are more likely to be labelled "ideology" and treated with caution. From whatever sources, clients are more likely to have derived positions and perspectives on social issues, and candidate-experts more often face screening on this basis.[47] Social scientists have found that in order to be accepted by wary clients, they need display not only proper intellectual orientation but also personal loyalty and congeniality, modesty and harmlessness.[48]

---

[45] R. Gilpin, "Natural Scientists in Policy-Making," in Gilpin and Wright, op. cit., p. 9; H. Brooks, "The Scientific Advisor," in Gilpin and Wright, op. cit., p. 81; Merton, op. cit., p. 211; H. Orlans, "The Political Uses of Social Research," in Lyons, op. cit., p. 32.

[46] H. Spencer, "The Study of Sociology," in J. F. Cuber and P. B. Harroff, eds., *Readings in Sociology* (New York: Appleton-Century Crofts, 1962), p. 8.

[47] Cf. the reception afforded the Moynihan Report on the Negro family by different clients. L. Rainwater and W. L. Yancey, *The Moynihan Report and the Politics of Controversy* (Cambridge, Mass.: MIT Press, 1967).

American conservatives in this century have been particularly wary of social scientists and their research, and not without reason; for intellectuals, academics, and social scientists in particular have been drawn to the intellectual tradition of the Progressive Movement and the New Deal, to liberalism, and to the Democratic party.[49] American intellectuals in this century have been commonly driven in repulsion from bourgeois modes; and although social research can be and has been accused of being used apologetically, the strength of this repulsion has been sufficient to shape the intellectual enterprise in ways which stress the contrived nature of social structures and the notion that things could be different. As a result, although few social sciences have achieved the intellectual and structural unity of the more "developed" physical science specialties, the paradigmatic candidates comprising the disarray in the social sciences generally share liberal moorings and more or less literal debts to Marx. All of this has been instinctively apparent to conservatives, and it is why anti-intellectualism is so often attributed to Republican, business, conservative sources. So far, however, conservatives have usually been able to find minority voices among social scientists for ascription of expertness and service where expert roles are institutionalized.

Studies of economists in government have, sometimes unknowingly, provided striking confirmatory evidence of these patterns. Early in the New Deal, the Bureau of Agricultural Economics was dominated by liberal economists, interested in problems of planning, rural reorganization, rural poverty, and expansion of urban purchasing power. These commitments became the basis for the economists' friendships and enmities in Congress and among interest groups. The powerful Farm Bureau became hostile to the intellectuals when they appeared to threaten Bureau interests, while the more liberal Farmers' Union supported the BAE. Congressmen, also applying partisan standards, chose experts either from the Farm Bureau or the BAE. Vulnerability to anti-intellectualism and the increasingly hostile environment of the latter New Deal years led ultimately to the restriction and demotion of the BAE.[50] The wording of the enabling legislation for the Council of Economic Advisors indicates that, from the beginning, Congress was wary of liberal economists. The structure of the

---

[48] Doob, op. cit., p. 659; Wilensky, *Intellectuals in Labor Unions*, p. 269.

[49] Lipset, op. cit., pp. 461–466; N. Glazer, "The Ideological Uses of Sociology," in P. F. Lazarsfeld, W. H. Sewell, and H. L. Wilensky, eds, *The Uses of Sociology* (New York: Basic Books, 1967), pp. 63–77; Orlans, op. cit., p. 34.

[50] Kirkendall, op. cit., pp. 11–254.

CEA was chosen in order to limit presidential influence and admit congressional checks. The first chairman, Edwin Nourse, was more conservative than the rest of the Council and its staff, and Truman abandoned his indifference long enough to substitute liberal Leon Keyserling in the interests of consistency. Congress was so disgruntled with the Keyserling Council that it voted funds only for the first nine months of fiscal 1953, and Eisenhower's new chairman, Arthur Burns, cleaned house thoroughly not only in the Council but within the staff as well. Burns was under pressure from administration and Congress to ignore economists associated with the Democratic party as he sought a staff; likewise, many economists did not want to be stamped Republican economists. Forced to employ some staffers trained in schools of thought different from his own, Burns remained skeptical of them, prohibited them from peddling programs, and did not rely on them for evaluation or judgment. Liberal Walter Heller was chosen as chairman by Kennedy through a personal network, on the advice of Paul Samuelson. Inevitably, another house cleaning followed, as Heller filled the staff with young Keynesians with quantitative bents.[51]

Throughout sequences of relationships like these, one can find the thread of partisanship guiding clients' ascriptions of expertness. This basis for ascription also affects the content of the expert role, for clients expect services from those designated experts in less awesome disciplines different from the functions performed by experts in the more awesome. When clients are sure of themselves intellectually, they can avoid most of the dilemmas of the classic, empiricist expert-client relationship. Aware, committed clients require of their experts rationalization, justification, legitimization, and endorsement. "Backstopping" is required of intelligence men when operators are already decided upon a policy course. Intellectuals employed by unions characterize their general function as "window dressing." They are there to look good, to make the union appear modern and progressive, and to match the experts on the management side in bargaining.[52] These are clearly other-directed functions; they imply the existence of wider publics on which clients desire impact. In this "horizontal" policy context, clients use experts as persuasive instruments, borrowing authority and prestige for their ideas and perspectives. Candidate-experts are brought up from the audience, pushed to center stage, and allowed to broadcast ideas reflecting the mind sets held in common by actors and stage manager-clients.

---

[51] Flash, op. cit., pp. 1–218.

[52] Wilensky, *Intellectuals in Labor Unions*, p. 35. Other examples of the use of experts for legitimization can be found in King and Melanson, op. cit., pp. 88–92.

In this way policy debates often involve confrontations of rival experts, designated by opposing clients. To have one's expert accepted is to have one's ideas accepted. The marketing of experts thus becomes an integral part of consensus-building. The context of an empiricist culture assures that credentials will be valued *instrumentally* by clients as packaging norms. Lawyers' use of experts in court provides an excellent microcosmic example. The adversary system assures lawyers' partisanship as candidate-experts are screened and those offering the "right" answers are chosen as witnesses. The "calling" of an expert witness, his mounting the stand, and his performance for consuming publics, judge, and jury, are all graphic. The expert's credentials are cited in an effort to show that through training and experience he has acquired access to specialized knowledge and that he is representative of and respected by his peers. Similarly, opposing counsel is eager to find flaws in an expert's credentials, perhaps by exploiting anti-intellectualism ("Do you mean to say, doctor, that you have not delivered even a single baby?") or by showing that the expert's ideas are discredited by or atypical of his peers. At the least, opposing counsel demonstrates that a matter of opinion is at issue, rather than settled scientific truth; and judge and jury are confronted by conflicting expertise.

Many of these ploys and counterploys are likely to appear in policy debates. President Truman used his Joint Chiefs of Staff as defenders of his foreign policy so much that by 1951 Senator Taft declared that he no longer considered them impartial experts. Identifying their partisanship as tools of the administration, he discounted their testimony. With his military background, Eisenhower had little need for borrowed prestige of this sort, and the Joint Chiefs' public relations function diminished after 1953. Anti-intellectualism conditions experts' roles. Leon Keyserling was concerned about his lack of a doctorate in economics when nominated chairman of the CEA, and he cited at length alternative endorsements of his competence. However, given Truman's antipathy toward economists, Keyserling's less than perfect academic credentials probably did not hurt him. If a client senses vulnerability on the folk-wisdom flank, hiring an expert with flawed academic credentials may even be advisable. Agricultural economists in the BAE during the 1930's found it necessary to disclaim responsibility for their own ideas, presenting them instead as farmers' initiatives.

Selling one's expert to other sympathetic, sensitive clients is usually easy. The candidate-expert is accepted by the other clients on the basis of partisanship, and all then join in boosting the expert. Selling an expert to other clients insensitive to the play of partisanship, however, requires a straight-faced presentation of the expert and his credentials. Selling one's expert to unsympathetic, committed, other clients is usually difficult, and the usual strategy therefore is to ignore and isolate them, building consensus among the more sympathetic and the more gullible. Investigatory, background, and review hearings staged by congressional committees often amount to elaborate exercises in the merchandising of experts. With clear notions of the points to be made and the perspectives to be aired, the task of committee chairman and staff is to find spokesmen willing to express these views. Precisely as in the courtroom example, candidate-experts are screened and experts created as they are seated at the witnesses' table and injected into the policy process. Credentials are paraded and testimony is led into proper channels. Rival candidate-experts presenting themselves as witnesses or presented by opposing clients in the executive or even by dissidents on the committee can either be ignored or treated to a display of the structural advantages enjoyed by interrogators and cross-examiners.

Experts designated by investigatory commissions often find similar roles prescribed. Staff directors of the Kerner Commission on Civil Disorders hired nine social scientists ostensibly to produce a theoretical framework within which research and a report would be set. The consultants' draft, "The Harvest of American Racism," was rejected by the staff, not because of its substance, but because it was too "loose," that is, it was not dressy enough, it displayed too little aura of scientific rigor. With the theoretical document rejected, field studies, unsystematic and haphazard, continued regardless; but, with one exception, even these were finished too late to have impact on the commission's report, written by the staff, although the interview "data" did allow marbling of the text with charts and graphs. Some of the untested hunches of the social scientists were incorporated into the report, but in general theirs was a "paprika role," adding color but little substance to preparations.[53] They were clearly chosen for their ideological rectitude and used for legitimization of the final product.

When advisory committees are not vehicles for interest group interaction, in which case clients' discretion in designating membership is more limited, they can be used as forums for presenting experts as persuasive instruments. Surgeon General Terry, choosing an advisory committee on smoking and health, knew what its members would conclude. He was interested not in "fact-finding" but in political ammunition. Such a committee report would allow him to disclaim responsibility for the findings, while at the same time claiming that increased public awareness of the danger of smoking cigarettes justified his seeking legislation. In congressional hearings on the subject, "the committee stood, collectively, as a 'super-expert' to whose side, amid the conflicting statements of other scientists, the uncertain legislator might come for assurance." [54]

---

[53] M. Lipsky, "Social Scientists and the Riot Commission," in Lyons, op. cit., pp. 75–82.

Those who control access to impersonal media, especially the "prestige press," radio and television networks, and the wire services, are pivotal clients in the process of expert creation and marketing. As consumers, they represent a prime target for clients with experts to project. The efforts of a committee chairman and staff in staging a showcase hearing may have little impact if lights, cameras, microphones, and reporters are not present; if films, tapes, soundtracks, and notes are not edited properly; and if enough of the right parts of the product is not aired. Reporters' and editors' discrimination on the basis of partisanship is limited only by the extent of their commitment to formal norms of "neutrality" and "objectivity." As intellectuals and as observers and participants deeply immersed in the intricacies of policy debates, they are particularly apt to be sensitive to the partisan implications of any "event," statement, or presentation.

The media as physical facilities represent the space in which experts are advertised. Because that space is always limited, choice on the part of "gatekeepers" is inevitable. Just as those who control impersonal media draw most of our maps of the world, they also provide casts of characters. The airing of a man and his ideas introduced as or subtitled "expert" is the precise equivalent of an attorney's calling a witness to the stand. Niches are filled and transcripts enlarged in both cases by conscious choices made by vendors. This is the "status conferral function" of mass media in which prestige, authority ,and status are awarded and legitimized by media recognition and presentation.[55] Argumentative circularity appears when access to mass media is justified by reference to prestige and reputation. If news is what appears in the newspapers, public personalities are those exposed to publics. If "big names" make news, names become big by appearing in the news. Not just prestige but *identity* itself, injection into the policy process, is acquired when access to media and the label of expert are offered.

It has been noted that media frequently support the expert.[56] A number of reasons suggest themselves. As liberals, most of the working press relevant to the national public policy process (the "establishment press") seem to subscribe to the empiricist, "clean government" faith in administration, technicism, and expertise, with its suspicion of politics, politicians, and interest groups. Formal norms of journalism are similar to those of science; both believe in facts, truth, and objectivity.[57] Then too, because the "gatekeepers" usually have latitude in deciding which candidate-experts to present and the context in which they will be presented, there are incentives for reinforcing the empiricist image of the expert as objective, disinterested seeker of truth.

---

[54] S. J. Reiser, "Smoking and Health: The Congress and Causality," in Lakoff, op. cit., p. 303.

[55] P. F. Lazarsfeld and R. K. Merton, "Mass Communication, Popular Taste and Organized Social Action," in L. Bryson, ed., *The Communication of Ideas* (New York: Harper, 1948), p. 101; J. T. Klapper, *The Effects of Mass Communication* (Glencoe: Free Press, 1960), pp. 104–106.

[56] Wood, op. cit., p. 58.

This should not be taken to suggest any particular impact on mass opinion. "Limited interviews . . . indicate that neither the scientists active in political affairs nor major contemporary scientific accomplishments are visible to the public. . . . Our samples all revealed chronic difficulties in naming distinguished scientists, their achievements, or their associations with public life." [58] Other studies have shown that "facts" can be communicated without audiences' getting the moral of the tale, the implicit, intended message, and without changing basic attitudes. "[I]n regard to affective matters, explicit material is more likely to be communicated, especially among the less intelligent, than is implicit material," [59] yet more explicit material, when attended, is the most likely to encounter sensitivity to partisanship with its attendant defense mechanisms. The relatively few sensitive media consumers may appreciate news presentations constructed as elaborate, subtle morality plays, but most people remain unaware of the drama, much less the story lines and cast changes. Exceptions occur with respect to issues and experts more salient to mass publics than most of those relevant to the public policy process. Witness Dr. David Reuben's wide mass acceptance as celebrity-expert on sexual practices and Dr. Linus Pauling's impact on sales of vitamin C for the common cold. But with respect to most public policy issues, the universe of potential clients is limited to the "attentive public," that better educated segment that consumes media, especially "quality media," output at highest rates and attends policy debates most often.

Since the Enlightenment, the spirit of science has demanded rude treatment of dogma, tradition, and authority. Irreverence is required by formal norms. Surely this spirit should not be abandoned when the object of scrutiny is the behavior of soothsayers, however well-credentialed, in policy processes. Functional analysis and role theory have suggested inquiry into recruitment processes and role contents. For purposes of political analysis, (1) the assumption that concepts of truth and reality can be treated as relative and (2) the concept of candidate-experts have been employed in order to treat actors equally and to consider the effect of credentials as a variable. Empiricist studies have described at length series of dilemmas facing experts, clients, and the political system. Yet these descriptions of experts' roles and their implications clearly do not account for the universe of experts' relationships with clients. The relativist approach affords a new definition of expertness more productive than that expressed in terms of expertise, and the search for variables affecting the expert-client relationship leads to a focus on the importance of clients' images of subject matters, which explains differences in the ways in which actors are found for expert roles and differences in the content of those roles.

---

[57] Friedman, op. cit., p. 7.

[58] Wood, op. cit., p. 60.

[59] Klapper, op. cit., p. 88.

Most examples have been drawn from scientists' behavior in national policy processes. Yet most of the generalizations apply to private, as well as public, policy-making; they do not vary with level or type of political system. The analysis would undoubtedly have been less disquieting had figures less sacrosanct in an empiricist culture than scientists been employed. For example, the roles of medicine men, witch doctors, seers, and astrologists can be accounted for in the terms presented here. Yet an analysis based solely on the behavior of consultants like these would surely be criticized as being only marginally applicable to American public policy-making systems. It has been possible to be more ambitious, moreover, because a firm foundation for qualified assumptions of relativity has been laid by developments in science itself, and precedents for iconoclastic treatment of scientists and the scientific enterprise have been set by developments in the history and philosophy of science and in sociology.

At least one implication of interest to students of comparative politics is the blurring of the achievement/ascription dichotomy found so often in studies of recruitment patterns. This analysis has suggested that all recruitment is fundamentally ascriptive. Comparison can be pursued in terms of differing bases for ascription, but the notion of "achievement" merely implies high client respect for a particular source of knowledge or competence. Clients recruiting on traditional bases of family, class, or caste would argue that these bases reflect skill and competence. Indeed, in "achievement-based" systems, credentials often acquire a life of their own and are accepted with as little thought as are family and caste credentials in less "modern" systems.

It is scarcely novel to observe that experts often differ. Empiricist analyses suggest that these differences provide both opportunities and dilemmas to clients. Yet we have seen that, for clients insensitive to partisanship, the sense of dilemma is likely to outweigh the sense of opportunity for manipulation and control. It is possible to conceive of a client sensitive to others' partisanship but without personal position, aware of groundings but personally ungrounded. His might be the indecision of the intellectual, defined as one who turns answers into questions. While this sort of client is conceivable, he is surely relatively rare. For one reason, philosophical paralysis is inconsistent with the engineer's mode, the action orientation of practical men of affairs, politicians and administrators alike. For another, partisanship, as the term has been used here to denote adoption of world view or ideology, is part of most men's intellectual equipment, almost by definition. Usually therefore, to reveal the distribution of partisanship among candidate-experts to a client is to activate his own partisanship and make his affinities and interests immediately clear. Other uncertainties may remain, but the primary dilemma posed by conflicting experts has been diminished.

Because of heightened sensitivity to partisanship with respect to obvious social issues, clients' control and responsibility are rarely allowed to be threatened by social scientists. Their complaints of being ignored are testimony to the pervasiveness of client self-confidence with respect to social policy. Rather, questions of control and responsibility are more often raised with respect to clients' relationships with physical scientists, in whose input clients are typically less willing or able to discover partisanship. A wider diffusion of client sensitivity, especially with respect to candidate-experts with credentials in physical sciences, would offer clients freedom from unthinking dependence upon the dogmas of others, but it would also require that clients assume full responsibility for coping with uncertainty and risk. If the mediation of intellectual constructs is inevitable in decision-making, the choice and application of ideologies can at least be conscious and voluntary.

PART IV

EXPERTISE AND DEMOCRACY IN AMERICAN SCIENCE AND SCIENCE POLICY

BACKGROUND AND SUMMARY

The readings in Part III provided a sampling of contemporary perspectives on the relationship between democracy and expertise. Several selections that address the role of scientists in the American political process were included for purposes of illustration, but the focus was, in principle, on "experts" and "expertise" in general. Part IV, by contrast, is concerned exclusively with American scientists and specifically with the American scientific community as a "corporate" entity. The readings highlight aspects of the tension between democracy and expertise as they relate to the formulation of American science policy in the post-War period.

Concern about the role of the scientific community in American politics in the modern era was obviously—and unavoidably—stimulated by the consequences of the Manhattan Project. But it seems fair to say that many aspects of of the science-polity relationship in the United States which we consider important today were not subject to systematic inquiry until the 1960s. This is not altogether surprising since, as Donald K. Price notes in the first selection in Part IV, it took some time to recognize that the experience of the Second World War had permanently transformed the institutional arrangements linking the scientific community to the federal government.

Early on in his pathbreaking article, Price makes the provocative assertion that "science has become the major Establishment in the American political system: the only set of institutions for which tax funds are appropriated almost on faith, and under concordats which protect the autonomy, if not the cloistered calm, of the [research] laboratory" (p. 20). Money and autonomy: it is Price's thesis that a number of forces—the basic character of the constitutional system, the peculiar pattern of development of American government in general and of the federal civil service system in particular, as well as the new contractual agreement worked out under the pressure of World War II—combined to produce a set of institutional arrangements ideally suited to meet these "two cardinal needs" of the research scientist.

To explain the "unusual way in which the role of scientists in public affairs has developed" (p. 21) in the United States, and how this contributed to the emergence of a "scientific establishment", Price draws a series of useful contrasts between our experience and that of Great Britain. In Britain, an old and well-entrenched Administrative Class, traditionally dominated by men trained in classical and historical studies, has exhibited "no great interest in maintaining easy channels by which scientists could move into its membership, or universities could work closely with it on its major policy problems" (p. 23). In the United States, by contrast, the "Jacksonian revolution" served to stifle early efforts to establish a generalist, elite administrative corps, and "it was in the scientific and professional fields that the career civil service was first pushed up to the higher ranks" (pp. 23–4). American scientists thus attained "insider" status in the administrative hierarchy and with it a special opportunity to make their presence felt: "each group of scientists had one foot in government, so to speak, and one outside, and the policy views that the insiders developed would come back to the Congress from the National Academy or the scientific societies" (p. 26).

Prior to the Second World War, the potential of this network was not fully exploited. The research efforts of scientists at "the most important universities and [in] the most advanced fields of science" had been "supported more by private corporations and foundations than by government" (p. 30), and a significant element of the scientific community did not savor closer association with federal authorities. Their initial concern may have been ill-founded. In Price's view, the whole structure of the American constitutional system, as well as the weakness of the civil service, militated against top-down control "under a highly organized governing elite" and in favor of "loose coordination and experimentation in policy matters" (pp. 25, 28). But the experience of the War, when scientists entered into "a thoroughly pragmatic set of arrangements for the conduct of weapons research" (p. 33), finally convinced most in the scientific community that federal support of science need not be purchased at the price of centralized government control. "[I]n the hands of Vannevar Bush, James B. Conant, and Karl T. Compton, the government contract became a new type of federalism" (*ibid.*), and the pattern of administrative relationships institutionalized in agricultural research was rapidly extended to other areas of science.

American scientists in the post-War period have generally supported the concept and the practice of administering research and development programs by federal grant or contract. The system they helped set up has, in Price's view, some important advantages, but is not without potentially serious costs. The benefits and liabilities extend from the question of the vitality and coherence of the scientific research enterprise itself to basic issues of democratic theory. If the argument for a measure of independence in the conduct of scientific research retains validity, it is equally important to recognize that "the fundamental responsibility of government cannot be delegated" (p. 36) and that the practice of turning over administration of public functions to private institutions (in science as elsewhere) threatens to undermine that principle. Price duly notes that "in a proper sense of the word, "sovereignty" is of course not affected by [the] type of delegation" implicit in federally-sponsored scientific research (*ibid.*). But he is making a more general point here: in a democracy, the terms and conditions governing the science-polity relationship (and the use of public monies for scientific research in particular) are ultimately and properly defined by elected officials. This lesson can hardly be lost on the American scientific community today.

In the second selection in Part IV, Robert C. Wood sets out, as had Price, to explain how and why the scientific community came to occupy a privileged position in the American social and political order. The fact that they agree on the outcome and share similar objectives, however, should not be allowed to obscure important differences in the accounts of Price and Wood, respectively. Whereas Price suggests that the emergence of a "scientific establishment" was a result of the interaction of basically benign forces (i.e., the scientists' desire for support and relative autonomy in the conduct of research on the one hand and the peculiarities of the American constitutional-administrative system on the other), Wood contends that the scientific community's position of influence was born of and sustained by a more-or-less conscious campaign to manipulate the system to its own advantage.

Wood basic objective in "Scientists and Politics: The Rise of an Apolitical Elite", is to explode what he regards as a widespread and "persuasive fiction" that American scientists have become "political influentials" primarily as a result of their technical competence. Their ability "to influence policy on crucial issues at key places of leverage" (p. 47), he suggests, has been enhanced by a self-serving effort on the part of scientists to portray themselves as "apolitical" experts who happen to be much in demand in the contemporary setting. By disavowing political ambitions and "refus[ing] to behave according to conventional political practice", leading members of the scientific community have been able to deflect attention from the fact that they constitute an elite which "bids fair to displace entrenched skill groups in certain parts of the system and establish an over-all new equilibrium in the competition for power among the professions" (p. 44).

According to Wood, the elite of "politically revelant scientists" (which operates as a "small, mutually respecting and respectable group" with certain characteristics that distinguish it from the scientific community at large (p. 49)) has been able to pursue this strategy successfully as a result of "the particular affinity that group has to American political ideology" (p. 70). Scientists who venture into the political arena might appear to be entering an "alien clime" because they lack those resources—"bargaining skills acquired through business experience, propagandist skills of manipulating symbols having mass appeal, political skills involved in the negotiation of favors, and administrative or military skills applicable in the direction of large organizations" (pp. 49–50)—normally deemed necessary for success in that environment. But those who focus on these alleged liabilities do not appreciate "the acceptable theoretical posture which the scientists' apolitical behavior achieves." They "fail to recognize that the scientist's greatest opportunity for enhancing his influence lies in the continued differentiation of his skills from those of other experts" (p. 70).

In fact, Wood maintains, scientists benefit from a number of distinctive "sources of influence" not generally available to others that combine to overcome any suggestion that they are "exceptionally ill-equipped" to make their presence felt in American politics. The particular dependence of decision-makers on their specialized knowledge in an era of technological complexity enhances the influence of the scientific elite by equipping them "with special political advantages not possessed by other experts in the bargaining process" (p. 55), as does the media's portrayal of the scientist as a "disinterested party" in policy disputes (pp. 57–60), the public image of the scientist as a "miracle worker" (pp. 60–2), and the scientists self-image (reinforced by the perceptions of others) as "nature's agent" (pp. 62-6). Finally, Wood suggests, the prominent position they have come to enjoy within the executive branch, where most of the decisions "that touch scientific concerns and, in fact, the long-run fate of the political system, are made" (p. 66), provides scientists with special opportunities to play off the popular perception of their role as that of "disinterested" observers: "Spokesmen for science often protest against this structure of decisionmaking as secret choices made by a handful of men. Yet, from the point of view of maximizing their own influence, it is difficult to conceive of a more advantageous location for policy-making" (p. 67).

Needless to say, the highly critical—some might say cynical—view that the "outsider" Wood entertains of the intentions and effects of scientific participation in the political process would not command the assent of most within the scientific community. The point of view espoused by Harvey Brooks in the third selection in Part IV is presumably far more representative of the sentiments of most scientists.

The ostensible purpose of "The Scientific Adviser" is to describe the functions of scientific advisory committees and the various responsibilities scientific advisers have come to assume in federal decision-making processes as they relate both to what Brooks terms "science in policy" and "policy for science". But the essay is of interest in this context chiefly because Brooks consistently attempts to bring to light the positive benefits that flow from the participation of scientists in American politics. Quite early on, after quoting from Laski's article on "The Limitations of The Expert",[1] Brooks suggests that:

> Scientists in government have not claimed that their advice should be overriding, but they do insist on the value and importance of this advice in reaching a balanced decision on matters involving the use of scientific results. . . . While scientific advice is not free of bias, or even of special pleading, it is probably more free of prejudice than much other professional advice, and at least has the virtue of providing a fresh perspective unprecedented in government councils. (p. 80)

As this passage suggests, an important part of Brooks' objective is to defend the role played by scientific advisers against the many claims of those (like Wood) who contend that a reliance on their "expert" advice threatens to undermine the democratic basis and character of our system.

Much of Brooks' essay is devoted to an appraisal and defense of the "science advisory committee" system. The entire evolution of a hierarchy of part-time advisory groups operating at the margins of the federal administrative structure has been viewed with concern by some, but Brooks suggests that a major cause for that concern—its capacity to encourage a "bypassing of the bureaucracy" in the policy-making process—is in fact "one of the most useful and important functions" of the science advisory committee (p. 74). As for the suggestion that those advisory committees tend to be self-perpetuating cliques that do not adequately represent "some particular disciplines, certain kinds of institutions, or some points of view", in which case their supposed "virtue" of providing new ideas and perspectives might be rather limited, Brooks contends that many of these objections "are either untrue or grossly exaggerated" (p. 82)—that "the diversity of viewpoints and institutions has been much broader than the critics claim" (p. 95). In any event, he insists, the ability of these committees to reach "a large measure of consensus" on the matters before them is a "much more important" consideration for the policymaker who solicits their advice (p. 82).

---

[1] Reproduced in Part II of the Reader.

Brooks' discussion of the role of scientific advisers in their individual capacities reiterates many of the themes found in his review of the committee system. Here too, he argues, suggestions that the participation of scientists in decisionmaking processes threatens to distort those processes in a manner inconsistent with democratic principles are rather inflated. The often-criticized tendency of scientific experts to expand their role from the technical into the "political" sphere has merit, but the tendency is "inherent in the nature of scientific advising rather than a deliberate attempt at usurpation on the part of scientists" (p. 84). Ultimately, the adviser's influence in government "rests solely on the degree to which his views are verified by subsequent events" (p. 86), and this provides a natural check on whatever pretensions to power he may have.

In general, Brooks argues that the advantages of relying on the expert advice of scientists usually outweigh the potential disadvantages, and he cautions against attempts to impose "restrictive and negative" cures that that might prove worse than the supposed disease. "As long as the federal government retains the necessary competence within its own full-time administrative and technical staff, it has less to fear from advice subtly or overtly biased from self-interest than it does from the loss of the benefit of a perspective from outside government" (p. 94).

In the concluding portion of his essay, Brooks acknowledges that he paints what some might regard as "an unduly rosy picture of the beneficial effect of scientific advisers in government" (p. 95). Nevertheless, most of the analyses that suggest the effect is quite otherwise are probably based (as are the leading criticisms of scientific advisory committees) on "superficial evidence" that "will scarcely stand historical examination" (*ibid.*). The future, he acknowledges, might present new and special problems. Particularly in light of recent events, Brooks' closing statement seems quite prescient:

> It seems probable that the test of the political viability of the present network of advisory committees will come during the next few years, when budgetary ceilings may put a greater strain than ever before on the advisory committee's task of judging [between competing claimants for federal funds], and when the accountability of scientists to Congress and to the public will be more carefully probed than it has in the past. (p. 96)

During the past two decades, as the role of scientists in public affairs has taken on new dimensions and salience, the literature on the subject has expanded enormously. The host of issues raised by Price, Wood, and Brooks have more than retained their vitality as subjects of debate, and it is hardly surprising that several in-depth studies of the character of the science policy-making process have recently appeared. Two chapters from one such study by Jon D. Miller comprise the fourth selection in Part IV.

In *The American People and Science Policy,* Miller elaborates a stratified model of public participation in science policy-making and uses this model as a conceptual framework for the purposes of both empirical and normative analysis. In the first of the two chapters selected for inclusion in the Reader ("Public Participation in Science Policy"), Miller provides an overview of the characteristics of five identifiable groups of people who play roles of varying importance in the formulation of science policy: the "science policy decision-makers", "science policy leaders", the "attentive public" for science policy, the "interested public", and finally the "nonattentive public".

After a brief summary account of the "science policy decision-makers"—a "relatively small group [less than 100 individuals] of legislators and executive officers who can, individually in some cases, but usually collectively, make binding decisions concerning science policy matters" (pp. 33–4)—Miller provides a fairly detailed portrait of the roughly 3,000 "nongovernmental leaders for science policy" who play an essential role in setting the agenda for public policy relevant to science and technology (pp. 34–9). He devotes special attention, however, both in this chapter and in his conclusion, to the "attentive public" for science policy. This group, which Miller estimates comprises 20% of the adult population in the United States, is the "*de facto* public" to which appeals are made when disputes arise over science and technology policy, and it therefore has principal responsibility for providing a "democratic check on policy leaders and decisions makers" (p. 40). Members of the "interested public" share certain attributes with the group above them in his hierarchical scheme, but Miller finds that they were "unable to demonstrate adequate substantive information [relevant to scientific and technological issues] to remain in the attentiveness classification" (p. 43). For the most part, he concludes, neither this group (which also comprises roughly 20% of the adult population) nor the sizable majority (60%) of citizens who are members of the "nonattentive public" will have any role to play in the formulation of science policy, although there may be a few exceptions (see p. 48).

In the last section of chapter 4, Miller discusses the general dispositions toward science and technology on the part of the groups in his model. Survey data discloses some significant differences, but also some important points of agreement, in the attitudes of the various groups. Perhaps the most interesting finding is that although much of the public regards science as something of a "two-edged sword", most believe the benefits of scientific research outweigh the risks (pp. 51–3). Miller rejects the suggestion that "there is a strong or growing distrust of science in the United States or the beginning of an overt antiscience mood" (p. 54).

In his concluding chapter on "The Formulation of Science Policy in a Democratic Society", Miller attempts to assess the normative implications of his findings. After noting that "There is an inherent tension between the democratic values of our political system and the stratified structure of public participation in the formulation of science policy", he suggests that the size and composition of the "attentive public" is the key factor in mediating the potential conflict (p. 125). On the matter of composition, Miller argues that the attentive public for science "is not a perfect cross-section of the total public, but neither is it a major distortion"; an examination of the data "does not support the conclusion that this is an elitist group" (p. 126). As for the size of the attentive public, Miller contends that it "already exceeds the minimum . . . needed to protect its interest" and probably will "continue to increase gradually" in the future (pp. 129–30).

What bearing does all this have on the "major issue" in science policy today, "the share of federal resources for scientific research and education generally and for basic scientific research in particular"? Miller argues that, despite the latent support potentially available to it, "it is unlikely that organized science could generate an attentive public large enough to produce sufficient pressure to force a reallocation of federal resources in favor of science" (p. 130). "The high rate of success of science policy leaders in obtaining their major objectives without broader public pressure", he suggests, "has resulted in a leadership group largely unskilled in mobilization and an attentive public unaccustomed to being mobilized" (p. 133). Miller concludes that if leaders of the scientific community today hope to compete successfully for federal resources and to attain other objectives of importance to them, they might be well advised to rethink their political strategy.

The principal objective of Miller's study is to analyze problems and issues in the current relationship between scientists and the American public at large. This is also Kenneth Prewitt's central concern in the last selection in Part IV. But while Miller approaches the matter primarily from the perspective of the leaders of the scientific community (as his concluding chapter reveals), Prewitt's discussion in "Scientific Illiteracy and Democratic Theory" proceeds from the vantage point of the average citizen. The crucial question, as Prewitt poses it, is this: "What is to be the foundation of intelligent citizen participation in a society increasingly directed by scientific and technical processes often beyond the comprehension of any save the experts?" (p. 51).

Prewitt clearly is not alone in suggesting that "The late twentieth century brings to this old issue a new set of challenges" (*ibid.*). But he evinces considerable skepticism about the answer that science leaders have tended to fall back on, namely to "deepen and extend the public understanding of science" (p. 50). Public misunderstanding of science—which science leaders fear will result in such detrimental policies as "wide fluctuations in science funding" and the use of "political rather than scientific criteria for setting research priorities" (pp. 50–1)—cannot realistically be redressed by creating a society in which more citizens can read *Scientific American*. This is, in Prewitt's view, "a wish begging to be frustrated" (p. 53). Nor does the introduction into the political process of "countervailing expertise" hold forth much promise as a way of ensuring "intelligent citizen participation". Rather than helping to preserve democracy, this practice reinforces citizens' reliance on experts and is an invitation to "fraud" (pp. 56–7).

What is "the scope and form of information necessary to ensure democratic politics" given the expanding technical content of many public issues? Prewitt formulates his own, admittedly first-step answer to this question in the following terms: "To protect democracy will require framing public issues, even those with scientific and technical content, such that they are accessible to the lay citizen" (p. 57). But to make this principle effective, Prewitt suggests, will require overcoming a bias that most scientists share (and tend to impart) regarding the nature and aims of science itself: "Unless it becomes clear to citizens that the purpose of science—not at the margins, but at the core of the enterprise—are commercial, strategic, and bureaucratic, there can be no democratically involved populace" (p. 58). A basic understanding on the part of the lay public of the nonscientific consequences of science (rather than a sophisticated grasp of scientific knowledge *per se*) is the key to creating a "scientifically literate" public and thereby minimizing the tension between expertise and democracy. "Scientists do not aid in this cause by claiming that knowledge is something sought for its own sake" (*ibid.*).

In the concluding portion of his essay, Prewitt attempts to give some indication of the advantages that might accrue were the principles he has propounded embraced. A "scientifically literate" populace, he contends, would be able to play a more effective role in the policy-making process than does the public today. Prewitt acknowledges that many difficulties will have to be overcome before the attributes of the "scientifically literate" individual—in essence, a "nontechnical understanding" of basic statistical principles and an ability to evaluate science-based forecasts against moral standards (p. 60)—come to be more widely shared. But he contends that the problems may not be quite as imposing as some have been wont to suggest, and that in their efforts to ensure that "basic principles" in the contract between science and society are not violated, science leaders have done all concerned a disservice. "[T]he public knows more about science than scientists know about the public", he suggests, and "such damage as science is likely to suffer will probably more likely come from scientists' misunderstanding of the public than vice versa" (p. 63).

THE SCIENTIFIC ESTABLISHMENT

DON K. PRICE

Dean, Graduate School of Public Administration, Harvard University

(Read November 10, 1961)

Now that the federal government is spending more money on research and development than its total budget before Pearl Harbor, American scientists find it hard to figure out their new role in society. They used to assume that democracy would never be a patron of the sciences, and even after the Second World War the Executive had to urge the support of research on a skeptical Congress. But even though the last administration started to cut back on expenditures for science, it ended by quadrupling them. And this was by no means for defense alone: over those eight years the Congress multiplied the budget of the National Institutes of Health more than ninefold, giving them each year more than the President recommended. It is almost enough to make one try to apply to politics the theory of Henry Adams that science, as it becomes more abstract, increases in geometrical progression the power that it produces.[1]

In his farewell message President Eisenhower warned the nation against the danger that "public policy could itself become the captive of a scientific-technological elite." Even though he quickly explained that he was not talking about science in general, but only those parts allied with military and industrial power, this was a shock to the scientists.[2] To one who believes that science has helped to liberate man from ancient tyrannies—who in short still takes his political faith from Franklin and Jefferson and the Age of the Enlightenment—it is disconcerting to be told that he is a member of a new priesthood allied with military power.

Yet the plain fact is that science has become the major Establishment in the American political system: the only set of institutions for which tax funds are appropriated almost on faith, and under concordats which protect the autonomy, if not the cloistered calm, of the laboratory. The intellectual problems involved in this new status are likely to trouble scientists almost as much as the fears of the apocalyptic uses to which their discoveries may be put by the politicians.

The scientists are not the first, of course, to find it difficult to adjust their political ideals to the new world of technology. For example, the old corporation executive liked the great power technology had given to industry, but wished to limit the role of government on Jeffersonian principles. But the American scientist has a better right to his political nostalgia. For while the Founding Fathers had very little idea that industrial corporations would ever exist, let alone claim freedom of enterprise as a fundamental of the Constitution, some of them had a strong faith that free science would advance the cause of political freedom.

This faith of the Enlightenment tended to persist in the political thinking of American scientists, even in the period between the two World Wars, when it came to seem naive to their colleagues abroad. Even to this day they have shown singularly little interest in the conservative political theorists who have been telling them that science cannot deal with basic values or solve the major human problems, and the radical theorists

---

[1] Adams predicted that "the future of Thought, and therefore of History, lies in the hands of physicists . . . ," and went on to speculate that a rapid acceleration of thought in the direction of the abstract sciences might "reduce the forces of the molecule, the atom, and the electron to that costless servitude to which it has reduced the old elements of earth and air, fire and water. . . ." His prediction was uncanny, except for the term "costless" (*The degradation of the democratic dogma*, 277, 303, New York, 1958; first published in 1919).

[2] Quoted in *New York Times*, 22 January, 1961, p. 4E. See also the authorized interpretation of this statement by the President's Special Assistant for Science and Technology, Dr. George B. Kistiakowsky, in *Science* **133**: 355, 1961. As Chief of Staff, General Eisenhower had told the Army in 1946 that "The future security of the nation demands that all those civilian resources which by conversion or redirection constitute our main support in time of emergency be associated closely with the activities of the Army in time of peace," and advised the Army to contract extensively for scientific and industrial services (Memorandum for . . . General and Special Staff Divisions, etc., "Scientific and Technological Resources as Military Assets," 30 April, 1946).

PROCEEDINGS OF THE AMERICAN PHILOSOPHICAL SOCIETY, VOL. 106, NO. 3, JUNE, 1962

who tell them that science can, if it will only join in a political system that will give it real power over society.[3] The conservative theorists have usually supported the conventional views of those in the European parliamentary tradition who believed that major political issues should be dealt with by party leaders and career administrators, with scientists speaking on such matters only when spoken to. And the most important radicals have been the Marxists, who proposed to let science, as they defined it, determine all human values through a disciplined system that would leave no room for the disorder of liberal democracy.

If American scientists generally ignored both the conservative and radical critics of the Enlightenment, it was probably, in the main, because they were simply not interested in political theory, or even in politics. But it may have been also because neither theoretical position seemed very relevant to their practical experience. In disregard of the conservative and conventional theory, American scientists have come to have a much more direct role in high administration and in the making of policy than their counterparts in the parliamentary systems of Western Europe. (This is not to say that they had a more satisfactory role in the performance of scientific functions in the government.) And the more influence the scientists acquire, the more they now seem to work toward the dispersal of government organization and the decentralization of decisions, a trend impossible to explain to technocrats or the theorists of Marxism.

If we wish to understand the nature of our present scientific establishment, and its role in the making of public policy, perhaps we should look at the unusual way in which the role of scientists in public affairs has developed in the United States, and what its influence has been on the governmental system. That influence, I think, has been profound, not because of anything the scientists were seeking to do deliberately, at least until quite recently, but because of the special opportunities that were offered them by the nature of American political institutions. From the Jacksonian period, indeed, American scientists rarely had any distinctive opinion about politics or its relation to science; they were most often inclined to combine the anti-political prejudices of the business community with an envy of the social status of the European scientist. But while the American scientist lacked the honorific status of a member of a European Academy, he probably found it easier to play a direct role in government policy-making.

Sir Charles Snow has written with great insight of the Two Cultures, of the persisting failure of the humanists to understand the scientists or the changes they are working in the world, and of the scientists' personal and institutional difficulties in their relationship to government administrators and politicians. He has warned Americans most cogently against the naive belief that their constitutional system protects them against the dangers that face all countries as the result of the terrible weapons that scientists have put at the disposal of politicians who still think in pre-scientific terms.[4]

But in the United States we need to understand the idiosyncrasies of our institutions, not in order to admire them, but to know how to remedy their shortcomings, which were only a minor nuisance a generation ago, but may be a mortal threat today. Our television experts and editorial writers may be addicted to oratorical overconfidence in our peculiar institutions, but our scientists and intellectuals generally—and government reformers in particular—are rather more addicted to applying constitutional cures that do not fit the disease.

I suspect that Sir Charles has a special degree of popularity in the United States for a reason that he would probably disapprove. We enjoy what he writes not only because we see many important ways that it applies to us, but also because of ways in which it does not. We like it much as we like Anthony Trollope; we like to read about a scholarly world in which the classicists can still snub the scientists, and social scientists hardly exist at all, just as we like to read about the squire and the vicar and the butler. And American scientists like to imagine, as they read about the problems that scientists have when serving under the career administrators of the United Kingdom, that they can blame their own problems on lack of status in the bureaucracy.

---

[3] Maritain, Unamuno, and Ortega y Gasset represent the conservative critics of the Enlightenment: J. D. Bernal may be taken as a sample on the Socialist side. Judith N. Shklar, whose *After Utopia* begins with the observation that "nothing is quite so dead today as the spirit of optimism that the very word Enlightenment evokes," goes on (p. 3) to admit that "The less reflective public, certainly until 1914, remained cheerfully indifferent to the intellectual currents of despair. . . ." In this optimistic category, I would include most American scientists, and bring the date up to the present.

[4] See especially his *Science and government*, 55, Cambridge, 1961.

Yet a look at the main outlines of the two systems gives a different picture. In Great Britain, in spite of decades of debate about the basis of recruitment of the Administrative Class, it is still dominated by men trained in the classical and historical studies; not one man in twenty among these guardians of public policy has had a scientific or technical education. In spite of recurrent criticism of its role, it still maintains a professional monopoly (though in a studiously amateur and nonscientific way) over the organization of the government departments, and a major share of influence in the formation of national policy. It thus has no great interest in maintaining easy institutional channels by which scientists could move into its membership, or the universities could work closely with it on its major policy problems.[5]

Now that we are both constitutional democracies, it makes much less difference that Great Britain has a king and the United States a president, but a great deal of difference how we set up the professional group of men who actually run the government. Our Jacksonian revolution indeed destroyed the hopes of John Quincy Adams for a continuation of the Jeffersonian alliance between science and republicanism. At the same time, by wiping out the beginnings of a career system, it prevented the development of an elite administrative corps and thus cleared the channels of promotion for the scientists who, decades later, were to begin to move up in the civil service. The frontier radicalism of the day distrusted all forms of Establishment; this was the era in which state constitutions forbade ministers to hold public office and prohibited educational qualifications for admission to the bar. But as the business of government got more complicated, the frontier had to admit that certain skills were necessary. Its essentially pragmatic temper insisted, as it became necessary to hire civil servants for merit rather than patronage, that the requirements be defined in terms of the needs of the specific jobs, rather than by general educational status. It was easiest to prove the need for special skills, of course, in technical fields, partly on account of the objective nature of the problem, partly because scientific societies were determined to raise and maintain their professional standards in the civil service as well as in private practice.[6]

As a result, it was in the scientific and professional fields that the career civil service system was first pushed up to the higher ranks. As we developed our top civil service, we made it something quite different from a career Administrative Class; most of its members are not only nonpolitical, but nonadministrative as well, and they are not career officials in the same sense as a U. S. Navy officer or a British Civil Servant.

In recent years, scientists and engineers, while certainly rare among those in high political office, have done reasonably well in the civil service. The positions of administrative continuity and bureaucratic power in Washington are, in the civil service departments, the bureau chiefs. A study in 1958 of the 63 bureau chiefs showed that 9 of them had advanced degrees in the natural sciences, and 17 others had been trained in lesser ways as engineers or technicians. By comparison with these 26 from various branches of technology, there were 9 economists and only 8 lawyers, and 20 from miscellaneous administrative or business careers.[7] Aside from the positions of bureau chief, the top career positions are the so-called "super-grades," which were added above the regular civil service grades to let the government compete for scarce talent.[8] The favorite justification for these positions is the need to employ capable scientists and engineers, notably in the technical branches of the Defense Department and the National Aeronautics and Space Agency. Administrators have ridden along to higher salaries on the political coattails of scientists.

---

[5] McCrensky, Edward, *Scientific manpower in Europe*, 27-29, New York, 1958, gives the general picture with respect to salaries and personnel policy. As for the classic attitude of the Administrative Class regarding its relation to the scientific civil service, see the testimony of Sir Warren Fisher, Permanent Secretary of the Treasury, before the Royal Commission on the Civil Service, 1929-1930, *Minutes of evidence*, 1276, 1282. For its contemporary attitude, see Sisson, C. H., *The spirit of British Administration*, London, 1959.

[6] As A. Lawrence Lowell put it, "The great professions, which have secured general recognition in the community, have been strong enough to insist that strictly professional work must not be intrusted to men who have had no professional training or experience" (*Public opinion and popular government*, 274, New York, 1926). Detailed illustrations for specifically scientific fields may be found in the series of "Service Monographs of the United States Government" published by the Institute for Government Research, notably those on the Steamboat Inspection Service, the Office of Experiment Stations, the General Land Office, and the Public Health Service. See also Lewis Mayers, *The federal service*, Institute for Government Research, 21, 1922, and Lewis Meriam, *Public personnel problems*, 317.

[7] Smith, Michael E., Bureau chiefs in the federal government, 1958, in *Public Policy*, the Yearbook of the Graduate School of Public Administration, Harvard University **10**: 62, 1960.

Scientists who become bureau chiefs are, of course, no longer practicing scientists; they are doing work that in the United Kingdom would be done by a member of the Administrative Class educated in history or the classics. But when they are good at their jobs, as some of them are, it is for a reason that would have appealed to Macaulay, who used to argue that he wanted to recruit university graduates in the classics not because they had been studying the classics, but because the classics attracted the best minds, which could adapt themselves to anything.[9] And the American scientist who turns administrator is the equal of his English humanist counterpart in at least one respect: his lack of interest in management as a science, or sometimes at all.

But while the scientists in top civil service posts have not been deeply interested in administration, they have been interested in policy. What chance do they have to make their policy views prevail?

In their influence on policy, as in their advancement in the hierarchy, the scientists in American government had a special opportunity because they did not have to work under a tightly organized governing elite. After the Civil War, there was no strong conservative tradition based on a landed interest, and no national party with a coherent ideology, to take control.

As a result, policy tended to develop separately in every field. There was no one to tell the scientific experts that they were on tap but not on top; indeed they were listened to all the more readily because they were usually not thought of as bureaucrats. There was no one from whom Congress wanted advice less than from the regular career service. But each group of scientists had one foot in government, so to speak, and one outside, and the policy views that the insiders developed would come back to the Congress from the National Academy or the scientific societies.[10] In a government of limited constitutional powers, a research program could be justified in a given field when an action program could not. But the research ultimately seemed to lead to action, in spite of the lawyers' scruples and the lack of interest of the party machines. This was only in part because the politicians were persuaded by objective data; it may have been even more because scientists (and in some fields, the economists) were the major organized communities of professional opinion with a continuous interest in specific public programs. This is a summary of the development of many new federal programs: you can trace it in agriculture, in natural resources, in the regulation of business, in labor and welfare, and we now see its beginnings in the support of education.

The most influential pattern was set in agriculture. Washington and Jefferson had been interested in fostering scientific improvements in agriculture, and in federal support of a national university. They were blocked by the lawyers' scruples about states' rights, until the agricultural scientists found a way to get there by a different route—one that evaded constitutional barriers by merging federal and state interests through the device of federal grants to states in either land or money, and by building a program up on scientific and educational bases. The foundation was, of course, the land grant college; from it grew the experiment station, the extension program, and the whole system of policy which has let the federal government play a more effective role in the agricultural economy than the government of any supposedly socialized state. In all this development, the land grant colleges and the associations of various kinds of agricultural scientists maintained an important influence on the Department of Agriculture, supplied most of its career personnel, and generally provided the intellectual leadership for national agricultural policy. They thus in effect greatly weakened the old constitutional distinction between state and federal functions, but without subjecting the field of agriculture to the control of a centralized bureaucracy.

---

[8] United States Civil Service Commission, *The federal top salary network*, Washington, 1960.

[9] He put it more pointedly in 1833: "If astrology were taught at our universities, the young man who cast nativities best would generally turn out a superior man." Royal Commission on The Civil Service, *Fourth report*, Cd. 7338, 1914.

[10] ". . . from the beginning the membership of the Academy included many officers of the Government. . . . On one occasion at least this led to some embarrassment, for the reason that through this double relationship it was thought that the view of subordinate officers might control the action of those higher in authority" (True, Frederick W., *A history of the first half-century of the National Academy of Sciences*, 202, Washington 1913). The same fear, or hope, exists in the present relationship between the Academy and the Federal Council for Science and Technology. True's history of the Academy, and his *History of agricultural experimentation and research in the United States* (Misc. Pub. No. 251, U. S. Department of Agriculture, Washington, 1937) tell a great deal about the role of scientific societies in the development of new Federal programs.

The pattern of grants in aid, with its new set of administrative relationships, met two cardinal needs: to provide money, as well as national policy direction, from Washington, and to maintain the autonomy of the states. It accordingly became the basis on which new programs were developed—highways, public health, social security, welfare, housing, and others. This was what political scientists came to call the "New Federalism," which has given the scientists and specialists in each field of policy a chance to work out programs without too much constraint by any party doctrine.

An elite administrative corps may look on scientists as properly subordinate, and science as a way of thinking that should deal with the means to support a policy, a tradition, or an ideology, rather than an end in itself. We can understand this relationship in other countries if we recall how until recent years our military services thought that civilian scientists in military laboratories should conduct their research only pursuant to "requirements" defined by military staff work. This notion was exploded as it became apparent that what scientists discovered by unrestricted research might be of greater military importance than the things the military officers thought they wanted—in short, that the means might determine the ends.

This example provides the extreme (and almost the only conspicuous) example in American politics in which scientists have been faced with difficulties in getting a direct political hearing for their policy ideas. The typical editorial writer may still think in terms borrowed from the experience of parliamentary constitutions with tightly knit administrative elites, but all the habits of American public life run on a different pattern.

Its constitutional peculiarities are typified in one trivial incident: in a recent Congressional hearing, a friendly Representative addressed the newly appointed political head of the National Aeronautics and Space Administration, to his mild embarrassment, as "Doctor." In a legislature that is supposed to distrust eggheads, a Congressman often wants his advice on a specific program undiluted by either party doctrine or the policy views of general administrators; he is so conditioned to go directly to the scientific expert whenever he can that he sometimes treats his witnesses as experts even when they are not. This constitutional model is worth looking at with more critical sympathy. Its essential parts —none of which exists in the classic parliamentary system—are the standing Congressional committee that considers policies without being bound by party doctrine; a chief executive who is elected independently of the legislature on a nonideological platform so that he can tolerate loose coordination and experimentation in policy matters; and a civil service which lets scientists move freely up into top administrative positions, and in and out of government, thus maintaining a continuous interchange of men and ideas between the government and universities. This system makes it impossible to maintain an institutional distinction between ends and means, between policy decisions on the one hand, and on the other hand scientific research or administration. Hence it makes party responsibility in the parliamentary sense impossible, and it greatly complicates the task of coordinating either policy or administration.

On the other hand, to deny the distinction between ends and means is a part of the scientific approach: no scientist likes to feel that his basic values and objectives have been set by others so rigidly that he cannot follow where his research leads him. It was, after all, the purpose of the Enlightenment to free both politics and science from the monarchical and ecclesiastical institutions that defined traditional values.[11] It may be even more necessary to deny the distinction between ends and means, in an institutional sense, in the twentieth century, when it is the requirements of new ideology, rather than old orthodoxy, that threaten freedom. For science itself, by introducing so many complexities into public policy, destroyed the comfortable nineteenth-century notion that public issues could really be determined by the parliamentary competition of two opposing doctrines. At the same time, it made possible, by the development of new techniques of mass communication, the means for producing disciplined support of authoritarian government. If the structure of political institutions does not specifically encourage some social experimentation based on scientific initiative, with some degree of deliberate freedom from the constraints of policy as determined by either partisan theorists or an administrative elite, it will narrow the range of free scientific and political development. Perhaps our eighteenth-century Constitution, with its implied distrust of party discipline, will yet prove to be more adaptable to our scientific era than the classic nineteenth-century parliamentary model of Walter Bagehot or Woodrow Wilson.[12]

---

[11] Frankel, Charles, *The case for modern man*, 58, Boston, 1955. Ernst Cassirer noted "the almost unlimited power which scientific knowledge gains over all the thought of the Enlightenment. . . . A . . . deeper insight into the spirit of laws, of society, of politics, and even of poetry, seems impossible unless it is pursued in the light of the great model of the natural sciences" (*The philosophy of the Enlightenment*, 45–46, Boston, 1955).

At any rate, it is easy to guess why large groups among American scientists—especially in the agricultural sciences—were less pessimistic in the period after the First World War than their European colleagues with respect to the role of science in democratic politics. In two very practical ways their situation was entirely different; in civil service, their advancement was not blocked by a career bureaucracy, and the constitutional system gave them a chance to advocate policies in comparative freedom from administrative or political discipline. It was no wonder that they had not lost faith in the political approach of the Enlightenment, for they had made it work.

Nevertheless, by the time of the Great Depression this naive faith was least prevalent in the most important universities and the most advanced fields of science. In them, science was supported more by private corporations and foundations than by government, and its leaders in newer fields like nuclear physics and biochemistry had closer intellectual ties with their European counterparts than with the agronomists or engineers of the land-grant colleges. For the loose American constitutional system had worked best in those aspects of public affairs in which the power of government and the power of the great industrial corporations were not in rivalry. The scientists in institutions that derived their support from industrial wealth and were interested in problems of the industrial urban economy saw the constitutional model in a different political perspective. Among them, accordingly, were to be found both those conservative scientists who were most distrustful of government, and those radicals who tended to take a Marxist view of the role of science in society.

The Depression had thus made it impossible for the American scientist to avoid the second challenge, explicit in Marxism, with respect to the significance of his role in society: does science as it grows in importance lead us away from constitutional liberalism, and require party dictatorship? In a society of growing complexity, is not an increase in the role of government inevitable, and does not that inevitably lead to a centralization of power that will destroy democratic freedom?

These are still troublesome questions, but they are being discussed on a somewhat higher level of sophistication than three decades ago. The change has come about partly because scientists, under the pressure of the Second World War, worked out a new type of contractual relationship that has brought private scientific institutions into a connection with the federal government as intimate and active as that of any land-grant college. And the extension of this system to industrial corporations may now be bringing about a new relationship between government and business following the quarrels of the Depression era, much as the grant-in-aid system transformed federal-state relations after the Civil War.

Before going into the nature of this new system, let us note two peculiarities of American politics that made it possible.

The first was the assumption that it was just as appropriate for the voters and legislators to control the administrative organization and procedures of government as its policies, that is to say, the means as well as the ends. This was a radical departure from British or European assumptions. The political progression from conservatives to liberals to socialists never changed the fundamental European assumption that, while governments might be responsible to legislatures for the substance of their policies, it was better for politics and legislation not to meddle with internal administrative organization, or the management of the bureaucracy. The socialist political leaders took the unity of the state and its bureaucracy for granted. If anything, they tended to make it all the more monolithic, and to push to its logical conclusion the tendency of Benthamite liberalism to abolish the privileges of guilds and public corporations. But in the United States the current of radicalism ran in the opposite direction; after the age of Jackson, lobbyists and legislators were likely to concern themselves at least as much with the details of administrative organization as with major policies, generally with the purpose of creating centers of independence within government. This tendency was pushed so far that it destroyed the unity of administration, and had disastrous effects on the competence and the political responsibility of government. But it also made it a mistake to assume—as was often assumed both by those who admired and those who feared socialism—that an extension in the scope of governmental business in the United States would automatically involve a corresponding centralization of power.

---

[12] Bagehot, Walter, *The English Constitution*, and Wilson, Thomas Woodrow, Cabinet government in the United States, *The International Review*, August, 1879 (reprinted 1947 by the Overbrook Press, Stamford). By the turn of the twentieth century, Wilson had apparently changed his mind, in view of the new role of the presidency, especially in international affairs: *Congressional government*, Preface to 15th edition, 1900.

The second peculiarity of American politics was the extent to which universities and private foundations had a hand in the initiation of new public policies. Private universities as well as the land-grant colleges were drawn into public service functions, partly because they were, in the absence of a career bureaucracy, the main reservoir of expertise on which politicians could draw for advice, and partly in response to the influence of the philanthropic foundations.

By the 1920's, some of the major foundations had lost interest in the charitable alleviation of social problems, and began to hope that science might solve them. This idea led to a strategy of supporting both scientific research and demonstration projects to test the application of such research, which could then be extended by the greater resources of government. Their aid to scientific education and research is a familiar story, in almost every branch of science. But equally important, they went on to help strengthen the professional organizations of scientists,[13] and to pay for the efforts of governments to improve their organization and administration, and to make use of research and research institutions as they did so. By the time of the Second World War, the leading scientists knew that a grant-making agency like a foundation could initiate nationwide programs by working with independent universities and governmental agencies, as the stories of hookworm control, the foundation of public libraries, and the reform of medical education all suggested. And political leaders were inclined to turn to private funds to help them explore future policy opportunities, or experiment with them, as when President Hoover sought foundation financing for his Committee on Social Trends and for a National Science Fund, and the Public Administration Clearing House provided the initial administrative costs for President Roosevelt's Science Advisory Board.[14]

As scientists learned that the organization of government was something that could be influenced from the outside, and that universities and foundations could have a substantial influence on public policy, they were in effect freeing themselves from the assumption that government and private institutions were sharply different in nature. They were accordingly ready, at the outset of the Second World War, to work out a thoroughly pragmatic set of arrangements for the conduct of weapons research. The approach that they adopted was simply to enlist institutions rather than individuals in the two great scientific programs of the war: the Office of Scientific Research and Development (OSRD), and the Manhattan Project of the Army Engineers.

To those who expect wartime crises and military authority to produce a centralization of authority, this approach must have been as surprising as if the Army had used the war as an excuse to increase, rather than decrease, its reliance on the state militias. But in the hands of Vannevar Bush, James B. Conant, and Karl T. Compton, the government contract became a new type of federalism. Under the OSRD, the Massachusetts Institute of Technology took on the responsibility for developing radar, and California Institute of Technology rockets, and under the Manhattan District, the University of Chicago set up the first sustained nuclear reaction and the University of California fabricated the first atomic bomb, while du Pont, General Electric, Union Carbide, and other industrial giants built the facilities to produce the fissionable materials.[15]

---

[13] The National Research Council, created by President Wilson to do in the First World War (in a rudimentary way) what the Office of Scientific Research and Development did in the Second, was supported not by appropriations but by the Rockefeller and Carnegie foundations (Axt, Richard G., *The federal government and financing higher education*, 78, New York, 1952).

[14] *Report of the Science Advisory Board*, 15, Washington, D. C., Sept. 20, 1934.

The postwar sequel is a well-known story. Through a continuation of this system of administering research and development programs by grant or contract, the Atomic Energy Commission, which was hailed by the draftsmen of the Atomic Energy Act as a triumph of socialism,[16] supports a program in which some nine-tenths of the employees work for private corporations. The adamant argument of many scientific leaders of the 1930's against federal support of science now seem as ancient and irrelevant as debates over infra- or supra-lapsarianism; no major university today could carry on its research program without federal money. The Massachusetts Institute of Technology, California Institute of Technology, Chicago, and Johns Hopkins, of course, all administer special military or atomic energy programs and consequently draw from three-fifths to five-sixths of their budgets from government, while Harvard, Yale, and Princeton now get a larger proportion of their operating revenues from federal funds than do land-grant colleges like Illinois, Kentucky, and Maryland.[17]

In dollar volume, the biggest contracts are between the military services and industrial corporations; while most of this money goes for procurement, much of it goes for research and development, and for the kind of systems analysis and the direction and supervision of subcontractors that in a simpler age would have been done by the technical services of the Army and Navy. And even in the business of procurement, the contractual relation is not the traditional market affair: the contract is not let on competitive bids, the product cannot be specified, the price is not fixed, the government supplies much of the plant and capital, and the government may determine or approve the letting of subcontracts, the salaries of key executives, and a host of other managerial matters. A sizable proportion of the government's (and nation's) business is done this way; any one of six industrial corporations spends more federal tax dollars than any of the four smallest executive departments.[18]

But the significance of this development does not turn on the sheer quantity of money, but on the possibilities of institutional development: if a contract can be made with an established academic or industrial corporation, why cannot a new one be set up for the purpose, and if the system will work for scientists and engineers, why not for others? Accordingly, we have been seeing not only the splitting off of certain functions that government might have operated directly and their administrative fusion with private institutions, but the creation of entirely new private corporate entities (e.g., the RAND Corporation, the Institute for Defense Analyses, the Aerospace Corporation) for the performance of government business.

As for the kinds of business that can be done under this system, Sir Henry Maine, who believed that progress was measured by the change from status to contract, would be intrigued to note that private corporations have contracts to maintain the Air Force's bombers and its missile ranges, private institutions make strategic studies for the Joint Chiefs of Staff and foreign policy studies for the Senate Foreign Relations Committee, universities administer technical-assistance programs for the State Department all over the world, and telephone and radio companies are about to help the National Aeronautics and Space Administration carry our messages through outer space.

This new system is doubtless breaking down the political opposition to federal programs even more effectively than did the system of grants to the states. State and local governments and private corporations used to join in their jealousy of purely federal activities, any extension of which was considered socialistic. The federal grants to states in the field of agriculture, however, were no longer socialistic in the eyes of the governors and the farm bloc: they were a defense of the American way of life, even though they involved more government controls than some avowedly socialistic states have ever managed. And now that the atomic energy and space and military programs support such a large share of the nation's business, and so much of its enterprise and innovation come from research and development financed by federal funds, and so much of that innovation and enterprise spills over quite naturally and properly into related commercial fields, it is no wonder that private business corporations are less jealous of government. More accurately, their jealousy no longer takes the form of fighting socialism, but of haggling over the administrative provisions of contracts. A great deal of private enterprise is now secreted in the interstices of government contracts. In short, what the grant-in-aid programs did to the arguments for states' rights, the new contractual systems are doing to those for pure private enterprise.

---

[15] See the first volume of the forthcoming official history of the Atomic Energy Commission, Hewlett, Richard G., and Oscar E. Anderson, Jr., *The new world*, to be published in 1962.

[16] "The field of atomic energy is made an island of socialism in the midst of a free enterprise economy" (Newman, James R., and Byron S. Miller, *The control of atomic energy*, 4, New York, 1948). Mr. Newman, writing a preface to this book a year after the text was completed, noted that "Only one major policy formulation, the decision by the Atomic Energy Commission not to conduct research in its own laboratories, departs sharply from the interpretations of the Act set forth in these pages" (p. xi).

[17] See the forthcoming study, to be published by the Carnegie Foundation for the Advancement of Teaching in 1962, on the relationship of American universities to the federal government.

[18] For a general discussion of this problem from the legal point of view, see Miller, Arthur S., Administration by contract: a new concern for the administrative lawyer, *New York University Law Review* **36**: 957-990, 1961. The economic aspects are discussed in a study by Carl Kaysen, *Improving the efficiency of military research and development*, to be published in 1962 by the Committee for Economic Development; and the general problems of weapons development and procurement programs in a study, to be published by the Harvard Business School in 1962: Peck, Merton J., and Frederic M. Scherer, *The weapons acquisition process: an economic analysis*.

But the argument for a measure of independence from central authority still remains valid in either case, and so does the need to recognize that the fundamental responsibility of government cannot be delegated. In a proper sense of the term, "sovereignty" is, of course, not affected by this type of delegation. Policy decisions remain the responsibility of government. But "policy" here means simply those aspects of the business that government authorities consider it important enough to warrant controlling, either because they think them of major importance, or because they realize that voters or Congressmen think so.

This means that they will consider as policy certain aspects of management (for example, fair employment practices or prevailing wage rates). But, as long as they retain ultimate control, they may act on the advice of contractors with respect to the most momentous new issues, or delegate major segments of the business whenever they can specify the purposes to be accomplished: the complex and costly nature of certain types of military studies, and the sophistication of the new techniques of operations research, make the possibility of such delegation very broad indeed. There is nothing in the nature of the contract itself (or the grant, which differs from it only symbolically and in technical detail) to determine whether in this relationship a central bureaucracy will control every detail of the contractor's management, or will leave him free to decide matters in secret that ought to be determined by the President and Congress.

But the general effect of this new system is clear: it has destroyed the notion that the future growth in the functions and expenditures of government, which seems to be made inevitable by the increase in the technological complexity of our civilization, would necessarily take the form of a vast bureaucracy, organized on Max Weber's hierarchical principles, and using the processes of science as Julian Huxley predicted to answer policy questions.[19] To the considerable extent that scientists have shaped this development, its political and administrative patterns have reflected the way scientists actually behave rather than the way science fiction or Marxist theory would have them behave: they have introduced into the stodgy and responsible channels of bureaucracy the amiable disorder of a university faculty meeting.

Compare, for example, our oldest and least scientific federal agency with a large operational mission with the newest and most scientific—the Post Office with the Air Force or the Space Administration. The Post Office is a relatively self-contained hierarchy. The Air Force develops its policies and runs its program with the advice and cooperation of several dozen of the most influential universities and industrial corporations of the country, whose executives and faculty members consequently have independent bases from which to criticize any policies, strategic plans, or administrative arrangements they dislike—and they can always find a Congressional committee to listen to them.

I do not think the role of science in this difference is entirely accidental. This is in part because the pursuit of science itself is a nonhierarchical affair; the best scientists either personally prefer, or are taught by their guilds that they should prefer, the university's combination of research, teaching, and irresponsible administration, and to get the best scientists the government took them on their own terms. But more important, I believe, is the long-range and indirect connection: when the Revolution of the Enlightenment proposed that the organization and procedures of government as well as its policies should be open to scientific inquiry and independent criticism, they started a process which has had deep effects on the constitutional system. These effects showed first in the relation of scientific administrators to their executive superiors and to Congressional committees, and later in the new structure of federalism, and in the new contractual relationships between the federal government and private institutions.

---

[19] Huxley, Julian, *Man in the modern world*, 120–121, New York, 1948. See also his *Religion without revelation*, 4, New York, 1957.

As the story of the President's Science Advisory Committee illustrates, to say nothing of the similar advisory groups to the military services and the Atomic Energy Commission, this type of relationship very greatly reduces the possibility that great issues will be decided by closed scientific politics, or that the increase in importance of scientific staff work will reduce the free play of policy debate. For the institutional bases from which advisers operate give them a measure of independence as public critics, and thus provide something of a counterbalance to the centralizing pressures of wars and rumors of wars.

American scientists, who have tended to be a little disillusioned about their relationship with politicians ever since the Jacksonian period, are now entitled to look with somewhat greater satisfaction on the domestic Establishment that they have helped set up. For to some small extent science has helped the political system of the United States develop along lines quite different from the classic patterns of either parliamentary government and laissez-faire economics on the one hand, or socialism and one-party rule on the other. Among its essential institutional features are universities that are concerned with applied as well as basic sciences, and continuously exchange personnel with the government at all age levels; a personnel system which puts up no barrier against the administrative promotion of men with scientific training; and grants-in-aid and contracts through which federal agencies may influence or guide the policies, but not direct the detailed management, of certain aspects of local governments, business corporations, and universities. Among these institutions, the connecting links are strongest in scientific and technical fields. And the peculiar looseness of the constitutional system enables the scientists in each field to take the initiative in developing policies—just as their innovations are providing the greatest impetus to industrial enterprise. Most important, science is not restrained in its impact on policy by any rigid distinction between ends and means, imposed by institutionalized systems of traditional or ideological values. The key to this is the freedom to influence or determine the organization and procedures of government from the outside, not conceding control over them to professional administrators or party leaders.

But there are some good reasons why scientists should not be too self-satisfied about their new status. A good many of them already think that science has been corrupted by this new system, and the wealth that it has brought.[20] They tend to look back on pre-war science as the Reformers looked back to the Primitive Church: a period of austere purity, an era in which no vows were needed to guarantee the poverty of the professor, no scientist was seduced by a government contract, and teaching fellows were obedient. One may well be a little skeptical about this point of view, and suspect that poverty probably brought its distractions no less troublesome than those of riches. But even if we discount such dangers so far, the worst may be yet to come. The public and members of Appropriations Committees are being led to think of science in terms of spectacular results like a space satellite or a cancer cure, and the political pressure to pass miracles may lead to some major distortions in our national policy, and put some uncomfortable pressures on the independence of scientific institutions. We probably have less reason to fear that major governmental decisions involving science will be secret than that they will be popular.

For while our new system of administration by contract temporarily avoids the political problems that come with the growth of bureaucracy, it encounters them again in more subtle and difficult forms. We do well to recognize that a government bureau is tempted to be more concerned with its own status and power than with the purposes of national policy. But if we entrust those purposes to industrialists or even scientists, we do not sterilize that political temptation. We only let it begin to work directly on the industrialists and scientists. If public ownership is no guarantee of unselfishness, neither is private ownership. And it is ironic, in view of the general public image of his political ideas, that it was President Eisenhower who presented most forcefully to the country the danger that, having hired private corporations to further specific public ends, we will see them use the public means for private profit, or even in political efforts to control the policy decisions of the government.

---

[20] Tuve, Merle A., Basic research in private research institutes, in Dael Wolfle, Ed., *Symposium on basic research*, 178, Washington, 1959.

Government policy, like science itself, needs to be conceived and pursued with some regard for its totality as well as its parts. By giving priority to the parts—by turning over the administration of public functions to private institutions—we have strengthened our ability to do a great many separate things, but not our ability to give integrity and discipline and direction to our total effort. Indeed, by relying too much on the contracting method we have probably weakened the quality of the scientists within the civil service, whose help is needed by the executive who seeks to manage our scientific programs as a coherent system.[21]

In the dimensions of its financial support and in the breadth of its influence, science has indeed become a national Establishment. Politicians are more likely to abuse it by calling on it to advance their special causes than they are to ignore it. In this predicament, scientists cannot protect their essential interests in government by setting themselves apart in a separate status or separate department. They used to be content with the control of particular bureaus or programs. Today, in the White House Office or the lobbies of the Capitol, they are obliged by the nature of the system they helped create to play a responsible role in all aspects of national policy, and in the development of a new pattern of relationships between public and private institutions in our society.

---

[21] Brown, Harold, *Research and engineering in the defense laboratories* (An address by the Director of Defense Research and Engineering), Washington, October 19, 1961.

216

Scientists and National Policy-Making

EDITED BY

ROBERT GILPIN AND
CHRISTOPHER WRIGHT

Columbia University Press
NEW YORK AND LONDON 1964

Scientists and Politics: The Rise of an Apolitical Elite

ROBERT C. WOOD

Professor of Political Science at the Massachusetts Institute of Technology

If the war years of the 1940s gave formal recognition to science as a national resource, to be used systematically and extensively for security purposes, the late 1950s marked the emergence of American scientists as genuine political influentials. Gone are the days, surely, when many eminent scientists lived the life J. Robert Oppenheimer describes as his in the early 1930s in Berkeley.

> I have never read a newspaper or a current magazine like Time or Harpers; I had no radio, no telephone; I learned of the stock-market crack [sic] in the fall of 1929 only long after the event; the first time I ever voted was in the presidential election of 1936. . . . I was interested in man and his experience; I was deeply interested in my science; but I had no understanding of the relations of man and his society.[1]

Today, Hans Bethe's account of his activities describes what seems to be the more typical relation of many distinguished scientists to public affairs.

> I would say I spent about half of my time on things that do not have any direct connection with my science. This is somewhat worse for older people like me, but even the young people feel the external pressures to do things other than their scientific work. . . . I used to be a member of the President's Science Advisory Committee. That committee has been assigned and has also made for itself great responsibility, particularly in the field of military technology. I sit on a large number of its subcommittees and they take a great deal of my time. Many of my

---

[1] Atomic Energy Commission, transcript of hearing before the Personnel Security Board, *In the Matter of J. Robert Oppenheimer* (Washington, D.C., USGPO, 1954), p. 8.

colleagues here in this department and in other departments sit on committees that must advise on funds from, say, the National Science Foundation. And there are many committees on scholarships. . . .[2]

Some of the forces which help account for this transformation in the workaday world of science are now familiar: the exponential rise in public research and development funds, the establishment of new governmental agencies and enterprises with predominantly scientific and technological missions, and the rapid evolution of mechanisms for the provision of scientific advice on policy. A less emphasized but perhaps more important factor has been the scientists' own participation in the political arena while these new arrangements were being fashioned.

Since the days of the Office of Scientific Research and Development (OSRD), scientists have articulated and fought for their political goals. Sometimes they have lost, sometimes they have compromised, but often they have achieved political goals which they conceived to be in the interest of their profession. Their struggle in Congress for civilian dominance of the Atomic Energy Commission (AEC) can be interpreted in this light, as can the matter of Dr. Oppenheimer, the long drawn out wrangle over the organization and management of the National Science Foundation (NSF), and, more recently, the triumphant revitalization of the President's Science Advisory Committee (PSAC) and establishment of the National Aeronautics and Space Administration (NASA). Scientists have demonstrated in these instances as well as in the continuing negotiations between government and universities over terms and conditions of sponsored research and in their heavy involvement in disarmament policy that they possess both political influence and, on occasion, the capacity to use it effectively.

The rise of scientists in political visibility has been so rapid that observers disagree on the nature and scope of their influence and the bases of their power. One prevalent interpretation, often advanced by the scientists themselves, amounts to an "orphan theory" of the scientists' political status. Thus, Sir Charles Snow and Gerald

---

[2] Center for the Study of Democratic Institutions, *Science: An Interview by Donald McDonald with Hans Bethe* (Santa Barbara, Calif., The Center, 1962), pp. 3–4. Hereinafter referred to as *Bethe Interview*.

Holton would have us believe that although the potential of their profession for constructive contributions to public policy-making is enormous, their counsel is misunderstood, misused, feared, and neglected. In this view, scientists bear the burden of men possessed with the capacity to solve the burning issues of the day, who are nevertheless excluded from counseling the great by reason of popular ignorance and cynical political manipulation.

A sharply contrasting view that is usually advanced by nonscientists asserts that in many major policy areas the scientific community bids fair to dominate the political scene. President Eisenhower is not alone in his concern for the "scientific-technological elite." It is common practice for nonscientist participants in high policy circles to refer, rather ruefully, to the cult of "doctor worship," to the conspiratorial atmosphere of the Cosmos Club, and to the uninhibited disposition of some scientists to take policy positions unrelated to their fields of expert knowledge. Indeed, a substantial body of scholarly and popular commentary now takes the natural scientist to task for his failure to "join the team" in the federal establishment and impose some measure of self-discipline in his advice-giving capacity.[3]

The divergence of these views is intriguing because of the great differences in the estimated nature and amount of influence of natural scientists, and in the normative evaluation of the proper role of the scientists as a group. Yet our present knowledge of civil-scientific relations is so limited that we should not take either of these views as an approximation of reality. We should reserve judgment until we have established a frame of reference for observing the political behavior of natural scientists and engineers and have collected some hard data with which to characterize our model.[4]

---

[3] See Don K. Price, *Government and Science* (New York, New York University Press, 1954) and James L. McCamy, *Science and Public Administration* (University, Ala., University of Alabama Press, 1960). This point was also elaborated during the Columbia University Civil-Science Relations Seminar held in the Winter of 1961–62. For an unbridled condemnation, see Frank Gibney, "The Missile Mess," *Harper's* (January, 1960).

[4] The empirical basis for the study of civil-science relations is still small and much of the data remain under security classification. Aside from the general literature in the field, this analysis rests on approximately 100 tentative probes conducted over the past five years at M.I.T. This material includes professional

As a first venture along these lines, we shall attempt to identify the distinguishing characteristics of the scientific and engineering profession and explore the process by which these characteristics might be transformed into political resources. The limited empirical evidence available indicates that if the scientist in politics is a little less than a leader of a snowballing political crusade, he is a good deal more than another expert performing tricks of virtuosity at the command of politicians, bureaucrats, or soldiers. He is instead a member of a still small and untutored elite which has entered an inhospitable political system but which possesses such valuable assets that it bids fair to displace entrenched skill groups in certain parts of the system and to establish an over-all new equilibrium in the competition for power among the professions.

It is especially important that the influence of this group apparently does not come about by conscious adaptation to the political world, that is, by learning the skills of other elite groups, but by continuing its own sharply differentiated behavior pattern. The group is an apolitical elite, triumphing in the political arena to the extent to which it disavows political objectives and refuses to behave according to conventional political practice.

IN QUEST OF A MODEL

In a field still imprecisely defined and with few substantial empirical investigations to its credit, there are, admittedly, many appropriate observation posts. Some students prefer to treat the majestic sweep of science as a force transforming almost every aspect of polit-

---

case studies and student field investigations of political conflicts involving scientists and engineers. On the local level, these studies are concerned principally with educational and public health matters, and in particular with fluoridation. On the state level, they deal with highway and natural resources controversies. On the federal level, they range from atomic energy and space matters to regulations respecting the marketing of cranberries.

The studies varied enormously in reliability and quality, of course, and cannot be interpreted as systematic and exhaustive explorations closely correlated to the total population of these conflicts. But they are something more than episodic histories since they deal with scientists in their several roles. These studies thus serve the classic function of case analysis by overturning simplistic notions of how scientists behave in politics, and by indicating where further inquiry is required.

ical behavior and requiring radical readjustments in our public institutions and processes if our knowledge of the universe is to be used for the benefit of mankind. Other students see the scientist's role as comparable to that of the military or administrative technician, speculating that characteristics of expertise, social responsibility, and corporateness apply to him in much the same way as Samuel P. Huntington has viewed the professional role of the soldier. Still other students look upon the scientific community as a pressure group, more loosely organized and with less formal obligations to the state than the military but capable of cohesive, effective political action on matters which directly touch their occupational and public interests. There are also those who prefer to deal with the "scientific enterprise" in structural terms by focusing on that complex of public agencies, advisory bodies, and contractual relations among government, universities, and industry in which scientists work for public purposes, and evaluating these arrangements in terms of more or less conventional organizational and operational criteria.[5]

Yet, if our principal concern is to comprehend the influence of scientists active in politics, we need a frame of reference somewhat less lofty than that which regards science as an impersonal historical force and begs the question of who directs and mobilizes that force. We require a model more comprehensive than that which deals piecemeal with a scientist in his various capacities as an expert, adviser, and private citizen in varied situations and structures and assigns him roles by the technique of analogy. We need a model which permits us to account for the scientist's notable achievements as expert and adviser, his spotty record as lobbyist, and his apparently inconsequential efforts to provoke the public to action. The middle-range theory which suggests itself as especially appropriate to the scientists in America is the "skill commonwealth" of Harold Lasswell, since this analysis organizes groups of influentials not on the bases of geography and class, but according to the exercise of a common set of skills which can be taught and learned.[6]

---

[5] James B. Conant, C. P. Snow, Gerald Holton, Vannevar Bush, Pendleton Herring, Lewis Mumford, and Walter A. Rosenblith are examples of individuals with the scientist-philosopher or philosopher-scientist view. Wallace S. Sayre has dealt with the possibilities of the pressure-group analogy in an essay in this volume. J. Stefan Dupré and Sanford A. Lakoff stress the enterprise model in their *Science and the Nation: Policy and Politics* (Englewood Cliffs, N.J., Prentice-Hall, 1962).

"Skill politics," as first used by Lasswell, was a term designed to distinguish among specialists in bargaining, organizational, propagandistic, and violence activities of classic political relevance. It provided a perspective for studying the rise of, and competition among, elites and decision-makers separate from those based on differences in social status, personality, race, ethnic origin, or occupation, when treated as productive of common political outlooks and ideology.[7] It coupled the possession of particular skills with major political issues and events; with the "redirection of culture" as Lasswell phrased it. It linked the emergence of particular skill groups in politics with long-run developmental tendencies in economic and social organization.

We shall not adopt the skill commonwealth in the fullest sense in which Lasswell has employed the concept, but shall modify its components in several important respects. For example, no claim is made that a scientific skill group enjoys an elite status which dominates all parts of the political process or makes it a prime contender for filling major decision-maker positions in the society. This analysis first seeks to establish the relevance of scientific and engineering skills to what seems to be a recent but vital new function of the modern state—the assurance of a high quality and an accelerating rate of technological innovation. Given that basic point of departure, the model takes on the familiar characteristics of contemporary political analysis. We inquire as to (1) what political significance the skills have, that is, how they may be used in the bargaining process, (2) how the political utility of these skills is augmented by other groups' perceptions of them, (3) what group disposition exists to spend the political resources possessed, and (4) where within the existing political structure these resources are maximized.

We proceed to study skill politics within the framework of a decision-making process which has been transformed rapidly since

[6] Harold D. Lasswell, *The Analysis of Political Behavior: An Empirical Approach* (New York, Oxford University Press, 1948), p. 134.

[7] For an excellent summary of elite analysis, including that of skill groups, see Donald R. Matthews, *The Social Background of Political Decision-Makers* (Garden City, N.Y., Doubleday, 1954).

World War II as major outputs contain more technical and less normative components, as the executive assumes predominance over the legislative, as the issues in a campaign bear less and less relevance to the issues which preoccupy those in authority between elections, and as the utility of different skill groups changes accordingly. It is this simultaneous consideration of the scientists' skills, their political uses, and the evolving political system which leads us to specify how and why a skill group traditionally aloof from political activity is now emerging to influence policy on crucial issues at key places of leverage.

PROPERTIES OF THE SCIENTIFIC SKILL GROUP

Identification of the scientific skill group is the starting point for tracing its political emergence. If we take the broadest possible interpretation of scientific skills, those required (in Bethe's words) for an understanding of nature, and add to them the companion skills of technology, those involved in the mastery of nature, we can identify a group of considerable magnitude whether reference is being made to the approximately 100,000 individuals listed in the *American Men of Science* or the more than 2,000,000 membership in the American Association for the Advancement of Science (AAAS).[8] Yet these are not the relevant characteristics for the politically active scientific skill group. At best they indicate the manpower pool from which the elite can be recruited with its members still having some claim of knowing about or speaking for science. Although attempts have been made to attribute to this expansive group and its members properties of cohesiveness, uniformity of personality, and common norms, incentives, and audiences, these attempts still appear to rest on shaky empirical foundations.[9] At least as far as political attitudes are concerned, the so-called scientific community is known to be divided by fields of specialization, geographical location, blocks in communication between government and university bases, positions on matters of public moment, and institutional environments. It lacks any overarching structure of professional associations or established procedures for determining membership attitudes and interests. Its engineering component in particular is cross-pressured by an industrial orientation and ideology and by the frequency with which the final career patterns of engineers bear little relation to their formally acquired occupational skills. The common ideology which members of this community are often presumed to share resembles the occupational analogue for God and motherhood—a belief in progress through experimentation and the accumulation and systematic application of knowledge.

---

[8] A systematic effort to assemble and compare various estimates of the size of the scientific community was made by Vernon Fladager, "The Size and Nature of the U.S. Scientific Community," in *Science and Public Policy* (Cambridge, Mass., M.I.T. Industrial Liaison Program, January 22–23, 1961).

[9] One of the most provocative theoretical analyses of science as a social system resting on identifiable membership, shared values, and incentives has been advanced by Norman W. Storer in his "Some Sociological Aspects of Federal Science Policy," *The American Behavioral Scientist*, VI, No. 4 (December, 1962), 27–30.

The politically relevant scientists constitute only a small sliver of the larger group. For federal issues, Christopher Wright has estimated that there are between 200 and 1,000 men and women who significantly and directly influence the availability of scientific knowledge and ideas and their technical application. James Killian is inclined to fix the number of the consistently influential at 200—the lowest point of Wright's estimate. In an initial empirical exploration, Howard Rosenthal screened names from the *American Men of Science* according to five measures of elite participation in advisory posts, professional associations, and mass media recognition to arrive at a core group of approximately 900 members. But Rosenthal's calculation of an "active elite"—participation in at least two elite capacities—resulted in a group numbering only 392.[10]

Reduced to these proportions, the scientific skill group tends to acquire characteristics of considerable cohesion. In reviewing their backgrounds, Rosenthal found that in comparison to his total population the active elite displayed a high propensity to be native-born, living in the New England and Middle Atlantic regions of the country, between forty and fifty years of age, university-employed, and engaged in professional fields other than engineering. Killian identifies as further characteristics a widely shared goal among members of the group to protect the "integrity" of science or assure respect for scientific knowledge and the means and institutions through which we acquire it and a highly efficient, though informal, communication network within the group. Wright concurs in these characterizations, emphasizing the further property of self-selection. He suggests that particular environments—notably eastern universities where contacts between scientists and nonscientists are likely to occur—and special intellectual traits, notably "virtuoso manipulation of facts and arguments," impel the elite members into political activities.

---

[10] For the Wright estimate see his essay in this volume. Killian's estimate was given in the course of his graduate seminar on Science and Public Policy at M.I.T., and Rosenthal's is contained in his "The American Scientific Elite" (mimeographed, May, 1961).

The group is not politically united in the partisan sense and is wracked with controversies in their judgment on technical issues. Moreover, below the national level the scientific expert often works without contact with his peers. But, granted these differences, a convergence of forces—basic skills, basic goals, shared backgrounds and environments, ease of communication, personal dispositions as to how issues are raised and resolved—operates to make this small, mutually respecting and respectable group a meaningful entity.

SCIENTISTS IN AN ALIEN CLIME

To identify the politically relevant scientific skill group—the vocal and effective scientists, as Robert Gilpin has termed them—is not to demonstrate that they have become influential or to explain how this status was achieved. Indeed, the protest against the "doctor cult" seems at first to reflect the hypersensitivity of other participants in national policy-making to the entry of a handful of scientists into positions of limited scope and responsibility. In terms of the history of the American commonwealth of skills, the scientist-activists are only a tiny minority among the occupants of key decision-making posts in the federal government. Clearly, they fail to fit the broad skill classifications used by Lasswell to distinguish among decision-makers: bargaining skills acquired through business experience, propagandist skills of manipulating symbols having mass appeals, political skills involved in the negotiation of favors, and administrative or military skills applicable in the direction of large organizations or the use of violence.

If we shift our attention from political to occupational skills, the scientists still seem overwhelmed by sharply contrasting skill groups. Aside from the two occupations confronting the scientist in his advisory capacity—the soldier and the administrator—it is the lawyer who has claimed near-monopoly in supplying the participants in public life. Traditionally, the legal profession in the United States has provided both the incumbents of high posts and their expert advisers including 70 percent of American presidents, vice-presidents, cabinet members, and a large portion of state governors and representatives. Of course, lawyers comprise the overwhelming majority in state and federal judicial systems. On the whole, lawyers seem to have fair claim to the title "high priests of American politics." [11]

With respect to the specific role of adviser, the role in which scientists have generally been assigned most influence:

> [T]he lawyer is today . . . the one indispensable adviser of every responsible policy-maker of our society—whether we speak of the head of a government department or agency, of the executive of a corporation or labor union, of the secretary of a trade or other private association, or even of the humble independent enterpriser or professional man. As such an adviser the lawyer, when informing his policy-maker, is in an unassailably strategic position to influence, if not create policy . . . for better or worse our decision-makers and our lawyers are bound together in a relation of dependence or identity.[12]

The typical reasons offered for the preeminence of the legal profession as the source of supply for decision-makers and their advisers are straightforward. The lawyer's training in interpersonal mediation and conciliation and his facility with words in debate and oratory seems obviously useful in the American system. The lawyer also finds it relatively easy to enter politics and subsequently return from politics to a private law firm, with the assurance that the practice of law has not been revolutionized in the interval. Most important of all, perhaps, is the fact that government service and professional advancement are inseparably intertwined since so much of the successful practice of law revolves around familiarity, contact, and influence with the political process.[13]

---

[11] Matthews, *The Social Background of Political Decision-Makers*, pp. 30–31.
[12] Lasswell, *The Analysis of Political Behavior*, p. 27.

The scientific skill group is not only overwhelmingly outnumbered by the legal skill group in its various roles, but appears at first to be exceptionally ill-equipped to do battle with the lawyer in any policy conflict. To use Lasswell's terms, men skilled in the manipulation of symbols of things confront men skilled in the manipulation of symbols of interpersonal relations. Men who are unaccustomed to reflective consideration of values and their allocation argue with men whose stock in trade is the clarification of value choices and their rationalization to secure acceptance. Men steeped in the tradition of empirical investigation undertake to compete with men versed in the dialectic, whose scholarship is heavy with syntax and virtually barren of the scientific perspective of even the social sciences. Men oriented toward the concept of change face men who by tradition are history-oriented. They do so in a process that apparently appeals to values, and is responsive to historical analogy and the regularization of political and social behavior by prescription and enforcement rather than by an empirical understanding of trends and probabilities. While almost all decision-makers can be expected to be familiar with, and sensitive to, the lawyer's reasoning and behavior, few communicate easily with the scientific skill group.

This characterization of the political environment, more often instinctively felt than explicitly identified by spokesmen of scientists, is frequently used to belittle the influence of the new skill group. Yet those who detect and protest against the rise of the influential scientist also base their indictment on this characterization. The one despairs at the "noise," the "nonsense," the endless disputation in which the legal, the historic, the value-laden approach enfolds questions of policy. The other fears that these criteria are poorly understood or cavalierly ignored when scientists undertake to decide public issues of moment.

---

[13] See for example, Esther L. Brown, *Lawyers, Law Schools and the Public Service* (New York, Russell Sage Foundation, 1948); John T. Doby, "The Lawyer as a Political Leader," *Wisconsin Law Review*, MCM, No. 2 (March, 1950), 308–13; William Miller, "American Lawyers in Business and Politics," *Yale Law Journal*, LX, No. 1 (January, 1951), 66–76; David R. Derge, "The Lawyer as Decision-Maker in American State Legislatures," *Journal of Politics*, XXI, No. 3 (August, 1959), 408–33.

On an empirical basis, the judgments of either are irrelevant. What counts are the rapid transformations taking place in the established political system and the drastic alterations in the channels for advice giving and receiving. Political scientists who have contributed to the description of the new circumstances of policy-making have yet to make a corresponding descriptive evaluation of the new skill group. Still protesting the insensitivity of the new breed to their established rules of conduct, these political scientists overlook the fitness of new functions and new duties of the state to the scientists' present talents, whether leavened by an awareness of ancient wisdom or not. Although scientists do not yet run for public office, exercise demagogic appeal over the masses, communicate easily with legislators, mobilize their ordinary members into smooth-functioning and hard-bitten lobbies, they are, nevertheless, occupying critical positions of strength in the evolving new process with increasing frequency while other occupations are not occupying comparable positions.

SOURCES OF INFLUENCE

The technological responsibilities of the modern state. The present eminence of the scientific skill group rests upon the frequently emphasized but not always accurately understood fact that since World War II major powers in the nation-state system live by their wits. Brainpower more than firepower is the crucial element from which national strength evolves. Every modern political system must have the capacity to translate basic scientific knowledge into workable, effectively engineered design and structure. Hence, its decision-makers must have the assurance that the society it directs maintains technological innovation at least on a par with major competitors.

The need for scientific quality extends beyond consideration of national security. In determining the availability of natural and human resources, the modern state needs assurance of first-rate technology across a wide spectrum of organized activity, civil and domestic as well as military and international. Thus, relative technological conditions in agriculture, in other resource development, medicine, and industry become as much a matter of moment to imaginative decision-makers as the scientific sophistication present in weapons systems. And all imply a concern for the quality of the national education and research enterprise—an assurance that curiosity and the expectation of change are deeply embedded characteristics of a national style.

In the period of a generation there has been a massive shift in the functional responsibilities of the private and public sectors of our nation. Twenty years ago, when the United States first assumed the role of arsenal of democracy, few would have assigned to Washington the task of underwriting the rate of technological innovation or establishing priorities among scientific fields. "Progress" in the industrial-business-oriented state took care of itself, was reinvigorated, strengthened, and accelerated by the invisible workings of the market place and the genius of individual effort. Except for the stimulation provided by the patent laws and the protection granted by trade regulations, the understanding and mastery of nature occurred without conscious and comprehensive public design. In the Western world, what a government "could do" was a matter for judicial deliberation, not the laboratory.[14]

In this context, a political decision-maker's overriding responsibility as far as the innovative force was concerned was to determine the impact of the force on the prevailing balance of political and social interests. There were, to be sure, serious problems of equilibrium: how to distribute the fruits of progress, what ideology best rationalized the allocation of increased material well-being, what weight to give to conflicting claims of property and labor, and what industrial structures to encourage or inhibit. One need only reflect on the tight grip which the English Poor Laws maintained on welfare philosophy for 400 years to recognize the elaborate rationalizations required of nations in the midst of industrial revolutions to accommodate an exponential increase in innovation. But it was the distributive, rather than the innovative, problems of industrialization and the technological bases that triggered radical philosophies and stout defenses of the existing order and consumed the attention of the political system. Thus, bargaining skills, propaganda skills, and violence skills all assumed great importance. The political order obviously required leaders and advisers with the lawyer's special skill in value clarification, his verbal capacity, and his experience as an intellectual jobber and contractor who could make a strong case wherever one was required.

---

[14] This is not to ignore the frequently cited instances of governmental sponsorship and use of science in the eighteenth and nineteenth centuries from the census to steamboat-boiler inspections. It is simply to emphasize that these activities were sporadic, unsystematic, and relatively inconsequential in the process of technological development.

Today, no national political system can afford to concentrate on distributive and allocative problems. Indeed, the decline of ideological conflict, the gross simplification of value alternatives engendered by the Cold War, the growing constraints upon applying the techniques of violence, and the continued pace of innovation with its accompanying increases in material well-being suggest a steady decline in the urgency of value-oriented decisions. A government can support farm programs made obsolete by increased agricultural productivity, countenance shorter work weeks, and hold out the promise of more business profits on the expectation that new technological break-throughs can provide more products for everyone. It can project its international image in the relatively simple, though not necessarily effective, terms of an open society. It must limit the use of force to extend value acceptance. What a political system cannot ignore, in either a domestic or international sense, is the quality of the scientific enterprise—the public policy adopted for science and the caliber of scientific advice in public policy-making.

This transformation of the functional responsibility of the modern state drastically alters the position of the scientist from that of a benevolent outsider occasionally affecting the affairs of state to one of a valuable insider possessing information vital to the continued survival of the system. It subtly shifts the emphasis of the persistent political question "Can we do this?" from the consideration of legal constraints to consideration of physical constraints. In these circumstances, it is not surprising that the ranks of senior career personnel of the federal government, executives, advisers, and specialists have been increasingly filled by the scientific skill group.

Analyzing the 1,354 supergrade positions of GS-16 to GS-18 in 1957, David and Pollock observed: "A considerable number are lawyers, an even greater number are engineers and natural scientists. . . . Most of these individuals act frequently as advisers to executives and most of them probably have limited executive responsibilities." [15]

The increased number of decisions about science or having scientific components and the corresponding rise of the skill group to positions of influence within government agencies is naturally accompanied by opportunities for influence. But of more significance is the fact that in many instances these opportunities equip the skill group with special political advantages not possessed by other experts in the bargaining process.

One advantage arises from the fact that the possession of technical know-how about physical matters gives the scientist direct initiative in proposing action which is not available to other experts. Military, legal, and administrative advisers can speak to the decision-maker in strategic and procedural terms. Once provided with a statement of objectives—a given set of resources available from a given state of nature—they can suggest alternative courses of action, different mixtures of components, and new methods of application. Still they cannot seriously propose to change the state of nature itself.

Actually manipulating nature is the scientist's stock in trade. From Einstein's letter to Roosevelt, to Teller's proposal for Super, and the Lincoln Study Group's recommendations for the Distant Early Warning Line (DEW), the scientific skill group is in the position of generating new policy issues. Its judgment as to value implications may be neither sophisticated nor seriously considered, but in fields as far apart as disarmament and water desalination, no program can seriously be considered until the technological potential is discovered and articulated.

It is true that the decision-maker does not always have to wait for the scientist to come forward with his innovations. As Walter Rosenblith has pointed out, the presumption exists that in an age of forced-draft technology "a massive national (even industrial) effort will produce almost any technological goal once the necessary basic facts are known."[16] Given the reliance of scientists on government support for the provision of the complex facilities and organization now required in research and development, the alert administrator, general, or politician can indicate the kinds of areas in which the national interest requires concerted scientific activity. He can organize projects, authorize special studies, and specify public policy needs for more intensified research. Thus, his decisions can powerfully affect the rate at which innovation goes forward in particular disciplines and fields of inquiry.

---

[15] Paul T. David and Ross Pollock, *Executives for Government* (Washington, D.C., The Brookings Institution, 1957), p. 92.

Nonetheless, the fundamental inability of a decision-maker to command specific results suggests a second source of influence dependent on the possession of scientific knowledge. The scientist's allegiance, interest, and loyalty to particular endeavors has to be secured. He must be persuaded that he ought to become associated with a venture. He is in a position to bargain over terms and conditions. He can choose to participate or not. In short, the scientist has recourse to behavior not usually available to other professionals in government. He may participate in a mutiny or strike.

It may be argued that these are not real alternatives, that ultimately the scientific skill group can be brought along. Appeals to basic loyalties have been persuasive in times of great emergency. Yet, in particular situations, scientific attitudes about prevailing governmental policies and programs can result in behavior very close to disassociation. At the height of the AD-X2 battery additive controversy between the Secretary of Commerce and the Director of the National Bureau of Standards, 400 scientists, or over 10 percent of the Bureau's technical complement, told newspaper reporters that they planned to resign in protest. The companion actions of the Federation of American Scientists, both public and private, made plain the disposition of the elite to treat the Secretary's proposed dismissal of the Director as an issue involving the integrity of the scientific effort.[17] On a broader front, the Oppenheimer hearings, coming after years of security investigations which had increasingly irritated and enraged scientists, diminished the disposition of numbers of the most distinguished members of the elite to participate in governmental programs.

---

[16] Walter A. Rosenblith, "The Federal Government and Science" (unpublished memorandum, August, 1960).

[17] Samuel A. Lawrence, "The Battery Additive Controversy," in *Inter-*

These incidents have generally led decision-makers to treat respectfully the claims of the scientific skill group for special working arrangements, new institutions, and new concepts of administration. As their experience in governmental affairs has grown, the group has exchanged its initial organizational preference embodied in the independent, self-selecting, self-perpetuating board of directors which managed the National Advisory Committee for Aeronautics (NACA) for access to the Presidency instead. But the institutionalization of the first *ad hoc* think groups into permanent corporations, the perpetuation of the great governmental laboratories, and the continued preference for a university environment in which to conduct sponsored research attest to the substantial modifications which scientists have made in conventional concepts of administrative theory. In the name of protecting scientific integrity, they have secured for themselves conditions of administrative discretion which contradict ancient principles of hierarchy, chain of command, and span of control. With powerful ideological assistance from the American free enterprise tradition, scientists and engineers in universities and defense industries have contributed mightily to the destruction of the governmental agency and the business corporation as meaningful entities in the development and execution of public programs. The present array of research and development half-way houses born of systems analysis are monuments to their ingenuity and to their success in escaping the established modes of organization. The growing concern with conflicts-of-interests of members of the skill group is evidence of the preference for "loose" administration and indicative of the problems raised thereby.

Media support—scientist as disinterested party. The core of the scientists' strength rests in the decision-maker's need to understand and manipulate the physical world as well as words and symbols.

University Case Program, No. 68 (University, Ala., University of Alabama Press, 1962).

This resource is powerfully enhanced by the perceptions of the scientists' value accorded by other political influentials and the alliances developed on this basis. One valuable alliance is based on the favorable and deferential treatment accorded the skill group by the communication enterprises. No comprehensive content analysis presently exists to demonstrate how frequently and how favorably the scientist is treated by the press, radio, and television. In the M.I.T. explorations, however, the scientific expert was clearly assured sympathetic interpretation.

The most striking examples of respect for scientists from the communications media attach to issues of fluoridation, matters of school curriculum, and transportation policy at the local and state levels. Each example involved severe controversy and in many instances the professional qualifications of the experts involved would be suspect to the national peer group. Nonetheless, the media treatment was almost uniformly on the side of the "expert" who was depicted as "objective, disinterested, uncorruptible," and an "impartial searcher for the truth." Dentists, highway engineers, and doctors are laudable figures in the eyes of many local newspapers in these cases because their participation tends "to take the problem out of politics," a goal apparently devotedly, though illogically, coveted by many of the fourth estate.

Similar support has been evident in national cases where the skill group has become embroiled in open disputes. In the Oppenheimer case, the most influential of columnists came to the defense of the scientist. In the AD-X2 controversy, Drew Pearson led the attack against the Secretary of Commerce, and was joined by national radio and television commentators and most of the metropolitan dailies in defending the right of scientists to "find the truth." Again, in the DEW line case, the press was instrumental in supporting the scientist-advocates of continental air defense. Steven Rivkin has written that the Alsops "kept up a drumfire of articles in favor of air defense proposals. . . . As the controversy developed, the newspaper columnists and reporters themselves became an important element in the policy-making process. The scientist-participants could think of the press as a useful aid to their arguments." Such comment as the Alsops' characterization of the continental defense problem as "whether to gamble . . . vast sums and perhaps the health of the economy on the scientists' being right, or to gamble the survival of America on the scientists' being wrong," sustained the scientists' cause.[18]

It is true that the press is not unanimously and consistently on the side of the scientists. Powerful counterarguments were launched both in newspapers and magazines against the DEW line decision and against Oppenheimer, and even in the AD-X2 controversy editorial opinion was divided. Nonetheless, on major incidences to date, the bulk of support seems to have been with the "expert."

This support is not surprising. The scientist's conception of the vital role of public opinion in a democracy corresponds to that of the media skill group, and may even be held more sincerely. Both emphasize the dangers of secret decisions by a handful of men, the essential need for public awareness of the vital issues of the day, and public participation in their resolution. Hence, Oppenheimer's conviction:

> We do not operate well when the important facts, the essential conditions, which limit and determine our choices are unknown. We do not operate well when they are known in secrecy and in fear, only to a few men. I believe that until we have looked the citizen in the eye, we shall be in the worst of all possible dangers, which is that we may back into him. More generally, I do not think that a country like ours can in any real sense survive if we are afraid of our own people.[19]

For instance, in the 1958 Conference on Science and Public Policy held at M.I.T., the scientist-participants time and again voiced their conviction that the public was a critical factor in decision-making: it "had to be assisted so that it could make wise choices," it "must be brought along," and the educational process should be devoted to the objective of allowing the layman to "understand and appreciate science" so that in a modern world the public could be re-equipped with Jeffersonian characteristics.[20]

---

[18] Steven R. Rivkin, "The Quest for Air Defense," in *M.I.T. Falk Cases in Science and Government* (mimeographed, 1959).

[19] J. Robert Oppenheimer, "Atomic Weapons and American Policy," *Foreign Affairs*, XXXI, No. 4 (July, 1953), 532.

[20] M.I.T. Endicott House Conference, "Science and Public Policy: A Range of Issues" (mimeographed, 1959).

Whether these common perspectives about the public promote an affinity between scientists and media, or whether the bond is one of convenience—the scientists seeking an escape from secret decisions and the journalist finding a ready source of news—may be a moot question. But in contrast to the politician, the businessman, the labor leader, and the lawyer, the scientist in these examples enjoyed a privileged position in the eyes of most editorial writers. Although the "doctors" might be naïve in the political jungle, or even silly, they have been deemed well-intentioned, while members of other occupations have been fair game for press attacks.

The public image—the scientist as miracle worker. Not only the press but the general public as well is likely to offer comfort and support to the scientists' political objectives, even while it misunderstands them. Most polls designed to rate professional prestige put the scientist close to the top, although interlaced with sentiments of respect are expressions of doubt concerning the benefits of science and the personal characteristics of scientists. Mead and Metraux found a double image of the scientific career among high school students—one strongly positive and the other negative. As portrayed in popular literature, the Frankenstein potential of unrelieved destructiveness and unsanctioned tampering with nature's mysteries vie with the power of science to bring health, prosperity, and contentment to the masses. In instances where scientists have been involved as neighbors and friends in local controversies, the public's respect for their judgment is more likely to hinge on evaluations of personalities than on deference to specialized knowledge.[21]

Coupled with doubts as to the ultimate effects of science is a gross lack of information about the skill group and its work. Limited interviews conducted under M.I.T. direction of students, businessmen, and housewives indicate that neither the scientists active in political affairs nor major contemporary scientific accomplishments are visible to the public. On the contrary, the "sensational" scientists (Teller, von Braun, Salk) or the "oddities" (Pauling and Oppenheimer) are most familiar. Our samples all revealed chronic difficulties in naming distinguished scientists, their achievements, or their associations with public life.

---

[21] Margaret Mead and Rhoda Metraux, "Image of the Scientist Among High School Students," *Science*, CXXVI, No. 3270 (August 30, 1957), 384–90. On the dual image among adults see Gerald Holton, "Technological and Scientific Society," an address to the Carolina Symposium, March, 1960. On the scientist at home among friends and neighbors, M.I.T.'s power-structure analysis of Lexington, Massachusetts, has been revealing.

The ambivalence and ignorance of the public concerning scientific affairs may be less important than its conviction that science may offer the "way out." Whether the popular concept of the scientist is good or bad, or accurate or distorted, it at least provides the skill group with respect and often with considerable support, if and when it chooses to make use of it. What the public learns of science is essentially related to its spectaculars, where payoffs seem clear: medical discoveries, moon shoots, satellites, missiles, new educational techniques, and new gadgets. These images may miss the mark from the point of view of what science is really about or the need for basic research. Killian has deplored the fact that "it is the technological spectaculars which the public at large and, often, the press tend to use as the sole measure of scientific as well as technological prowess, and thus, of military power as well." [22] Whether or not this emphasis on the dramatic is a burlesque, it assigns to the scientists the fundamental attribute of power.

In the same address in which he scored the public's misconception of science, Killian acknowledged: "Since World War II the status-seekers in the community of nations have relied increasingly on science and technology to build their prestige." The very inability of the public to understand the scientific mind or its work, the lack of communication which scientists deplore, the aura of magic about their investigations, and the faith in research augment their position, at least in the minds of the Washington community where the scientists are most visible.

Contrasted with the shopworn image of the lawyer, where the public may easily be as ill-informed, but much more negatively persuaded, the scientist's position seems stronger. He is not yet identified with big business, notorious crimes, shabby practices, sharp tricks, unpopular court decisions, and unsettled convictions so as to earn the aphorism "to be a good lawyer is to be a poor Christian." [23] To be viewed as an oddity, as naïve in political affairs, or as inexpert in compromise in a nation traditionally suspicious of its public leaders may well be preferable to being regarded as members of the profession from which all politicians are naturally recruited.

---

[22] James R. Killian, Jr., "Making Science a Vital Force in Foreign Policy," address to the M.I.T. Club of New York, December 13, 1960.

[23] Doby, "The Lawyer as a Political Leader," pp. 308–13.

The scientist's self-image as "nature's agent." The possession of skills critically needed for national development and strength, access to, and perhaps rapport with the national communications networks, and public prestige are valuable political assets only if the skill group is disposed to use them. If scientists were plagued by self-doubts as to the utility of their calling and its effects on social and political affairs, they might operate under considerable restraints in the political arena. However, since Hiroshima, scientists have "known sin," and there is little evidence that their faith in the essentially benign effects of expanding knowledge or the enobling nature of their calling has been shaken. What James McCamy has called the "myth of science" has served to assure the skill group that its substantive work is not related to unfortunate political consequences, and that its participation in policy-making brings much needed qualities of reason and objectivity to the political process.

In this generation, spokesmen for science have articulated at least three versions of the scientific enterprise designed to establish its social value and public usefulness. The first version emphasizes the innocence of scientific inquiry. Compton insisted that science contains no moral imperatives in that the use of discoveries for good or evil is not determined by the discoverers. Bethe has asserted that science provides "a true picture of the world of living things so far as they are physical entities" but excluding "those things which man puts into the world."[24] The second version asserts the beneficial sweep of science: Vannevar Bush's classic statement of science's contribution to three great public goals—national security, conquest of disease, and increase in public welfare.[25] The third version expresses the conviction that scientists are specially endowed to bring order and sense to the political process: Snow's lyrical characterization of the scientific community as "tough and good men determined to fight it out at the side of their brother men," prepared to accept individual tragedy but not the tragedy of the human race, or Dean

---

[24] *Bethe Interview*, p. 8.

[25] Vannevar Bush, *Science, the Endless Frontier* (Washington, D.C., USGPO, 1945).

George Harrison's call in the M.I.T. Conference for the triumph of the scientific attitude over mysticism, inertia, and political disorganization.[26]

To the degree that these attitudes toward science are general within the skill group, a better impetus for unfettered political activity can scarcely be imagined. Not responsible for past errors in public policy, carrying the keys which will unlock the doors to a better way of life for all, and capable of bringing order out of political chaos, the scientists can enter the decision-making process secure in the knowledge that their activities are not impelled by personal ambition and the thirst for power. Reluctantly, they emerge from the laboratory at the time of a great emergency to save the nation or the world or, as at Pugwash, to use "the international community of science" as a basis for improved political understanding. They sacrifice professional careers in the interests of an informed debate on great public issues. Their function in the controversies over space, missile systems, or education is not to find new glory or power for their peer group, to promote new funds for a pet project, or to protect a vested interest. Rather, their mission is to assure that the wellsprings of creative achievement continue to flow, and that the same characteristics of reason, logic, respect for individualism, and objectivity are brought to bear on problems of human relations. Compared to the self-seeking, the parochialism, and the limited knowledge of other participants, the scientists offer the welcome contrast of prescient men concerned only with explaining and using the powers of nature. As Gilpin has explained, their higher duty is to their profession, in contrast to the lawyer's cynical willingness to forget his search for justice and serve only the interest of his clients.[27]

The issue is not whether the scientist's view of his political self is an accurate one. Nor is it relevant that a careful reading of public controversies involving scientists leads to a conclusion that personal ambition, avarice, and egotism can characterize the members of the scientific skill group as much as members of other groups. What is important is the fact that the view of science serves as an operative ideology sustaining the evaluation of the group's activity in time of stress. To believe simultaneously, as Bethe does, "that science has nothing to say or little to contribute to these human values" but that you "don't need much formal training in this [value choices]" is at once a reassurance of the fitness of one's work and an invitation to extend its sphere of influence because the mingling of technical and political advice is "unavoidable." [28] Secure in the conviction that the twentieth century is the age of Big Science, with the acceleration of innovation as its major theme, scientists can easily take the next step of extending the approach and spirit of scientific inquiry to human affairs. Problems of defense, of resources development, education, and space are not to be approached in an atmosphere of emotion, conflict, and controversy. Rather, effective policy-making requires objective sifting of the facts, balanced vision, thoughtful reflection, and "the mobilization of the best wisdom and highest competence available to a nation." [29] These work habits, presumably possessed by scientists, serve the larger purposes of human endeavor. Therefore, the presence of scientists in high policy councils is more likely to result in "better policy" than if they were to function in strictly technical and informational roles.

---

[26] M.I.T. Endicott House Conference, "Science and Public Policy."

[27] Robert Gilpin, "Civil-Science Relations in the United States: The Example of the President's Science Advisory Committee," unpublished paper presented to the American Political Science Association Convention, September, 1961, p. 8.

The disposition for action. Closely akin to the scientist's conception of the special benefits he brings to decision-making is his conviction that something can be done to resolve major public issues. That is, an analysis of a problem by his skill group is not only likely to proceed without the distortion of emotions and special interests but will also result in more definitive solutions. The participation of the group will make a difference. In Bethe's words:

> One major problem which one faces as a scientist lies in the difference in approach to the problem-solving process between scientists and many non-scientists. For instance, when one testifies before a Congressional committee one often has the impression that the purpose of the hearing is not to search out the facts and then reason a solution, but that the solution has been determined and the hearing will now put such facts on the record as will support the solution.[30]

---

[28] *Bethe Interview*, p. 15.

[29] James R. Killian, Jr., and Albert G. Hill, "For a Continental Defense," *The Atlantic Monthly*, CXCII, No. 5 (November, 1953), 41.

[30] *Bethe Interview*, p. 27.

Throughout the M.I.T. inquiries there is evidence of the scientists' instinct to rely on rationality and creativity as capable of bringing profound changes in an existing state of affairs, the urge to bring together a "think group" for a one-shot onslaught on a problem, and the preference for searching for bold, imaginative new approaches and deprecating the importance of obstacles and inpediments. Warner Schilling contrasts the scientist's perspective to that of the diplomat inclined to believe there are no solutions to his problem, or the politician desirous of finding the lowest common denominator of many approaches.[31] It also contrasts with the military attitude that although action is often required no ultimate change in a world of violence or conflict is to be expected; or with C. Wright Mills' administrator who listens intently to all sides in a dispute before suggesting soberly and cautiously that no action is warrented for the time being.

Just as the scientist's faith in his role as an agent of nature can be questioned, so his disposition for quick far-reaching solutions may be suspect. Albert Wohlstetter has suggested that the solutions of the skill group represent drastic oversimplifications of the issues, an apparent reliance on reason mixed with heavy doses of intuition that are never explicitly recognized, with solemnity, and with an unwarranted deprecation of politicians and generals.[32] Others have termed the scientist's posture "naïve utopianism or naïve belligerency," "intellectual arrogance," or the compulsions of a latent "social conscience."[33]

Once again, the correctness of the scientist's position in the political labyrinth is beside the point. In a context where urgent problems are piled one on top of another, where there are constant crises, and where progress is necessary, the scientist's disposition for action can be appealing. It can be especially appealing in a political system where conventional ideologies stress rationality, creativity, and ingenuity as important elements in the national style. It is most important to realize that when innovation and change is a public function, it may be difficult for even the most skeptical decision-maker to contain the proposals of the scientists. Men who see a way out in desperate situations, who have grand designs and bold new schemes, and who promise relief from age-old torments have political advantages not enjoyed by those intent on making marginal adjustments in a going system. The results of the proposals may be disastrous but they may still be politically potent. After all, the Women's Christian Temperance Union achieved the goal of prohibition for a time.

---

[31] See the essay by Schilling in this volume. See also E. B. Skolnikoff, "Science, Scientists and National Policy in the United States," unpublished paper presented to the American Political Science Association Convention, September, 1962.

[32] See the essay by Wohlstetter in this volume.

[33] See the essay by Schilling in this volume.

The arena of the executive branch. If one simply counts the noses of persons in formal positions of authority or traces the sweep of politics from nominations and campaigns through final administrative action, scientists do not appear frequently enough to seem significant. This is simply to say that the talents of the skill group are not marketable political commodities everywhere in the political system. What is important is that the arena in which the skill group demonstrates competence is precisely the one where the public decisions that touch scientific concerns and, in fact, the long-run fate of the political system, are made. In the modern age of the executive and bureaucratic state, contemporary political analysis fixes the locus of policy-making away from the legislative halls and great public deliberations. Decisions on military strategy, space explorations, budgetary allocations, foreign policy, medical research, and transportation facilities are in the first instance almost always formulated within the executive branch, and are frequently conclusively determined there. If a small number of key legislators can be brought in, a few prominent members of the press briefed, and an advisory committee established for prestige purposes, then the "cardinal choices" are made in executive chambers. The legislative process, the prevailing public sentiment, and the articulate opinion of organized interest groups operate as constraints, reduce the number of feasible alternatives, and sometimes upset the calculations of the policy-makers. They do not, however, alter the prevailing concentration of power to propose, persuade, and establish the critical policy positions which the insiders now possess. So, at the height of the DEW line controversy the Alsops could write from the normative vantage point of established democratic ideology:

> It was ludicrous and it was also pretty shocking to listen to the windy public debates of the last campaign, and meanwhile to have a general notion of the topics of secret debate . . . [the leaders of government] were privately arguing about the increasing danger of a full scale catastrophe in South Asia; the increasing Chinese Communist threat on the approaches of Formosa . . . the desperate need for an American air defense . . . the urgent need for a mighty effort to overtake the Soviet lead in guided missiles . . . and other such grim matters. You could almost say in truth that while the politicians were solemnly discussing the President's multi-billion highway construction program, those with Q-clearance were discussing whether there would be any Americans left to ride the highways.[34]

Spokesmen for science often protest against this structure of decision-making as secret choices made by a handful of men. Yet, from the point of view of maximizing their own influence, it is difficult to conceive of a more advantageous location for policy-making. The skill group is not required to possess the personality attributes needed for election to public office, or to observe the stylized dialectic ritual of legislative or judicial proceedings. Rather, its participation is confined to a process of conferences, interviews, committees, task forces, memoranda, and close personal communications where technical and reasoned arguments are permitted, where the capacity to articulate a particular position lucidly or to summarize data in comprehensive terms and extract their logical implications persuasively are important assets. In this arena, consensus may be achieved precisely on the basis of the performance for which scientists are best equipped or through coalitions built in terms of a relatively few personalities, the marriage of interests among a limited number of agencies and departments, or the impact of a well-executed briefing. With the line between technical advice-giving and policy-making obscure, and the emphasis on documents, consultations, proposals, and counterproposals continuing, the scientific skill group works in an atmosphere which reflects their best personalities to fullest advantage.

The ideology and work habits of the scientist in bureaucracy can lead to special advantages. Other occupations can also argue, reason, and, to a limited extent, propose, but they are likely to be constrained by the shibboleths of adminstrative doctrine: notions of responsibility and loyalty in the case of the general administrator; respect for procedures, processes, and the position of his client in the case of the lawyer; and at least formal acknowledgment of the concept of civilian supremacy in the case of the military officer.

---

[34] Joseph and Stuart Alsop, "That Washington Security Curtain," *Saturday Evening Post*, CCXXVII, No. 34 (February 19, 1955), 33.

The scientist often has no such inhibitions. His higher loyalty is to the scientific community and the preservation of its integrity and vitality. In the executive setting, it is a help rather than a hindrance that this community is not organized and lacks clear channels of communications. The natural base for the scientist is the university, and he is fond of pointing out that he would return there happily, not reluctantly as the lawyer might return to private practice. His disposition is to value individual, unorganized activity. Hence, the scientist is often prepared to ignore established channels, cut red tape, minimize the importance of touching base, make strategic contacts with press and public, and fume at delay and procedural "nonsense." As a "wild man" that must be handled with kid gloves he can secure the advantages of his executive environment while ignoring the traditional limitations of action in a large organization. A predilection for violating the rules of the game often leads to greater opportunities for winning the game. In McCamy's words:

> Scientists, whether they think so or not, still have a favored status now in the emotions of public administrators. They walk into a room where lay administrators still stand in awe of imagined mystery. That myth of science, the picture of the infallible specialist in immutable fact, lingers on in the minds of administrators.[35]

In an institutional framework undergoing rapid transformations, where the lines between public and private activities are increasingly blurred, contractual relations replace hierarchical relations, and notions of authority and responsibility become more and more difficult to implement, the freewheeling perspective of the scientific skill group may be a more accurate appraisal of the reality of administrative influence than that of other skill groups still carrying more conventional models of political strategy in their heads.

---

[35] McCamy, *Science and Public Administration*, pp. 204, 208.

The scientist may not as yet understand how he wins the day and what the sources of his strength are, but his instinctive behavior and natural predilections fit the circumstances. He possesses technical information critically required by top decision-makers, he forecasts the effectiveness of innovation, and he can articulate his conviction with few restraints. As he comes to participate in executive politics, he may play the game with zest and even ruthlessness, and still disclaim any understanding or involvement in it. In short, as Wohlstetter suggests, he may practice the politics of Mr. Dooley while remaining innocent of the precepts of Aristotle. No wonder other participants view his emergence with some discomfiture.

THE APOLITICAL ELITE

We have speculated that the study of civil-scientific relations can fruitfully proceed from a model of skill politics which treats the scientists as an emerging elite with influence based upon, but not limited to, its particular type of technical competence. We have suggested that analyses which see the scientist simply as another expert, or part of a poorly organized pressure group or an amorphous community, are likely to underestimate his potential. The relevant measures of scientists' present influence are not visibility in the public eye, cohesions within the scientific community at large, accurate impressions of scientific personalities and their contributions by the media, or even the degree to which scientists attain the goals they profess they wish to attain. What matters is their important role in the expanding set of decisions which resolve around the new public responsibility for innovative excellence and the need for both technical and political judgment.

There are those who counsel now that the prime deficiencies in the pattern of civil-scientific relations is the failure of the scientific skill group to recognize the true character of the political process, including the demands it makes on general knowledge and the compulsions it contains for seat-of-the-pants decisions made with a sensitivity for competing values. The scientist is scored for carrying in his head at the same time the simplified version of the democratic system born in the Age of Reason and a self-image akin to the Platonic navigator. The ideal arrangement for science within the federal government is seen as one in which the general administrator serves, in Don Price's words, "as a useful layer in the pyramid of policy between the peak of political power and the base of science and technology." [36] In this view, the scientific skill group would be admitted to positions of influence only after its members had been steeped in the humanities and social sciences so that they "can appreciate the problems faced by the administrator and the politician." [37]

In terms of skill politics analysis, these proposals seem to miss the mark on two counts. First, they fail to recognize that the scientist's greatest possibility for enhancing his influence lies in the continued differentiation of his skills from those of other experts. It is the maintenance of his magic, the continuation of his separate expertise, the preservation of his independent base in a university, and the admiration and respect he can evoke from press and public and apply within executive confines and the social order that have moved the scientist ahead in politics and created the special agencies and offices for him against the opposition of general administrators. It is asking a great deal to require even a skill group which disavows conscious and personal political ambition to surrender its major instruments of influence.

Second, those who would "regularize" the scientific skill group in the executive arena where they are most preeminent miss the particular affinity that group has to American political ideology. They fail to acknowledge the acceptable theoretical posture which the scientist's apolitical behavior achieves. It may well be that contemporary students of our politics correctly describe our political process pluralistically as multiple "whirlpools of influence," a web of subgovernments, a tortuous path of conflict, bargaining, negotiation, and compromise in which the ultimate end is to "muddle through." As Price has pointed out, superimposed over that reality is a set of myths and symbols conceived in the Age of Reason, paying deference to individuals, not groups, expecting rational behavior, and giving due weight to strokes of genius such as the "invention" of the Constitution that is devoted to a government so carefully balanced in its distribution of powers and procedures that it might be expected to function according to some version of Newtonian physics.

---

[36] Price, *Government and Science*, p. 186.

[37] *Ibid.*, p. 203.

This ideology is doubtless a fiction for it expects no fundamental conflicts and looks forward to the almost automatic resolution of momentary disagreements through the process of sweet reason, sanctified by custom and law, and embedded in folklore. It is, nevertheless, a persuasive fiction and an operative one, and the scientists' espousal of it places them in the mainstream of American political thought. At the same time, those who "know" or work in the real world of politics with its irrationality, repetitiousness, and deep divisions gain no lustre by their greater wisdom. They lose status and influence in conventional philosophy by their very pragmatism.

Of course, the power and appeal of the scientific skill group may bode ill for the Republic. Misconceiving the complexities and intractability of the real world of politics, the scientists may be just another band of Niebuhr's "foolish children of light." All the criticism of their naïveté, arrogance, and irresponsibility may be justified.

But it may also be that the scientists' greatest failing has been that to date they have not actually applied their skills competently and seriously to an analysis and manipulation of the political process. They have not paused to become versed in new types of data or bothered to observe seriously the phenomenon with which they are now concerned. They have encountered trouble by being casual or careless, or even diffident, in their study of the political world not because they function as poor politicians, but because they function as poor scientists. Perhaps those who now best represent the scientific and the revolutionary tradition in policy-making are that small but growing number of experts variously dubbed civilian strategists or operations and systems analysts. These are social scientists working seriously at the business of applying the techniques of empirical investigation and controlled experimentation to political aspects of human behavior.

If the actual state of affairs is that we are only at the threshold of the application to public problems of science, broadly conceived, then an accurate evaluation of the function of the present skill group is not that its members should learn new skills or limit their advice to physical matters. On the contrary, the prudent political decision-maker would invite members of this group to exercise their curiosity in the solution of political puzzles and extend their scope of inquiry in the manner which they would employ in their investigations of less complex subjects. He would not seek to beat them in the political arena by the exercise of his instinct, intuition, common sense, or incompletely understood experience—he would seek to join them.

The Scientific Adviser

HARVEY BROOKS

Dean of the Division of Engineering and Applied Physics of Harvard University
and member of the National Science Board,
the President's Advisory Committee, and the
Naval Research Advisory Committee

Throughout American history the federal government has used scientific advisory committees made up of part-time outside consultants. Since World War II this practice has flourished, and has even become institutionalized in the form of statutory scientific advisory committees. The function of giving scientific advice to the federal government has begun to assume a professional status, and, as Gilpin points out,[1] a hierarchy of part-time advisory groups has emerged that parallels the bureaucratic hierarchy within the structure of government. This interesting development has accompanied the rapid increase in the use of contracts and grants by federal agencies to support research and development in the private sector—in industry, universities, and research institutes. It is difficult to decide which is cause and which is effect, but there is little doubt that federal support for private research and development and scientific advising have gone hand in hand.

Government scientific advisory committees form a complex interlocking network and many scientists and engineers are members of committees at several different levels in the hierarchical structure. In some cases this overlap is deliberate; for example, in the Department of Defense the chairmen of the advisory committees to the three military services are automatically members of the Defense Science Board, which advises the Secretary of Defense. In other cases the overlap is accidental; the same individual is co-opted for different committees on the basis of his individual talents and experience. In both instances, this overlap forms a parallel communication network within the federal government which to a very considerable extent circumvents the customary bureaucratic channels. This bypassing of the bureaucracy is probably one of the most important and useful functions of scientific advisory committees. In science and engineering no level of the bureaucracy has a monopoly on new ideas, and the loose nature of the advisory system provides one means by which ideas originating at a low level in the bureaucratic structure can be brought directly to the point of decision without going through regular channels, and new ideas from outside the federal structure can be introduced quickly into governmental operations.

---

[1] See essay by Gilpin in this volume.

We shall begin with an examination of some functions of scientific advisers, and will then examine the roles of scientists in the advisory process, the qualities and skills of scientists that are called upon when they give advice. Finally, we shall discuss a number of problems and conflicts which arise in scientific advising.

FUNCTIONS OF SCIENTIFIC ADVISORY COMMITTEES

The term "scientific advisory committee" is used generally although such committees are often as concerned with technology and engineering as with science and include many engineers or other applied scientists among their members. The role of the adviser varies greatly, depending upon the level in the federal hierarchy at which his advice is sought and implemented. In general, the lower the level, the more strictly technical the nature of the advice sought, although this is not always the case.

We may distinguish five advisory functions:

1. To analyze the technical aspects of major policy issues and interpret them for policy-makers, frequently with recommendations for decision or action. At the highest levels this often involves the analysis of political issues to determine which issues are political and which can be resolved on a technical or scientific basis. It also involves interpreting the policy implications of technical facts, opinions, or judgments. Familiar examples of this type of advice are such questions as whether to seek a nuclear test ban or whether to resume nuclear testing and, if so, when.

2. To evaluate specific scientific or technological programs for the purpose of aiding budgetary decisions or providing advice on matters affecting public welfare or safety. Many important decisions involving choice between alternate weapons systems, or determinations of whether to proceed with major technological developments such as civilian nuclear power are of this nature. The review function of the Advisory Committee on Reactor Safeguards of the Atomic Energy Commission (AEC) is an example of such use of scientists in the area of public safety.

3. To study specific areas of science or technology for the purpose of identifying new opportunities for research or development in the public interest, or of developing coherent national scientific programs. Such studies may be science-oriented, that is, concerned with specific scientific disciplines as in the work of the Committee on Oceanography of the National Academy of Sciences (NAS)[2] or the Panel on High Energy Physics of the President's Science Advisory Committee (PSAC). They may also be need-oriented, that is, concerned with the use of science for a specific social purpose, as in the case of the recent study on natural resources made by the National Academy.[3] These studies may be conducted either on a continuous or *ad hoc* basis.

4. To advise on organizational matters affecting science, or a particular mission of an agency involving the use of science or scientific resources. The continuing advisory boards of the military services and the Defense Science Board serve mainly this function. A recent example is the recommendation of a PSAC panel which led to the establishment of the National Aeronautics and Space Administration (NASA), or the recommendation of another panel for the creation of the Federal Council on Science and Technology (FCST).[4]

5. To advise in the selection of individual research proposals for support, as in the so-called "study sections" of the National Institutes of Health, or the Advisory Panels of the National Science Foundation.

---

[2] NAS-NRC, Committee on Oceanography, *Oceanography 1960 to 1970* (Washington, D.C., NAS-NRC, 1959).

[3] NAS-NRC, Committee on National Resources, *Natural Resources; A Summary Report* (Washington, D.C., NAS-NRC, 1962), Publ. No. 1000.

[4] PSAC, *Strengthening American Science* (Washington, D.C., USGPO, 1958).

With the possible exception of the fifth designation, none of these functions is purely scientific in nature or depends purely on technical knowledge or expertise. All recommendations involve nontechnical assumptions or judgments in varying degrees. In some cases the nontechnical premises are provided by the policy-maker seeking the advice, but more often they have to be at least partly supplied by the scientist himself. For instance, in the judgment as to the safety of a nuclear reactor installation, "safety" itself is a relative term. In a sense the only truly safe reactor is the one which is never built. Every technical judgment on safety is actually a subtle balancing of risk against opportunity—the tiny risk of injury to the public against the advantages of nuclear power. Yet the administrator seldom makes clear to the adviser just how this balance between advantage and risk is to be achieved. Much of the apparent disagreement among scientists over the danger of fallout from bomb tests stems not from a conflict as to actual technical facts, but rather from a difference of views as to the relative weight to be assigned, on the one hand, to the political and military risks of test cessation and, on the other hand, to the possible threat to human welfare resulting from the continuation of testing in view of the large uncertainties in our knowledge about radiation effects.

In a somewhat oversimplified way, the functions of the scientific adviser may be divided into those concerned with science in policy and those concerned with policy for science. The first is concerned with matters that are basically political or administrative but are significantly dependent upon technical factors—such as the nuclear test ban, disarmament policy, or the use of science in international relations. The second is concerned with the development of policies for the management and support of the national scientific enterprise and with the selection and evaluation of substantive scientific programs. It is not possible to draw a sharp line between these two aspects. For example, the negotiations over a nuclear test ban—which certainly involved science in policy—led directly to recommendations for a greatly expanded program of federally supported research in seismology, with quite specific suggestions as to the particular areas of promise—which is obviously policy for science. Conversely, the proposal for an International Geophysical Year, which was essentially a very interesting and exciting scientific proposal, involved highly significant political considerations and in many ways became an important tool of U.S. foreign policy.

ROLES OF SCIENTIFIC ADVISERS

What are the particular qualities and skills demanded of scientific advisers? What kind of knowledge and experience do they bring to bear? Although the public image of the scientific adviser is primarily that of expert or specialist, the way in which he is actually used is much broader. One may distinguish at least seven different roles which closely relate to the above five functions.

The scientist or engineer is used for his expert knowledge of particular technical subject matter, as in the study sections of the National Institutes of Health whose function it is to rate research proposals.

He makes use of his general "connoisseurship" of science and scientific ways of thinking. In this role he is required to transfer his scientific experience from fields in which he is expert to fields of science and technology with which he is only generally familiar. He is used for his ability to understand and interpret quickly what other experts say, to formulate general policy questions involving scientific considerations in terms suitable for presentation to a group of experts, and to detect specious or self-serving arguments in the advice of other experts.

He makes use of his wide acquaintance within the scientific community and his knowledge of scientific institutions and their manner of operation. In this role he often helps by suggesting key technical people to serve in full-time government positions, and even in persuading the preferred candidate to accept the appointment. He may also predict the effects of government policies or actions on scientific institutions, and serve the ends of both government and science by defending the scientific community against ill-advised or inappropriate administrative procedures of government affecting the conduct of research and development.

The public administrator often makes use of the confidence and prestige enjoyed by scientists in order to obtain backing for projects or policies which he has already decided to undertake. While this particular use of scientific advisers is not necessarily to be deplored, it is subject to abuse. In many cases the administrator may "stack" his committee to obtain the advice he wants, or to obtain a "whitewash" for doubtful decisions. On the other hand, an advisory committee may often legitimately be used to help an administrator rescind an unwise decision without humiliation or embarrassment. A famous example is the appointment of an advisory committee of the NAS to investigate the National Bureau of Standards at the time of the battery additive controversy.

The scientist is increasingly being used as a specialist in policy research. This practice began during World War II with the development of the science of operations research, mainly by physicists in Great Britain and the United States. It involves the construction of mathematical models of varied military situations and the quantitative prediction of the military results of the use of varied weapons systems and strategies. During the postwar period this methodology was extensively elaborated by mathematicians, economists, and theoretical physicists, and has been institutionalized in such organizations as the RAND Corporation, or the Weapons System Evaluation Group which serves the Joint Chiefs of Staff. A number of amateur and professional groups have also developed to carry on policy research in the field of disarmament or arms control. In all these examples natural and social scientists collaborated in order to bring to policy problems the methods of analyzing problems which are characteristic of the physical sciences.

Scientists are often sought for policy advice merely because the scientific community provides a convenient and efficient process for selecting able and intelligent people. One is reminded of Macaulay's dictum that he wanted "to recruit university graduates in the classics not because they had been studying the classics, but because the classics attracted the best minds, who could adapt themselves to anything."[5] If one substitutes nuclear physics for classics in this quotation, one has a basis for the selection of certain kinds of advisory committees. It is also probably true that physicists have a way of simplifying problems which is especially useful to harassed administrators—a capacity which has its pitfalls, since the temptation to oversimplify is always present.

---

[5] See reference to this in the essay by Price in this volume.

According to C. P. Snow, science is more oriented toward the future than most other disciplines, and scientists are animated by a belief that problems are soluble.[6] Such natural optimism, even when unjustified, is an asset in attacking disarmament problems which have resisted solution for such a long time. It is undoubtedly a characteristic which has brought scientists into policy advisory roles even in areas where they are not especially qualified. Similarly, science forms the most truly international culture in our divided world, and scientists probably enjoy better communication with their counterparts throughout the world than members of any other discipline. Consequently, it was natural that scientists should lead in the cultural penetration behind the iron curtain, and in organizing and promoting joint international activities and exchanges.

PROBLEMS IN SCIENTIFIC ADVISING

The preceding discussion demonstrates that specific expertise is only a small part of the scientist's role as adviser. It is this very fact that is responsible for much of the controversy surrounding the present role of scientists in government and especially their role in the White House and the Executive Office of the President. The criticism of scientists is epitomized by Harold Laski's stricture on expertise:

> It is one thing to urge the need for expert consultation at every stage in making policy; it is another thing, and a very different thing, to insist that the expert's judgment must be final. For special knowledge and the highly trained mind produce their own limitations which, in the realm of statesmanship, are of decisive importance. Expertise, it may be argued, sacrifices the insight of common sense to intensity of experience. It breeds an inability to accept new views from the very depth of its preoccupation with its own conclusions. It too often fails to see round its subject. It sees its results out of perspective by making them the centre of relevance to which all other results must be related. Too often, also, it lacks

---

[6] C. P. Snow, *Science and Government* (Cambridge, Mass., Harvard University Press, 1961). See also essay by Schilling in this volume.

> humility; and this breeds in its possessors a failure in proportion which makes them fail to see the obvious which is before their very noses. It has also a certain caste spirit about it, so that experts tend to neglect all evidence which does not come from those who belong to their own ranks. Above all, perhaps, and this most urgently where human problems are concerned, the expert fails to see that every judgment he makes not purely factual in nature brings with it a scheme of values which has no special validity about it. He tends to confuse the importance of his facts with the importance of what he proposes to do about them.[7]

The view so eloquently expressed by Laski is sometimes echoed by political scientists and government administrators in referring to the present influence of scientists in the high councils of government. While it is a valid warning against the uncritical acceptance of scientific advice, particularly where tacit ethical and political judgments are involved, it is not exactly a fair description of the way in which the senior scientific advisers have exercised their responsibilities. Scientists in government have not claimed that their advice should be overriding, but they do insist on the value and importance of this advice in reaching a balanced decision in matters involving the use of scientific results. This is true even when the scientist is speaking primarily as a citizen outside his area of special competence. Especially on matters of military technology, scientists are often in a position to exercise their political and ethical judgments as citizens in a more realistic and balanced manner than other citizens. Precisely because they are so familiar with the technological aspects, they are able to concentrate more on the other issues involved without becoming bemused by mere technical complexities. While scientific advice is not free of bias, or even of special pleading, it is probably more free of prejudice than much other professional advice, and at least has the virtue of providing a fresh perspective unprecedented in government councils.

A number of problems arise in scientific advising which are, in one way or another, related to the type of problem raised by Laski in the above quotation. Discussion of these problems can be organized under eight topical headings: (1) the selection of advisers, (2) the scientific adviser as the representative of science, (3) communication between the scientist and the policy-maker, (4) the relation between advice and decision, (5) the responsibility of the adviser, (6) the resolution of conflicting viewpoints, (7) executive privilege, and (8) conflict of interest.

---

[7] Quoted by I. L. Horowitz, "Arms, Policies, and Games," *American Scholar*, XXXI, No. 1 (January, 1962), 94. The original source of the quote is Harold Laski, "The Limitations of the Expert," *Fabian Tract* No. 235.

SELECTION OF ADVISERS

Although the method of selecting members of a scientific advisory committee depends strongly on the function of the committee, the usual procedures are rather informal and based, for the most part, on personal acquaintance. This is especially true of committees that operate at the higher levels of policy discussion. The final selection is made by the executive to whom the committee is responsible, but usually the executive accepts the suggestions of present members of the committee or of other advisory committees. Thus the advisory role tends to become self-perpetuating, and constitutes a kind of subprofession within the scientific professions. Certainly administrative skills and some degree of political sophistication are factors almost as vital as scientific competence and reputation in the selection of members for the top committees. Experience in one of the major wartime laboratories, especially the M.I.T. Radiation Laboratory and the laboratories of the Manhattan Project, or an apprenticeship on one or more of the military "summer studies," still appears to be a useful qualification for scientific advising. There is as yet little sign of a change of generations that would affect this pattern. Even the relatively few younger scientists who have filtered into the higher-level advisory committees are often students of one of the wartime giants like Rabi, Teller, Oppenheimer, or Fermi. Full-time administrative experience in the federal government or long experience on lower-level advisory committees or panels are also important qualifications. One of the most common methods of evaluating possible candidates for membership on the PSAC is a tryout on one of its numerous specialized panels. In this process of selection for advisory committees the characteristics deplored by Laski often tend to be weeded out.

REPRESENTATION OF THE SCIENTIFIC COMMUNITY

The higher-level committees are often criticized for inadequately representing some particular disciplines, certain kinds of institutions, or some points of view on major national questions such as disarmament. For example, the PSAC has been criticized for having too many physicists and not enough engineers, too many academic scientists and too few industrial scientists, too many scientists from the east and west coasts and too few from the central areas of the nation, too many scientists who are prepared to negotiate with the Soviets and too few representing the school of thought of which Professor Edward Teller is the most articulate spokesman. All of these criticisms have some factual basis and yet it is essential to remember that scientific advisory committees are not legislative bodies, that the ability to reach a large measure of consensus and settle matters by a good deal of give and take in rational argument is much more important to the policy-maker than assurance of equal representation for all the "estates" of science and technology. People with very strong viewpoints which are impervious to rational argument or compromise merely tend to lead to a hung jury which does not help the decision-maker. A majority vote is much less useful than a well-reasoned consensus in providing scientific advice.

Many of the criticisms regarding lack of representation are either untrue or grossly exaggerated. For instance, there have always been industrial scientists on the PSAC. Although a majority of the Committee are physicists, it also consists of engineers, life scientists, and members of the medical profession. A high representation of academic scientists on the presidential committees and panels is balanced by a predominant representation of industrial scientists and research directors on the Defense Science Board and the top advisory committees of the military services. Yet to some extent this is beside the point. The members of the Committee are supposed to be selected for their ability to look at problems in broader terms than those of their own corner of science. The advice of a committee is not the sum of the individual expertise of its members, but a synthesis of viewpoints of people accustomed to looking at problems in the broadest terms. The high proportion of physicists stems from their wartime experience and their subsequent military advisory experience, for it must be remembered that scientists came into the top advisory role in government via their contributions in the national security field, and it was only later that they became concerned with the broader problems of basic science policy and the impact of science on international affairs.

It is important to bear in mind that on any given problem it is the practice of most scientific advisory committees to delegate much of the groundwork to panels whose memberships are carefully chosen to reflect the scientific and engineering skills required in the solution of particular problems. Furthermore, if the issue to be resolved is politically controversial, a special effort is made to ensure representation of a wide spectrum of viewpoints on the panel even though such a variety may not be represented on the parent committee. At the same time it is important to avoid people who are so committed to one view that a discussion of real significance is impossible. A surprising measure of agreement can be reached by a group of scientists of divergent views when they are partially protected by individual anonymity and not constrained by the need to be consistent with previously voiced public positions.

The Special Assistant to the President for Science and Technology, as well as the PSAC, is often thought of as the "spokesman" of science. It is sometimes said by both scientists and nonscientists that the members of the PSAC should be lobbyists for the interests of the scientific community and promoters of science. This feeling came about partly because the creation of the Office of Special Assistant was the result of public concern over inadequate national attention to the cultivation of national scientific strength. An important part of the task initially facing the Special Assistant was to promote the support of science, particularly basic science, in every way possible.[8] Actually, the PSAC and the Special Assistant are not, and should not be, official spokesmen for science but are organs of government. They regard themselves not as advocates of science, but as mediators between science and government. In this function they feel an obligation to take into account the needs and interests of the government as a whole and not just the needs of the scientific community. It is the NAS and its committees which are and should be the advocates of science. The enthusiasts for a particular field of science are represented on these committees, but their reports are criticized and reviewed by the Special Assistant, by the PSAC and its special panels, and by panels of the FCST. This process is an attempt to balance the demands of a special field against the over-all requirements of science, and to adjust the requirements of a special program to the fiscal and administrative limitations of the federal government. It is still an imperfect process which has not yet been fully tested. In recent years science budgets have been rising so rapidly that the problems of maintaining balance between fields of science within the confines of severely limited resources have not had to be faced.

---

[8] See essay by Kreidler in this volume.

COMMUNICATION WITH THE POLICY-MAKER

Scientific advisers are frequently criticized for their tendency to expand their role beyond that of purely technical advice into the political, financial, and organizational spheres. The point is a valid one, but this tendency is inherent in the nature of scientific advising rather than a deliberate effort at usurpation on the part of scientists. Science makes progress largely by redefining the key questions and problems. The scientist approaching a policy problem wants to begin by understanding the whole problem, in order that he can break it down into its components in his own way. This habit, learned from his experience as a scientist, has been strongly reinforced by his wartime experience in solving military-technical problems. As Rabi puts it: "[T]he military man who doesn't come clean on the *whole* problem is like a patient who doesn't tell his doctor all the symptoms." An important part of the task of a scientific adviser is to define just what the technical issues are that have a bearing on a given policy decision. The policy-maker who tries to define the technical issues himself will not obtain the best advice from his scientific advisers. Similarly, the scientific adviser has an obligation to interpret his advice in terms of its policy implications, while at the same time trying to make explicit the nontechnical assumptions which necessarily underlie his recommendations. The policy-maker and the adviser together have the obligation to differentiate between technical and political questions, as well as between what is actually known and what is a matter of professional judgment.

The science adviser cannot always be blamed when he steps outside the technical area. Too often the politician or administrator is tempted to throw the onus of difficult or controversial political decisions onto his scientific advisers. An important decision may be much more palatable to the public and to Congress if it is made to appear to have been taken on technical grounds. In the early days of the negotiations on a nuclear test ban, both scientists and diplomats fell into the trap of believing that the basic issues were primarily technical ones which could be resolved by discussions among experts, if not at the time, then later on as new scientific knowledge became available. Subsequently, it became increasingly clear that the really difficult issues were related to the degree of assurance which the United States felt it must have against the conduct of clandestine underground tests, and to the Soviet judgments as to what would be the acceptable degree of penetration of their military security. The importance of detecting clandestine underground tests has been differently estimated by the United States, depending on judgment as to the military decisiveness of tactical nuclear weapons. The winds of public and governmental opinion appear to be too much influenced by day-to-day changes in technical developments and ideas such as the "Latter hole" or the Tamm "black boxes." [9]

The nonscientist often has an exaggerated faith in the exactness of physical science, and has great difficulty in distinguishing between what is known with a high degree of certainty and what is only a matter of reasonable probability or scientific hunch. Under pressure to make concrete recommendations, the scientist has often tended to exaggerate the validity of his data and to permit the administrator to erect an elaborate superstructure of policy on a very flimsy technical base. Something of this sort happened twice in the nuclear test ban negotiations when much too general conclusions were drawn from fragmentary data obtained from one particular U.S. underground test. As in military decisions, policy decisions can seldom be made with all the necessary information available, and the scientist who refuses to commit himself until he considers his data completely adequate is not very useful to the administrator. He does, however, have an obligation to explain the areas of uncertainty to his political master and to prepare him as best he can for technical surprises.

---

[9] U.S. Disarmament Agency, State Department, *Geneva Conference on the Discontinuance of Nuclear Weapons Tests; History and Analysis of Negotiations* (Washington, D.C., USGPO, 1961), Publ. No. 7258.

ADVICE VERSUS DECISION

Political scientists writing about the PSAC and other advisory groups have often tended to confuse advice with decision, thereby investing the PSAC with a power and responsibility which it does not in fact possess. It is true that the scientist's public prestige occasionally gives his advice an influence and authority with decision-makers. He thus has power which is in some respects equivalent to, though not identical with, political power. Ultimately the scientific adviser recognizes that his influence in government rests solely on the degree to which his views are verified by subsequent events. He has no true constituency to give his advice political force, and perhaps to a greater degree than in the case of any other type of adviser his influence must rest with the persuasiveness of his arguments. Professor Bethe's prestige as an adviser was, perhaps unjustifiably, dimmed by his failure to anticipate the possibility of decoupling in underground nuclear explosions. At the same time, his prestige would have suffered much more if he had not been so quick to accept the new technical suggestion and examine it on its own merits without reference to its effect on his own deeply held political convictions regarding the desirability of a test ban.

Congressman Melvin Price has publicly attacked the PSAC for its role in the decision to abandon the aircraft nuclear propulsion program (ANP),[10] implying that it alone was responsible for this decision and that its recommendation was based on political and budgetary grounds. While there is no doubt that nontechnical considerations played an important role in the actual decision, the PSAC's part was to give advice on primarily technical grounds, as has been clearly stated by Kistiakowsky.[11] Contrary to Congressman Price's implications, the responsibility for the ANP decision was shared by many administrators and advisers; the voice of the PSAC was only one among many voices, and this voice was probably not decisive. Such decisions within the executive branch are seldom reached through the advice of a single group or individual but are the result of a gradually evolving consensus among many advisers.

---

[10] Melvin Price, "Atomic Science and Government—U.S. Variety," an address delivered to the American Nuclear Society in Washington, D.C., on June 14, 1961.

THE PROBLEM OF RESPONSIBILITY

The scientific adviser differs from the military adviser in government in that he is seldom responsible for carrying out his own advice, or even for the consequences of the advice he has given. This situation has both advantages and disadvantages. It can lead to the particular type of irresponsibility which Laski describes above so graphically, or it can permit a degree of detachment and objectivity which would be difficult to achieve if the adviser were more deeply concerned with the consequences of his advice. The advice given by the Joint Chiefs of Staff has always been plagued by parochial service interests.

The problem of the responsibility of advice comes to the fore in the field of budgetary decisions affecting science. Unfortunately, advisory committees of scientists are seldom presented with the hard choices between attractive alternatives which usually concern the budgetary officer or administrator. The competing claims of different fields of science have yet to be squarely presented to a scientific advisory committee. When confronted with the virtually unlimited opportunities in a scientific field, the advisory committee is tempted to recommend expansion without much reference to other alternatives. The balance between scientific fields in the past has been determined by the somewhat accidental resultant of many pressures, both political and scientific. The committees that recommend expansion, while not achieving all they hoped for, generally see enough effect from their recommendations to be reasonably satisfied. In an era of expanding scientific budgets this has worked fairly successfully, and the general balance of scientific effort seems to have been preserved. The scientific adviser may find himself faced with an entirely different responsibility when the time comes, as it inevitably will, that scientific budgets level off. The scientist who has to live with his professional colleagues outside government, especially in universities, will find himself torn between his natural inclination to appear as the champion and promoter of science on every occasion and his sense of responsibility as a government adviser.

---

[11] George B. Kistiakowsky, "Personal Thoughts on Research in the United States," in *Proceedings of a Conference on Academic and Industrial Basic Research* (Washington, D.C., USGPO, 1960), NSF 61-39, pp. 49–53.

RESOLUTION OF CONFLICT

In controversial issues the ideal advisory committee is one which succeeds in enlarging the area of agreement and reaching as wide a consensus as possible. A wise committee or panel can often succeed in narrowing the disagreements on a complex issue to a few technical issues which might be resolved by further research or to a clean-cut set of political alternatives which can then be resolved by the administrator. The committee which strives for consensus at all costs usually ends up with a series of pious platitudes which are useless to the policy-maker—and useless in a peculiarly irritating and frustrating way. Having reached the widest possible area of agreement the committee should then attempt to formulate the disagreements as clearly and objectively as possible. Recommendations should generally be formulated in terms of forecasts of the probable consequences of alternative actions rather than in terms of exhortation. The responsibility for these tasks rests, for the most part, with the chairman, on whom usually falls the further duty of interpreting both the agreements and the disagreements to the policy-maker. This calls for unusual objectivity and detachment on the part of the chairman. In some cases it is wise for the policy-maker himself to hear the arguments of both sides directly from the proponents rather than filtered through the chairman.

It has sometimes been suggested that major issues involving technical advice should be resolved by a sort of adversary procedure, as in a court of law. There are instances when this is desirable, as, for example, when the rights or interests of individuals or groups may be in jeopardy as a result of the decision to be made. However, on broader policy issues the advisory process should be designed to encourage convergence rather than divergence of views. An adversary procedure tends to produce a polarization of viewpoints which then must be resolved by the policy-maker himself. The administrator would be forced to immerse himself in the technical details of every decision as does a judge in patent litigation. Given the many decisions which have to be made by every administrator, and especially by the President, such a process would be ludicrously cumbersome and would paralyze decision-making.

The alternative suggestion that every advisory committee should include a devil's advocate, is probably a good one. There are times when the chairman should deliberately assume this role. Sometimes it is the only way that the strongest arguments for the committee's final position can be brought out and potential objections to its recommendations anticipated.

On the whole the greatest occupational hazard of advisory committees is not conflict but platitudinous consensus.

EXECUTIVE PRIVILEGE

No aspect of the PSAC has received as much criticism as the sheltering of its deliberations and recommendations under executive privilege. Other scientific advisory committees enjoy varying degrees of privilege, but never to the extent of those operating directly under the aegis of the White House. Of course, this is only a part of a more general source of irritation between the executive and legislative branches. The irritation has been directed at the PSAC only because its advice has frequently been followed, and has sometimes been against important agencies or congressional positions and projects. In practice, Congress, and for that matter, the public, have seldom been denied access to the technical and nontechnical considerations on which the PSAC advice was based; only questions of who said what are withheld. In some instances the PSAC panels have actually been reconstituted as panels of other agencies in order that their views could be made public without revealing the advice precisely as it was given to the President. In the early days of the PSAC, the Special Assistant, Dr. Killian, frequently advised the heads of agencies about the recommendations he intended to make to the President, and this has remained a common practice.[12] The decision to drop the ANP project, for example, was actually concurred in by the Director of Defense Research and Engineering and a panel of the General Advisory Committee of the AEC. Most of the technical and military considerations involved in that decision were covered in testimony before Congress by Dr. Herbert York and others.

There is a general feeling in the scientific community that J. Robert Oppenheimer was persecuted because of the unpopularity of his advice, supposedly given in private. The protection of executive privilege is sometimes necessary to induce scientists to join panels. They feel, rightly or wrongly, that their private interests may be vulnerable to reprisal by Congress or by powerful agencies which may be adversely affected by their recommendations.

A different type of problem has arisen when scientific advisers have chosen to speak out publicly on issues with which they have also been concerned as scientific advisers under the mantle of executive privilege. People opposed to their views feel that advisers who take public stands on controversial issues are trying to have their cake and eat it too. These advisers have, however, been willing and even eager to testify before Congress, with the exception of specifics like who recommended what to the President. The suspicion may remain that their testimony is filtered or distorted by the omission of privileged matter, but it is hard to see how this problem is any different for a presidential adviser than for an agency or department head who also gives privileged advice to the President. Perhaps the principal difference is that the part-time adviser does not consider himself under political or administrative discipline, and so may feel more free to volunteer a public statement at variance with the official line of the moment. The administrator who differs from the official line is constrained from expressing his difference except when called upon to do so while testifying under oath.

---

[12] See essay by Gilpin in this volume.

This is a complex question, but its facets are not peculiar to scientific advisers, except insofar as scientists may enjoy greater public prestige than other experts. Because of their relative newness in the higher government councils, the reputation of scientists is less tarnished by special pleading or self-serving, and it is hoped that in their public statements scientific advisers will bear in mind their responsibility to preserve the reputation for objectivity which scientists generally enjoy, and which is their greatest asset in the political arena.

Speaking out on public issues should not in itself be considered an abuse of executive privilege. Under the Eisenhower administration, the Special Assistant often felt hampered by the rigidity of the practice of executive privilege, which is even more enforced when the Executive and Congress are controlled by different parties. There were times when the Special Assistant was unable to testify although it would have been to the interest of the government for him to do so. Reorganization Plan No. 2, which became effective in June, 1962, created the Office of Science and Technology and gave the Special Assistant two hats—one as a confidential White House adviser and the other as statutory Director of the Office, subject to Senate confirmation. One of the purposes of providing such statutory underpinning to the science advisory role was to permit the Director to testify before Congress and thereby formally defend administration positions on new science legislation, on budgetary matters affecting basic science, and on the coordination of federal scientific programs. As a result of this reorganization, the area which we have called "policy for science" can become the subject for congressional testimony, while the area which we have called "science in policy" remained under executive privilege.

In the reorganization, the PSAC remained in the White House and continued to enjoy the protection of executive privilege. It is possible that if all the PSAC members had been made subject to Senate confirmation, this might ultimately have led to political control of science and to a serious threat to the independent and apolitical nature of the Committee. This nonpartisan character was explicit in the PSAC's original charter from President Eisenhower, and was recognized by President Kennedy through his continuation of the membership after the change in administration. Offsetting the fear of partisan control is the historical fact that there has so far been no problem of partisan politics with respect to the National Science Board, whose membership is subject to Senate confirmation.

A possibly more serious problem resulting from the new status of the Special Assistant is the amount of time involved in congressional relations that must now be added to that required for all the other responsibilities of the office. A major part of the past effectiveness of the whole presidential science advisory operation has been due to its compactness and lack of bureaucratization. The Special Assistant and the PSAC were able to be highly selective in the problems undertaken for study. With greater congressional visibility the Special Assistant may find himself forced to know less and less about more and more, and to depend increasingly on staff work rather than first-hand study for his expression of views. Since he has no decision-making or executive responsibility, other than for his own small staff, he can, I believe, avoid this dilemma by proper selectivity with respect to the type of things on which he chooses to testify. The Director of the Bureau of the Budget is in an analogous situation, and has so far lived with it successfully by delimiting his areas of testimony.

The preservation of executive privilege is clearly essential if the President is to get honest and independent advice. Too frequently the violently unpopular position of today becomes government policy tomorrow. If the adviser could not look ahead without fear of political crucifixion, policy would soon become frozen in a static mold. Furthermore, it would be ossified at the very moment of generation, when it should be most fluid and dynamic.

CONFLICT OF INTEREST

Nearly two-thirds of all the nation's research and development is financed by the federal government, and 55 percent of the work is carried out through contracts and grants from the government to the private sector. Much of this work is of a nature that is unique to government financing. It is usually impossible to recruit advisers in these fields unless they are closely associated with institutions whose work is heavily financed by the federal government. In its modern guise, the problem of conflict of interest is much more subtle and complicated than envisioned in the conflict of interest statutes, which were designed originally to prevent government officials from directly and personally benefiting from their official position.

Until recently the law was not very clear on the extent to which it was applicable to part-time advisers. By a strict interpretation, most of the complex advisory structure in the federal agencies would have been made illegal. The best that could be done from a practical standpoint was to avoid situations in which government consultants were in positions to affect directly a specific contract or grant with an institution in which they had a substantial interest, or to which they owed an allegiance. Many government consultants, and especially scientific advisers, have acted on the assumption that the statutes applied only to employees or stockholders of profit-making institutions, arguing that as long as a government contract could not affect their personal compensation or financial status there was no possibility of conflict or even impropriety. Few advisers were more than vaguely aware of the conflict of interest statutes; they could be excused for not taking them very seriously since there had been no prosecution under these statutes against a part-time adviser, and therefore the law had never been tested or interpreted in the courts. Most scientists feel that the best protection for the government lies in full and public disclosure of all the outside interests of the advisers together with sufficient technical competence and general sophistication on the part of the administrator receiving the advice to enable him to make allowance for possible bias and discount it in his own mind. In many instances the loyalties of a scientist or engineer are much more closely identified with his professional community than with the institution which pays his salary. Most scientific advisers would assert their ability to be objective even when the interests of their own institution are involved, though they may recognize the undesirability from the standpoint of public relations of having a potential conflict of interest.

Recent legislation and legal interpretation have clarified the concept of conflict of interest, especially as it pertains to part-time consultants. While this legislation does not fully embody the principles enumerated above, it does permit the government to retain the services of its advisers, and to operate with advisory boards and panels much as in the past, though with greater circumspection as to the public record.

No government adviser can be free of bias. Although no statute recognizes the possibility of a conflict of interest within government, every administrator is well aware of the agency biases harbored by federal employees who give scientific advice. The scientists of the three military services tend to favor strategies and weapons systems which lead to the aggrandizement of their service, and the professional military scientist can seldom be depended on for unbiased advice on disarmament.

In fact it is often to counteract the effects of conflicts of interest within government that administrators have sought advice from the private scientific community. This community, in turn, has its own bias toward contracting out as much research as possible rather than doing it within government. Government scientists generally favor government laboratories and are opposed to the practice of contracting out either research or development. University scientists have a bias favoring basic research. These biases are seldom consciously self-serving; they are merely a part of human nature. "What's good for General Motors is good for the country" is not a sentiment unique to a famous Secretary of Defense; there is a little bit of it in every institution and profession. Some advisers are more able than others to recognize this sentiment in themselves and consciously strive to discount it in formulating their advice. These are generally the people that make good advisers. There is also an opposite danger of leaning over backwards, of failing to defend the interests and values of science because of a fear of the appearance of pleading the cause of a special interest.

Issues of the kind described above are much more important in scientific advising than are the obvious conflicts of interest involving personal profit. Yet it is basically impossible to resolve such issues by any legislative prescription. As long as the federal government retains the necessary competence within its own full-time administrative and technical staff, it has less to fear from advice subtly or overtly biased by self-interest than it does from loss of the benefit of a perspective from outside government. The answer to the conflict of interest problem in scientific advice is not restrictive and negative legislation but positive legislation and administrative action to improve working conditions within government so that it can continue to attract people capable of properly using outside scientific advice. With respect to scientific advisers acting at the higher policy levels, it is important to develop a professional code of ethics connected with the advisory function. Fortunately, among basic scientists there is a stern ethical code associated with the question of scientific credit and priority which is powerfully sanctioned by public opinion within the scientific community. It provides a model for a similarly sanctioned, though unwritten, code with respect to the objectivity of scientific advice.

CONCLUSION

The reader may conclude that this essay has presented an unduly rosy picture of the beneficial effect of scientific advisers in government, and particularly of the operation and influence of the PSAC. There are certainly many critics, especially among those who have disagreed with the influence exerted by the PSAC in the field of disarmament and weapons policy, who would emphatically and sincerely disagree concerning the beneficial effects. Only history will tell who was right with respect to policy.

A major criticism would be of the tendency toward self-perpetuation among the most influential committees, and especially the consequent preservation through two administrations of a single viewpoint on many questions of policy. Furthermore, critics would argue, the presidential advisers have, through their influence on major appointments, gradually imposed their policy viewpoints throughout the upper levels of the scientific agencies and in the Bureau of the Budget. Although there may be superficial evidence for such an analysis, it will scarcely stand historical examination. The diversity of viewpoints and institutions represented on high-level advisory committees has been much broader than the critics claim. If it can be said that any policy viewpoints have become dominant in government, this has been imposed more by the logic of events than by any particular group of advisers. The advisers merely foreshadowed what would probably have been brought about by events anyway: the creation of an invulnerable retaliatory force; the inadequacy of the policy of massive retaliation; the importance of limited and guerrilla warfare and of conventional arms; the creation of the NASA, the Arms Control and Disarmament Agency, and a research and development section in the foreign-aid program; the centralization of research and development responsibility in the Department of Defense; increased support for basic research and graduate education; the fostering of international scientific activities and of scientific exchanges with the Soviet bloc; the creation of the FCST and its interagency committees; and the creation of the Office of Science and Technology by reorganization plan.

In matters of armament policy the critics have had their day in court and indeed have spoken from a much more firm base of power and influence than the various scientific advisers. Perhaps where the advisers have won out they have done so because their case was more persuasive, rather than because of illegitimate pressures or some sort of inside track to policy.

The scientific community and its influence in government is in a politically exposed position if only because of the magnitude of the scientific enterprise and the success of scientists, especially university scientists, in influencing policy despite their lack of a true constituency or base of power. To an increasing degree the country is looking to the Special Assistant and his supporting advisory staff, and, rightly or wrongly, there will be a growing tendency for Congress to blame his office for everything that goes wrong in the manifold scientific activities of the government. Whether the office can maintain its ability to concentrate on the key issues and not become lost in a rash of bureaucratic brush fires is the crucial question for its future success. It seems probable that the test of the political viability of the present network of advisory committees, and particularly of the presidential advisory structure, will come during the next few years, when budgetary ceilings may put a greater strain than ever before on the advisory committee's task of judging, and when the accountability of scientists to Congress and to the public will be more carefully probed than it has been in the past.

The AMERICAN PEOPLE and SCIENCE POLICY

The role of public attitudes in the policy process

JON D. MILLER
Northern Illinois University

Pergamon Press
New York Oxford Toronto Sydney Paris Frankfurt

Chapter 4

Public Participation in Science Policy

The preceding chapter outlined a stratified model of public participation in low-salience issues, set that model in the context of a broader political-specialization process, and suggested conditions under which various types of public participation in the policy process would tend to occur. It is now appropriate to turn to an examination of the membership of each of the strata in the stratified model as applied to the formulation of science policy.

SCIENCE POLICY DECISION MAKERS

In the stratified model, the decision makers are those persons or groups of persons who have the power to make binding decisions on policy matters in any given area. The principal decision makers relevant to the formulation of science policy include the leadership of both houses of the Congress and the president and the major officers of the executive branch concerned primarily with matters relating to science policy. In rare instances, federal judges and legislative, executive, and judicial officers at the state and local levels can become decision makers. It is a small group, fewer than 100 persons, but one that has grown significantly since the end of the Second World War.

In the American system of government, the separation of powers means that there is not a single hierarchical set of decision makers. It is possible, if not likely, that there will be differences in the policy preferences of the relevant science policy decision makers on any given issue. These differences usually are resolved through normal negotiation processes, often with the active assistance of key nongovernmental policy leaders.

Most of the decision makers have responsibilities broader than science policy. One of the tasks of nongovernmental policy leaders is to get decision makers at the higher levels to focus on science policy issues. The discussion in recent years over the role of the science advisor to the president may be viewed as a dispute over the level of access that the scientific community should have to the president. The establishment of the Office of Science and Technology (OST) by President Kennedy was a reflection of his desire to have direct and regular communication with the scientific community, and the selection of Jerome Weisner to head the OST symbolized an open channel of communication. In contrast, the abolition of the OST by President Nixon reflected his desire for a more formal and distant relationship with the scientific community. To reestablish the office and prevent its abolition in the future, the Congress gave statutory status to an Office of Science and Technology Policy (OSTP) in 1979. By selecting George Keyworth, a scientist who had not been a principal in the science policy leadership group, President Reagan has attempted to utilize the position as a communication filter.

The Congress, on the other hand, has generally recognized the growing volume and importance of science policy matters and adapted its structure accordingly. The House of Representatives has a permanent Committee on Science and Technology with a substantial subcommittee structure and adequate staffing. The Senate has continued to rely primarily on two major subcommittees but has provided specialized technical staff to assist in the review of science policy matters. In addition, the Congress has established an Office of Technology Assessment (OTA) that studies science and technology policy issues as assigned by a congressional oversight committee. With a professional staff of over 200, the OTA provides a counterbalance to the technical expertise available within executive branch agencies and gives the Congress an independent assessment of executive branch proposals.

In summary, the decision makers for science policy are a relatively small group of legislative and executive officers who can, individually in some cases, but usually collectively, make binding decisions concerning science policy matters. In some cases, the decision makers may include federal judicial officers or legislative, executive, or judicial officers from state and local governments; but the instances of decision making at those levels are not frequent. In general, the Congress has recognized the importance of science policy and provided committee and staff structures to facilitate the decision-making process. The place of science policy in the executive branch has varied from administration to administration.

SCIENCE POLICY LEADERS

Nongovernment science policy leaders play an essential role in the formulation of public policy relevant to science and technology. In contrast to the decision makers, most science policy leaders focus almost exclusively on science and technology matters. There are about 3,000 individuals in the nongovernmental leadership group for science policy. To a great extent, the science policy agenda is formulated by this leadership group. The presentation of policy objectives to the decision makers and any subsequent negotiations are conducted by the policy leaders. As noted above, when there is consensus among the policy leaders themselves and between them and decision makers, policy is established without wider public involvement.

Who are these nongovernmental leaders of science policy? What are their backgrounds? How do they exercise this leadership role?

A 1981 survey of science policy leaders[5] indicated that the leadership group was broadly representative of the major disciplines and professions within the scientific community, predominantly from universities but with significant representation from both the for-profit sector and from independent research centers, and predominantly male with a modal age in the early 50s (see table 3). The distribution of disciplines and professions across sectors was relatively stable, with a slightly higher proportion of engineers and other professionals and a slightly lower number of social scientists in the for-profit sector. The predominance of males in the group was most pronounced in the for-profit sector. The distribution of leaders by age and region of residence was relatively uniform across all sectors.

The policy leaders have a strong organizational base within the scientific

TABLE 3
DEMOGRAPHIC PROFILE OF LEADERS OF SCIENCE AND TECHNOLOGY: 1981

	Total	For Profit	University	Other Nonprofit
Age				
Under 45	27%	21%	30%	23%
45–54	33	40	39	38
55–64	27	31	22	30
65 and over	14	10	19	9
Gender				
Female	12	3	14	13
Male	88	97	86	87
Discipline				
Biological Science	15	13	17	15
Physical Science	25	21	27	26
Social Science	20	9	26	15
Engineering-Prof	20	30	16	23
Other	20	27	13	21
Region				
Northeast	28	32	32	16
North Central	19	22	21	10
South	29	25	20	54
West	24	20	27	19
N =	281	59	147	67
% =	100	21	52	24

---

[5] A complete description of the technical characteristics of the 1981 survey of science policy leaders is provided in the Appendix.

community. Almost half were members of an editorial board of a scientific or engineering journal (see table 4). Membership on the editorial boards of journals was most frequent among leaders from universities and relatively rare among leaders from the for-profit sector. Approximately a quarter were members of a governing board of an independent laboratory or research center, and about the same number were members of a board of directors of a for-profit corporation involved primarily in scientific or engineering activities. Sixteen percent were members of a governing board of a private foundation engaged in supporting scientific research or development, while only 11 percent were members of a board of trustees of a college or university.

The 1981 data indicated extensive leader participation in national professional and disciplinary societies, with 42 percent reporting that they were, or had been, a national officer of a scientific or disciplinary society. This experience was significantly less frequent among leaders from the for-profit sector and significantly more frequent among leaders with backgrounds in the physical sciences (see table 5). Slightly over half of the leaders had chaired national committees of their professional or disciplinary societies, and almost 70 percent

TABLE 4
PARTICIPATION IN THE GOVERNANCE OF SCIENTIFIC INSTITUTIONS: 1981

	Percentage Who Are a Member of . . .					
	A	B	C	D	E	N
Total	48%	27%	27%	16%	11%	281
Sector						
For-profit	17	24	41	17	20	59
University	60	27	23	14	8	146
Nonprofit	46	30	24	19	12	67
Discipline						
Biological Sci.	64	19	19	12	5	42
Physical Sci.	57	31	22	15	77	69
Social Sci.	50	32	24	9	9	54
Engineering-Prof	36	23	43	25	21	56
Other	37	24	26	17	13	54
Region						
Northeast	49	38	26	19	14	77
North Central	50	18	26	16	14	51
South	39	27	28	19	13	80
West	57	20	26	9	5	65

A = an editorial board of a scientific or technological journal
B = the governing board of any independent center or laboratory that engages in scientific research or development
C = a board of directors of any corporation involved in science or technology
D = the board of directors of any foundation that provides funds for scientific research and development
E = a board of trustees of a college or university

TABLE 5
LEADERSHIP IN PROFESSIONAL OR DISCIPLINARY SOCIETY: 1981

	Percentage Who Have Served as . . .			
	National Officer	Committee Chair	Committee Member	N
Total	38%	49%	63%	281
Sector				
For-profit	17	39	51	59
University	43	54	68	147
Nonprofit	45	49	66	67
Discipline				
Biological Sci.	48	60	79	42
Physical Sci.	46	60	74	70
Social Sci.	30	43	52	54
Engineering-Prof	36	46	61	56
Other	31	39	48	54
Region				
Northeast	28	35	54	80
North Central	37	63	77	51
South	46	51	60	85
West	40	54	66	65

had served as a member of one or more national committees for a professional or disciplinary group.

Sixty percent of the leaders were members of the American Association for the Advancement of Science (AAAS). Fourteen percent were members of the National Academy of Sciences, 11 percent were members of the American Academy of Arts and Sciences, and 6 percent were members of the National Academy of Engineering (see table 6). Leaders affiliated with for-profit organizations were slightly less likely to be members of these invitational leadership societies.

Working from these organizational bases, the leaders reported a wide array of efforts to influence both colleagues within the scientific community and persons in the general political community on science policy matters. Approximately two-thirds of the leaders had attempted to influence the policy views of their colleagues in their own discipline or profession, and about 60 percent had sought to influence colleagues from other disciplines and professions (see table 7). Approximately half of the leaders reported seeking to influence persons outside the scientific community who were interested in, and informed about, science and technology matters: the attentive public for science policy. One in five of the leaders had attempted to influence the attitudes of the general public on science and technology issues, with leaders from the university sector being the least likely to have attempted to influence the general public.

The development of consensus on science policy matters requires frequent

TABLE 6
MEMBERSHIP IN NATIONAL LEADERSHIP ORGANIZATIONS: 1981

	Percentage Who Are Members of . . .				
	AAAS	National Academy of Sciences	National Academy of Engineering	American Academy of Arts and Sciences	N
Total	60%	14%	6%	11%	281
Sector					
For-profit	34	10	5	5	59
University	71	16	6	15	147
Nonprofit	60	13	6	6	67
Discipline					
Biological Sci.	79	17	2	10	42
Physical Sci.	73	17	7	13	70
Social Sci.	67	6	0	13	54
Engineering-Prof	48	21	14	9	56
Other	39	11	2	9	54
Region					
Northeast	60	13	4	15	80
North Central	59	10	6	10	51
South	58	13	6	5	85
West	65	22	8	14	65

TABLE 7
EFFORTS TO INFLUENCE PUBLIC ATTITUDES ON SCIENCE POLICY: 1981

	Percentage Seeking to Influence Persons from . . .				
	Own Discipline	Other Discipline	Attentive Public	General Public	N
Total	65%	59%	51%	22%	281
Sector					
For-profit	61	55	48	22	59
University	64	60	50	17	147
Nonprofit	79	69	61	36	67
Discipline					
Biological Sci.	69	62	64	21	42
Physical Sci.	71	67	49	19	70
Social Sci.	61	59	41	20	54
Engineering-Prof	64	59	57	23	56
Other	61	50	50	26	54
Region					
Northeast	66	60	54	29	80
North Central	59	65	49	20	51
South	67	60	53	25	85
West	66	54	46	12	65

TABLE 8
EFFORTS TO INFLUENCE OTHER LEADERS ON SCIENCE POLICY: 1981

	Percentage Seeking to Influence Leaders from . . .			
	Professional-Disciplinary Societies	State & Local Gov't	Church and/or Community	N
Total	50%	49%	20%	281
Sector				
For-profit	44	48	12	59
University	50	50	18	147
Nonprofit	60	54	33	67
Discipline				
Biological Sci.	50	57	14	42
Physical Sci.	51	56	17	70
Social Sci.	44	39	22	54
Engineering-Prof	57	54	21	56
Other	46	43	20	54
Region				
Northeast	49	44	20	80
North Central	41	49	14	51
South	54	49	29	85
West	52	55	11	65

discussions both within the leadership group and with external political and community leaders. Half of the leaders reported that they had attempted to influence other leaders of scientific or professional societies with their own policy views (see table 8). The same proportion reported seeking to influence state or local governmental leaders on a science policy matter, but only 20 percent indicated that they had made a similar effort to influence the views of community leaders.

In summary, the nongovernmental leaders of science policy reflected a broad spectrum of disciplines and professions, were predominantly university based, and were predominantly male with a modal age in the early 50s. The leaders reported high levels of governance and leadership activities both within the scientific community and in regard to the broader political system. Appropriate to their leadership role, this group indicated a high level of persuasive activity.

THE ATTENTIVE PUBLIC FOR SCIENCE POLICY

Those citizens who are sufficiently interested in a given policy area to become that issue area. When the policy leaders of science and technology make an appeal to "the public" for support in regard to a particular policy dispute, it is usually the attentive public to which the appeal actually is addressed. Although the attentive public includes only a segment of the total public, for matters of science policy it is the *de facto* public and the democratic check on policy leaders and decision makers.

In Almond's (1950) original conceptualization, inclusion in an attentive public required: (1) a high level of interest in an issue or area, (2) a functional level of knowledge about that issue or area, and (3) a pattern of regular information acquisition relevant to the issue or area. In the 1979 survey, interest was measured by a combination of direct inquiry (e.g., How interested are you in this issue?) and a set of hypothetical headlines concerning a wide range of issues. A factor analysis of the reported headline reading preferences produced a clear factor structure that allowed the identification of a set of headlines associated with an interest in science policy matters. These items were used to construct an index of interest, that was very strongly and positively associated with the responses to the direct inquiry measure of interest. Since both measures were strongly associated, the 1981 survey carried forward only the two direct inquiry items and used those items to measure interest.[6] In both 1979 and 1981, slightly over a third of the adult population expressed a high level of interest in issues concerning new scientific discoveries and issues concerning the use of new inventions and technologies (see table 9).

Women and men did not differ significantly in regard to their interest in "new scientific discoveries," but a significantly higher proportion of men expressed a high level of interest in "the use of new inventions and technologies." It would appear that women were not less likely to be interested in science, but rather were less interested in more applied and technological issues.

In both years, there was a significant and positive association between the level of formal education completed and the level of interest in both science and technology. This association was the strongest of all of the independent variables studied.

The definition of a functional level of knowledge about science and technology involved a two-step procedure. In both years, respondents were asked to rate themselves as very well informed, moderately well informed, or not very well informed on each of the same nine areas used to assess interest. Those respondents who reported that they were either very well or moderately well informed concerning either new scientific discoveries or the use of new inventions and technologies were classified as perceiving themselves to be functionally

---

[6] In the context of the interview, each respondent was asked to indicate whether he or she was very interested, somewhat interested, or not very interested in each of nine issues or areas. One of the issues concerned "new scientific discoveries" and a second involved the "use of new inventions and technologies."

TABLE 9
INTEREST IN SCIENCE AND TECHNOLOGY ISSUES

	Percentage "Very Interested" in Issues Concerning . . .					
	New Scientific Discoveries		Use of New Inventions and Technologies		N	
	1979	1981	1979	1981	1979	1981
Total	36%	38%	34%	34%	1635	3195
Age						
17–24	38	37	37	35	309	570
25–34	42	45	39	35	360	747
35–44	32	40	29	33	257	528
45–54	38	38	35	36	229	505
55–64	37	40	28	39	225	354
65 and over	27	28	30	25	241	488
Gender						
Female	35	36	27	28	854	1677
Male	37	41	41	40	768	1514
Education						
Less than high school	22	24	19	26	455	632
High school	39	38	36	33	931	1912
Baccalaureate	54	55	45	44	146	456
Graduate degree	53	48	58	42	92	192

knowledgeable.[7] The surveys indicated that about 60 percent of the adult population of the United States perceived themselves to be at least moderately well informed about science, technology, or both (see table 10).

Women were less likely than men to see themselves as knowledgeable about science and technology. As with interest, there was a strong and positive association between the level of formal education completed and the perceived level of knowledge about science and technology.

The first step in the definition of attentiveness to science and technology policy required that a respondent report a high level of interest in, and at least a moderate level of knowledge about, either the science or the technology area. In both 1979 and 1981, approximately 39 percent of the adult population met this minimum standard (see table 11).

The second step in the definition of attentiveness concerned a somewhat more rigorous test of knowledge about science and technology. In both years, respondents were asked to define the meaning of a "scientific study," and were

---

[7] The moderately well informed group was included since both surveys incorporated a battery of substantive information items which were used for a final determination of knowledgeability. In other studies where a substantive knowledge battery was not available, attentiveness has been calculated based on a high level of interest and a self-reported very well informed.

TABLE 10
PERCEIVED KNOWLEDGE ABOUT SCIENCE AND TECHNOLOGY ISSUES

	Percentage "Very Well" or "Moderately Well" Informed About Issues Concerning . . .					
	New Scientific Discoveries		Use of New Inventions and Technologies		N	
	1979	1981	1979	1981	1979	1981
Total	63%	63%	60%	61%	1635	3195
Age						
17–24	66	63	62	63	309	571
25–34	69	69	66	63	361	747
35–44	63	62	57	61	257	528
45–54	62	64	58	60	231	503
55–64	63	63	63	62	224	354
65 and over	50	57	53	54	239	488
Gender						
Female	59	58	54	54	853	1677
Male	68	69	67	68	768	1514
Education						
Less than high school	43	50	44	49	455	631
High school	67	63	64	60	929	1911
Baccalaureate	82	77	77	75	143	456
Graduate degree	87	80	82	69	92	192

TABLE 11
IDENTIFICATION OF THE ATTENTIVE AND POTENTIALLY ATTENTIVE PUBLICS

1979		1981
39.6%	Percentage reporting a high interest in science and at least a moderate level of science knowledge	39.3%
– 18.8	Minus persons who scored less than two on the Index of Scientific Knowledge	– 17.6
– 1.3	Minus persons who did not report a pattern of regular news acquisition	– 1.7
19.5	Attentive public for science policy	20.0
20.1	The interested public is a residual group with high interest but that did not pass the knowledge or information acquisition tests.	19.3

asked to rate their comprehension of three terms: radiation, DNA, and GNP.[8] An Index of Scientific Knowledge was constructed, scoring two points for a correct understanding of scientific study and one point each for a clear understanding of radiation, DNA, and GNP. Those respondents with a score of zero or one on the index were deleted from the attentiveness group (see table 11). This step resulted in a deletion of about 18 percent of the population from the potentially attentive group in both years.

The third and final step in the definition of attentiveness to science and technology policy was the certification of a regular pattern of relevant information acquisition. To be classified as attentive, it was necessary for a respondent to (1) read a newspaper daily or "almost every day," (2) read one or more news magazines "most of the time," (3) read one or more science magazines "most of the time," or (4) watch a television science show like *Nova* "most of the time." Respondents who failed to do any of the information acquisition activities were dropped from the attentive group, leaving an estimated 20 percent of the public attentive to science and technology policy in both years (see table 11).[9]

In the process of defining the attentive public for science and technology policy, a second relatively important residual group was identified, which will be referred to as the interested public for science policy. This group has a high level of interest in science and technology matters and perceives itself to be at least moderately well informed about science, technology, or both. The members of this group, however, were unable to demonstrate adequate substantive information to remain in the attentiveness classification. In both 1979 and 1981, about 20 percent of the adult population of the United States were members of the interested public for science policy (see table 11).

A comparison of the demographic distribution of the attentive public, the interested public, and the nonattentive public in 1979 and 1981 indicated that there was a very high degree of stability in the estimates across the two years. Because of the larger sample, the estimates from the 1981 survey should be taken as the more accurate (see table 12).

---

[8] In the interview, each respondent was asked to indicate whether he or she had a clear understanding of what it means to study something scientifically, a general sense of the meaning of the term, or little understanding of it. In both years, those respondents who asserted that they had a clear understanding of the term were asked to tell the interviewer what they thought it meant, "in your own words." The responses were recorded verbatim and were coded independently by two coders into a six-tier classification that ranged from an understanding of theory building and hypothesis testing to no understanding at all. For a more complete description of the coding procedures, see Miller (1982b).

[9] It should be noted that a previous technical report on the 1979 survey (Miller, Prewitt, and Pearson, 1980) utilized the more comprehensive 60-item definition and estimated that approximately 18 percent of the adult population was attentive to science and technology policy. To foster the maximum level of consistency for this analysis, the attentiveness rate for 1979 was reestimated using the new procedures and the revised estimate was 19.5 percent. The difference between the two estimates is not significant at the .05 level.

TABLE 12
A DEMOGRAPHIC PROFILE OF THE ATTENTIVE PUBLIC FOR SCIENCE POLICY

	Attentive Public		Interested Public		Nonattentive Public	
	1979	1981	1979	1981	1979	1981
Age						
17–24	24%	18%	17%	19%	18%	18%
25–34	32	33	21	18	19	22
35–44	12	16	15	15	17	17
45–54	12	15	17	17	14	16
55–64	13	10	14	15	14	10
65 and over	7	8	17	16	17	18
Gender						
Female	35	40	55	54	58	57
Male	65	60	45	46	42	43
Education						
Less than high school	7	8	26	19	36	24
High school	59	48	63	69	55	61
Baccalaureate	19	32	8	9	6	10
Graduate degree	16	12	4	3	3	5
College science courses						
None	46	NA	79	NA	82	NA
Some	54	NA	21	NA	18	NA
N =	319	637	328	617	988	1940

To assess the influence of education, age, gender, and exposure to a college-level science course, a multivariate logit analysis was performed on the 1979 data, using the methods described by Goodman (1978) and Fienberg (1980). Since this is the first logit utilized in this analysis, it may be useful to review the procedure. A logit analysis is analogous to a regressional analysis, with a dependent variable and two or more independent variables. For this analysis, attentiveness to science policy was treated as a dichotomous dependent variable, although the logit methodology could have utilized a polychotomous dependent variable. The level of formal education was dichotomized into those with and without a baccalaureate degree. The college science course variable was dichotomized into those persons who had completed one or more college-level science courses and those who had not. Age was grouped into three categories (17 to 34, 35 to 54, and 55 and over) and entered into the analysis as a trichotomous variable.

An examination of the distribution of the dependent variable within each combination of independent variables is useful in understanding the structure of multivariate cross-classification analysis. In regard to attentiveness to science policy, the multivariate distribution indicated that courses and gender were associated with attentiveness within all of the strata of the multivariate distribution

TABLE 13
MULTIVARIATE DISTRIBUTION OF ATTENTIVENESS: 1979

Education	Age	Gender	Science Courses	Attentive to Science Policy	N
No college	17–34	male	none	18%	199
			some	63	75
		female	none	14	228
			some	23	61
	35–54	male	none	12	171
			some	40	20
		female	none	7	190
			some	13	30
	55 and over	male	none	15	158
			some	—	12
		female	none	3	229
			some	—	19
College	17–34	male	none	—	7
			some	52	46
		female	none	—	6
			some	46	46
	35–54	male	none	—	7
			some	49	45
		female	none	—	4
			some	25	24
	55 and over	male	none	—	5
			some	69	26
		female	none	—	6
			some	—	16

(see table 13). By systematically comparing male and female respondents with similar characteristics, it is possible to determine that a higher proportion of males were attentive to science policy within each stratum, but this cell-by-cell procedure does not produce a single estimate of the relative influence of gender on the likelihood of attentiveness.

Whereas a traditional regression model seeks to place individual scores in a space defined by the dependent and independent variables, logit analysis estimates an expected frequency for each cell based on the marginal distributions of the variables, controlling for all relationships among the independent variables. This first estimate is referred to as the independence model since it would fit the data perfectly if there were no relationships between any of the independent variables and the dependent variable. The difference between the estimated cell frequencies and the actual data distribution is measured in terms of likelihood-ratio chi-squares[10] and, for this specific analysis, was 272.01 (see table 14). This sum is a measure of the total level of association between the independent variables and the dependent variable, and it is referred to as the total mutual dependence in the model. Goodman (1978) suggests that mutual dependence is analogous to variance in a traditional regression model, but that mutual dependence is a more appropriate term since it reflects the cell basis of the analysis.

In this specific analysis, a stepwise logit procedure was utilized. The first step added the direct effect of exposure to college-level science courses on attentiveness to the model, and the procedure produced a new set of estimated cell frequencies, utilizing the additional marginals inserted into the model. The resulting fit indicated that 125.42 chi-squares remained unexplained, but that 146.59 chi-squares of mutual dependence were accounted for by exposure to a college-level science course. The coefficient of multiple-partial determination (CMPD) is analogous to a multiple R^2 in a traditional regression, and indicated that approximately 54 percent of the total mutual dependence in the model was accounted for by the college-level science course variable (see table 14).

Following the stepwise procedure, the education variable was then added to the model. The resulting estimates indicated that the effect of a baccalaureate accounted for an additional 8 percent of the total mutual dependence. Substantively, this result meant that holding constant the level of exposure to college science courses, there was a residual effect of completing a baccalaureate on the likelihood of being attentive to science policy. This "college effect" has

TABLE 14

A LOGIT MODEL TO PREDICT ATTENTIVENESS TO SCIENCE POLICY: 1979

Models		df	LRX^2	CMPD
H1	CEAG.Y	23	272.01	—
H2	CEAG.YC	22	125.42	.539
	Difference due to YC	1	146.59	.539
H3	CEAG.YC.YE	21	103.33	.620
	Difference due to YE	1	22.099	.081
H4	CEAG.YC.YE.YA	19	81.42	.701
	Difference due to YA	2	21.91	.081
H5	CEAG.YC.YE.YA.YG	18	42.61	.843
	Difference due to YG	1	38.81	.143

Y = attentiveness
C = college science courses
E = education
A = age
G = gender

df = degrees of freedom
LRX^2 = likelihood ratio chi-square
CMPD = coefficient of multiple-partial determination

---

[10] Goodman (1978) recommends the use of likelihood-ratio chi-squares because they are based on a geometric mean and are thus additive across the entire model. In contrast, Pearson's chi-square

been noted by Newcomb (1976), Astin (1977), and others; but these data suggest that its magnitude is only about one-seventh as large as the impact of a college science course. The cumulative CMPD indicated that approximately 62 percent of the total mutual dependence in the model was accounted for by the combined effects of a college-level science course and the completion of a baccalaureate (see table 14).

Next, the age variable was added to the model and the results indicated that another 8 percent of the total mutual dependence was attributable to age. Substantively, this result means that there was a significant residual age effect on the likelihood of attentiveness to science policy, holding constant differences in formal education and exposure to a college-level science course. The influence of the age variable points to a period effect. Persons who matured to adulthood in the scientific environment of the post-1945 years were more likely to be attentive to science than persons socialized in earlier, less scientific, periods.

Finally, gender was added to the model and the resulting estimates indicated that there was a significant residual gender effect on attentiveness, even after taking education, science courses, and age into account. The logit model indicated that the residual effect of gender accounted for about 14 percent of the total mutual dependence in the model (see table 14). This result suggests that there has been significant sex-role socialization in American society that identified science and technology as male domains and that this socialization persisted even when education, science course exposure, and age were all held constant. In relative terms, the magnitude of this residual gender effect was larger than either the residual effect of age or education, suggesting that the impact of early sex-role socialization has been substantial.

The combination of all four independent variables in the logit model is called the "main-effects model," and the combined effects of the four independent variables accounted for 84 percent of the total mutual dependence. This was a very good fit.

In summary, an attentive public for science policy has been defined empirically and includes one in five American adults. Another 20 percent of the adult population were classified as "interested" in science policy matters, but were deficient in substantive information about science and technology. The remaining 60 percent of the population has little interest in, and a relatively low level of information about, science policy issues. Exposure to a college science course and the completion of a baccalaureate program were positively associated with attentiveness, as were youth (under age 35) and the male gender.

THE NONATTENTIVE PUBLIC

At the base of the policy pyramid is the nonattentive public, which has little interest in, or knowledge about, science policy. While some of these individuals agricultural policy, foreign policy, economic policy, local politics, or other current issues. Although the nonattentive public was somewhat older and less likely to hold a college degree than the attentive public for science policy, its demographic profile was not significantly different than that of the interested public for science policy in either 1979 or 1981 (see table 12).

Under normal circumstances, the nonattentive public would have no involvement in the formulation of science policy. When decision makers or policy leaders seek broader public support for a particular policy outcome, it is usually the attentive public to which their appeals are directed. The normative standards of the science policy leadership group would view appeals to persons who were uninterested in and, more importantly, unknowledgeable about science and technology as inappropriate leadership behavior.

There are, however, two instances in which the nonattentive public may have a significant impact on the formulation of science policy. First, when science policy questions are included in a formal referendum, the balance of power swings to the nonattentive public. Referenda on issues like nuclear power, fluoridation, and laetrile illustrate the potential involvement of the nonattentive public in science policy matters. Second, if a science policy matter were to become an electoral issue on which candidates for public offices were to take opposite positions and to campaign on these differences, then the balance of power would again swing to the nonattentive public, which comprises the majority of the electorate. To date, there is no record of any candidate for public office winning or losing on a science policy issue, but if differences on science policy matters were to become partisan issues, this would be a potential avenue for broader policy involvement by the nonattentive public.

While extensive participation by the nonattentive public in the formulation of science policy appears unlikely, it is important to recognize that the mass public always holds a final veto on all public policy matters if, and when, they become sufficiently interested or concerned. Like foreign policy, science policy in the postwar years has been a bipartisan low-profile activity, which has insured control of the policy process by the leadership group primarily and the attentive public secondarily. The prospects for a continuation of this approach will be discussed in the final chapter.

Given the lack of interest in, and knowledge about, science policy on the part of the nonattentive public, it is useful to specify what this segment of the population thinks about organized science. Hennessy (1972) has suggested a useful differentiation between opinions and attitudes. In Hennessy's terms, opinions reflect the general disposition of an individual toward an attitude object about which he has little interest or knowledge. Opinions are fragile and highly unstable. In contrast, attitudes refer to more deeply rooted views about a policy or issue that are based on the values and life goals of the individual. There would be a higher level of internal consistency among an individual's attitudes about a salient issue or policy and a greater degree of stability over time.

In terms of the stratified policy-formulation model described above, it would be expected that the members of an attentive public would have well-developed attitudes toward the issue cluster to which they were attentive. These attitudes would tend to be ordered and consistent over time. To study public participation in the formulation of policy in any given area, it would be necessary to understand the attitudes of the appropriate attentive public. In contrast, it would be unlikely to find many individuals in the nonattentive public with well-developed attitudes on issue areas to which they were not attentive, but these individuals might be expected to have a set of general dispositions toward various issue areas.

GENERAL DISPOSITIONS TOWARD SCIENCE AND TECHNOLOGY

As a preface to an examination of direct policy participation by the policy leaders and the attentive public for science policy, it is useful to focus briefly on the general dispositions of all strata of the system toward organized science. In broad strokes, it appears that the American public holds the previous achievements of science in high regard and expects even greater achievements in the future. When asked about the sources of U.S. influence in the world, there was broad agreement that "our" technological know-how and form of government were the two most important influences (see table 15). The nonattentive public ranked the American economic system third, in contrast to the attentive and interested publics, which ranked scientific creativity third. In the view of the nonattentive public, "our" scientific creativity and "our" educational system were tied for sixth.

TABLE 15
SOURCES OF U.S. INFLUENCE IN THE WORLD: 1979

Sources of U.S. Influence	Attentive Public	Interested Public	Nonattentive Public
Our technological know-how	68%	47%	40%
Our form of government	37	40	45
Our scientific creativity	33	29	17
Our economic system	31	27	26
Our natural resources	12	16	23
The racial and ethnic mixture of our population	7	11	13
Our religious heritage	6	15	19
Our educational system	6	15	17
N =	321	334	1595

Beyond this positive assessment of previous contributions to national influence, the public appears to hold moderate to high expectations for the future. When asked to estimate the likelihood that scientific research would be able to achieve selected objectives within the next 25 years, the attentive and interested publics were significantly more optimistic than the nonattentive public (see table 16). A majority of the attentive and interested publics thought it "very likely" that science would develop accurate earthquake predictions, desalinate seawater, and find a cure for the common forms of cancer in the next 25 years. Reflecting their shallower knowledge base, respondents who were not attentive to science policy were more reluctant to use the "very likely" response and were more disposed toward the "possible but not likely" position. All strata of the public thought it relatively unlikely that scientists and social scientists would be able to place communities in outer space, reduce crime, or control inflation in the next 25 years.

All strata of the public attributed substantial responsibility for the national standard of living to science and technology, but there was also evidence of an awareness of potential negative results. Approximately 90 percent of all strata of the public agreed that scientific discoveries and technological know-how were responsible for our standard of living and were making our lives healthier, easier, and more comfortable (see table 17). At the same time, a majority of the interested and nonattentive publics were willing to agree that scientific discoveries and new technologies were making "our lives change too fast." Over 40 percent

TABLE 16
PUBLIC EXPECTATIONS FOR FUTURE SCIENTIFIC ACHIEVEMENTS

	Attentive Public		Interested Public		Nonattentive Public	
	1979	1981	1979	1981	1979	1981
Percentage expecting science within the next 25 years to . . .						
Predict earthquakes*	72%	63%	54%	63%	46%	NA
Supply cheap energy*	81	74	60	65	50	NA
Cure cancer*	58	59	48	61	43	NA
Put communities in space*	28	21	18	23	13	NA
Reduce crime	14	NA	17	NA	14	NA
Desalinate seawater*	64	63	47	63	39	NA
Control inflation	NA	20	NA	28	NA	NA
Mean score on Index of Scientific Expectations (range: 0–5)	3.0	2.8	2.2	2.7	1.8	NA
N =	289	637	292	617	839	1940

* = included in Index of Scientific Expectations

TABLE 17
GENERAL DISPOSITIONS TOWARD SCIENCE AND TECHNOLOGY: 1979

Percentage Agreeing That . . .	Attentive Public	Interested Public	Nonattentive Public
Scientific discoveries are largely responsible for our standard of living in the U.S.	92%	90%	87%
Scientific discoveries are making our lives healthier, easier, and more comfortable	90	84	82
Technological know-how is largely responsible for our standard of living in the U.S.	96	91	90
Scientific discoveries make our lives change too fast	42	53	59
New technologies based on scientific discoveries make our lives change too fast	45	62	60
Scientific discoveries tend to break down people's ideas of right and wrong	27	43	43
Future scientific research is likely to cause more problems than to find solutions to our problems	18	35	41
N =	321	334	1595

of the interested and nonattentive publics concurred with the idea that scientific discoveries tend to break down people's ideas of right and wrong. In addition, 40 percent of the nonattentive public agreed that future scientific research is more likely to cause more problems than find solutions to current problems: an extremely pessimistic view.

These data suggest that a significant segment of the population has come to view science as a two-edged sword, capable of bringing significant advances in health and other material comforts but also challenging traditional views and values and perhaps creating new types of problems for society. When asked to assess this balance of risks and benefits, a substantial majority of all strata of the public concluded that the benefits of scientific research have outweighed actual and potential hazards (see table 18). In both 1979 and 1981, about three-quarters of the interested public and two-thirds of the nonattentive public con-

TABLE 18
PUBLIC PERCEPTION OF THE RELATIVE BENEFITS AND RISKS OF SCIENTIFIC RESEARCH

	Benefits Greater Than Risks			
	1979 %	1981 %	1979 N	1981 N
Attentive Public	88	90	322	637
Interested Public	74	79	335	617
Nonattentive Public	63	66	978	924

TABLE 19
MULTIVARIATE DISTRIBUTION OF RISK-BENEFIT ATTITUDES: 1979

Gender	Education	Science Courses	Attentive to Science	Benefits Greater Than Risks	N
Male	no college	none	yes	91%	79
			no	64	450
		some	yes	93	58
			no	80	51
	college	none	yes	—	8
			no	—	11
		some	yes	92	63
			no	83	54
Female	no college	none	yes	81	53
			no	61	594
		some	yes	81	21
			no	74	92
	college	none	yes	—	6
			no	—	9
		some	yes	84	32
			no	83	54

cluded that the benefits of organized science outweighed its risks. Nine of 10 science policy attentives reached the same conclusion.

While some individuals perceived various actual and potential negative effects from scientific research, a majority in all strata believed that the benefits outweighed or justified the risks (see table 19). This balanced view may be seen as healthy, since it allows an individual to accept and reconcile an instance of harm from scientific work without discarding his or her support for the larger scientific enterprise. Since almost a third of the nonattentive public rejected the conclusion that the benefits of scientific research were greater than its risks, it is important to better understand the structure of these reservations.

A multivariate logit analysis of the public's assessment of the risks and benefits of scientific research was conducted, following the same procedures utilized in the logit analyses reported previously. The dependent variable was dichotomized into those persons who concluded that the benefits were greater than the risks and those who did not share that view. The same dichotomies utilized in previous analyses were retained for college science course exposure, formal education, gender, and attentiveness. The percentage within each multivariate category that believed that the benefits outweighed the risks indicated that attentiveness to science policy and exposure to a college science course were strongly associated with a positive view of the risk-benefit ratio for scientific research.

The results of the multivariate logit analysis indicated that attentiveness to science policy and exposure to a college science course accounted for a substantial share of the total mutual dependence in the model. In a stepwise analysis, exposure to a college-level science course accounted for 43 percent of the total mutual dependence, while attentiveness to science policy explained an additional 31 percent (see table 20). Once exposure to a college science course was taken into account, the effects of the completion of a baccalaureate and gender were reduced to about 7 percent each: a minor contribution to the total model.

When this set of general dispositions is viewed as a composite, it appears that the public attributes to science and technology a central role in the nation's influence in the world and in the standard of living of the American people. The level of expectations for future scientific achievements indicates that the past is viewed as prologue, especially among the attentive and interested publics. The substantial proportion of the public that reported that the benefits of scientific research had outweighed its risks points to a solid foundation of public confidence in science and technology.

TABLE 20
A LOGIT MODEL TO PREDICT RISK-BENEFIT ASSESSMENT ATTITUDES: 1979

Models		df	LRX^2	CMPD
H1	CEAG,Y	15	111.91	—
H2	CEAG,YC	14	63.15	.436
	Difference due to YC	1	48.76	.436
H3	CEAG,YC,YE	13	55.27	.506
	Difference due to YE	1	7.88	.070
H4	CEAG,YC,YE,YG	12	48.56	.566
	Difference due to YG	1	6.71	.060
H5	CEAG,YC,YE,YG,YA	11	14.48	.871
	Difference due to YA	1	34.08	.305

Y = risk-benefit assessment
C = college science courses
E = education
G = gender
A = attentiveness

df = degrees of freedom
LRX^2 = likelihood ratio chi-square
CMPD = coefficient of multiple-partial determination

There is, however, a significant level of awareness among the public of actual and potential hazards associated with science and technology. There was widespread awareness that the pace of change has been increasing and that science and technology were at least partly responsible for this development. Some people appeared to view new scientific discoveries as a challenge to moral beliefs, particularly among the interested and nonattentive publics.

All of these results must be set in the context of Hennessy's differentiation of opinions and attitudes. It is likely that the views expressed by the attentive public reflected a combination of interest, knowledge, and other political and social values. In contrast, the views of the nonattentive public appeared significantly less well organized. For example, when asked about the sources of national influence, the nonattentive public ranked technical know-how relatively high and assigned relatively low ranks to both scientific creativity and the educational system, reflecting a profound misunderstanding of the sources and nature of technical know-how. Similarly, when asked in the 1979 survey about the scientific or non-scientific basis of astrology, almost half of the nonattentive public expressed the view that astrology was "very" or "partially" scientific. Given these indicators of confusion, it would be inappropriate to interpret the preceding data as reflecting firm and consistent attitudes on the part of the nonattentive public. These are fragile views at best and when confronted with a concrete issue in a referendum, for example, the endorsements of prominent entertainers and other public figures may be as important as the policy pronouncements of scientists, engineers, and scientific organizations. The major value of these data may be that they do not support the claim that there is a strong or growing public distrust of science in the United States or the beginning of an overt antiscience mood.

Chapter 10

The Formulation of Science Policy in a Democratic Society

There is inherent tension between the democratic values of our political system and the stratified structure of public participation in the formulation of science policy. Despite the analysis of the preceding chapters concerning the necessity, even the imperative, for a high degree of political specialization in modern societies like the United States, there is an uneasiness about a system in which two or three thousand policy leaders and a fifth of the public make policy decisions that have broad societal import. Recognizing this tension, this chapter will explore the question of whether the stratified structure of public participation is inherently elitist and whether there is an optimal size for the attentive public for science policy. The chapter will conclude with an examination of the utilization and effectiveness of the attentive public in the policy-formulation process.

IS AN ATTENTIVE PUBLIC INHERENTLY ELITIST?

The issue of whether the attentive public concept is inherently elitist concerns not only the size of attentive publics but, more importantly, their composition. Although it would be possible to argue on philosophical grounds that all binding political decisions ought to be made by at least a majority of the citizenry, the pressures discussed above in regard to the political-specialization process indicate that this alternative is not feasible. If some degree of political specialization is accepted as a necessary condition of modern life, then the size of the attentive public becomes relatively less important, provided that the proportion of persons attentive to any given issue is large enough to allow for some diversity of representation. In quantitative terms, it would not be reasonable to expect a majority of the American public to qualify as attentive to science policy, but, at the other extreme, an attentive public of only 1 or 2 percent would be too small to capture the diversity of public attitudes concerning an important issue domain like science policy.

The composition of an attentive public is central to the issue of elitism. If an attentive public were composed of only an exclusive segment of society or closed to large portions of the public, then that attentive public well might be considered elitist and would be offensive to the concept of democratic government. For example, if the attentive public for foreign policy were composed exclusively of persons with graduate degrees in political science or who were occupationally involved in foreign trade, it would take on an exclusive and self-serving character that would be largely inconsistent with the idea of a democratic political system. Since, in the first instance, attentiveness as defined in this analysis is driven by issue interest alone and since issue interest is not limited, or limitable, by degrees held or occupations practiced, the exclusivity or elitism implied in the preceding example would be most unlikely to occur in the American political system.

Looking at the attentive public for science policy, the analysis of its composition in chapter 4 indicated that it reflected age, gender, and educational diversity. For example, although the attentive public for science policy was somewhat better educated than other segments of the population, a majority of the attentive public did not have a college degree in either 1979 or 1981. Similarly, the proportion of males who were attentive to science policy was significantly higher than the proportion of females, but women made up about 40 percent of the attentive public for science policy in 1981. From these data, it is clear that the attentive public for science policy is not a perfect cross-section of the total public, but neither is it a major distortion of the composition of the American public. It included some persons from all educational and occupational groups and reflected a considerable range of age and regional variation. It would be very difficult to look at the demographic characteristics of the approximately 34 million Americans who are included in the attentive public for science policy and label that group elitist. Similar analyses of the demographic composition of the attentive publics for foreign policy, energy policy, local government, and local school politics have demonstrated a similar demographic diversity.

In summary, setting aside the issue of size *per se*, an examination of the demographic composition of the attentive public for science policy would not support the conclusion that this is an elitist group. More importantly, a consideration of the central role of personal issue interest in the definition of attentiveness and the unlimitable and uncontrollable nature of personal interest suggest that the concept is inherently pluralistic.

IS THERE AN OPTIMAL SIZE FOR AN ATTENTIVE PUBLIC?

In the preceding section, it was argued that the nature of the political-specialization process negated the likelihood of majority participation on issues like science policy and that the size of an attentive public was not evidence of elitism.

At the same time, it was suggested that there was some level below which the size of the attentive public would be unacceptable. This section will rejoin that issue and inquire as to whether it is possible to define an optimal size for an attentive public in general and for the attentive public for science policy in particular.

It is necessary to begin with an examination of the pattern of growth in the attentive public in recent years. The only available source of data concerning the size and composition of the attentive public in previous years is a 1957 survey of the interest of the public in science news and the public's knowledge about scientific concepts and issues.[19] The interviewing for the survey was completed only a matter of weeks before the launching of Sputnik I, making it the only available measure of public attitudes toward, and knowledge of, science in the pre-Sputnik period. Although the items were not identical to those used in the 1979 and 1981 studies, there was sufficient commonality to allow the construction of a comparable measure of attentiveness to science policy.

A comparison of the level of attentiveness to science policy in 1957 with the later measures indicated that the intervening 22 years had witnessed a modest growth in attentiveness and a substantial growth in interest in science-related matters (see table 70). An analysis of these changes within demographic classifications indicated that most of the growth in attentiveness to science policy occurred among persons who were under 35, males, and college graduates. In

TABLE 70
ATTENTIVENESS TO, AND INTEREST IN, SCIENCE POLICY: 1957–1981

	Attentive Public			Interested Public			N		
	1957	1979	1981	1957	1979	1981	1957	1979	1981
Total	14%	19%	20%	8%	20%	19%	1919	1472	2942
Age									
21–34	15	27	26	8	18	16	584	507	1068
35–54	15	16	20	9	21	19	853	492	1033
55 and over	9	13	13	5	21	22	470	473	843
Gender									
Female	11	12	15	8	21	20	1091	776	1568
Male	17	26	26	7	18	19	828	696	1375
Education									
Less than HS	5	5	7	6	17	17	1091	437	552
HS graduate	22	18	16	11	22	23	678	797	1746
Coll grad	40	46	43	7	17	12	144	238	645

---

[19] Sponsored by the National Association of Science Writers, the 1957 survey was designed to measure the public's interest in, and knowledge about, science and technology. The study was conducted by the Survey Research Center at the University of Michigan and obtained interviews with 1919 adults. For a technical description of the sample design, instrument, and data-collection procedures, see the Appendix and Davis (1958).

contrast, the growth of the interested public for science policy cut across all age, gender, and educational groupings. These data point to several important conclusions.

First, interest in science-related public policy matters appears to be broadly based and to be increasing among all sectors of the population. Since a high level of interest is a prerequisite for attentiveness, this pattern of interest indicates that science issues compete relatively well in the political-specialization context. In short, the major barrier to broader public participation in science policy does not appear to be the level of interest in science-related matters.

Second, the differential patterns of growth in attentiveness to science policy suggests that there are parallel differences in the distribution of knowledge about scientific concepts and issues. The largest and most obvious difference concerns the level of formal education completed. The level of attentiveness was highest among college graduates in all three years, and the marginal increase from 1957 was also the largest among this group. It should not be surprising that college graduates are better informed about scientific concepts and issues than other segments of the population. In contrast, the slight decline in the proportion of high school graduates who were attentive to science policy and the sharp increase in the proportion of high school graduates who were interested in science-related matters suggests that high school graduates were more likely to be deficient in knowledge about scientific concepts and issues in 1979 and 1981 than they were in 1957. Although aggregate in nature, this result raises questions about the quality of the high school science experience over the last two decades.

These education-related differences account for a large portion of the differences observed by age group, but they do not account for all of the gender-related differences in attentiveness. There was no significant gender-related difference in interest in science policy matters. Since the level of knowledge of scientific concepts and issues is the only difference between the attentive and interested publics, it would appear that the lower proportion of women who were attentive in all three years and the lower rate of increase in attentiveness must be associated with the acquisition of scientific information.

To examine the relative impact of gender in the context of educational and age differences, a multivariate stepwise logit analysis was performed, following the procedures described in previous sections. Attentiveness was treated as the dependent variable and dichotomized into attentives and nonattentives. Age was treated as a trichotomous variable and gender as a dichotomous variable. Education and exposure to a post-high school science course were combined into a trichotomous variable: less than college with no post-high school science courses, less than college with at least one post-high school science course, and college graduates.

The results of the stepwise logit analysis indicated that the new educational typology accounted for 80 percent of the total mutual dependence in 1957 and 70 percent in 1979 (see table 71). With educational level and course exposure

TABLE 71
A LOGIT MODEL TO PREDICT ATTENTIVENESS TO SCIENCE POLICY: 1957, 1979

Models		CMPD	
		1957	1979
H1	TAG,YT.	.804	.703
	Difference due to YT	.804	.703
H2	TAG,YT,YA.	.860	.747
	Difference due to YA	.056	.044
H3	TAG,YT,YA,YG.	.946	.890
	Difference due to YG	.086	.143

Y = attentiveness to science policy
T = educational typology
A = age
G = gender
CMPD = coefficient of multiple-partial determination

held constant, age differences accounted for about 5 percent of the total mutual dependence. With the educational typology and age held constant, the residual association between gender and attentiveness accounted for 9 percent of the total mutual dependence in 1957 and about 14 percent in 1979.

This result indicates that the gender difference in attentiveness to science policy has grown both numerically and in relative importance during the last two decades. Since there was no difference in the proportion of males and females in the interested public for science policy, the residual gender difference must be attributed to differences in knowledge about scientific constructs and issues. In a previous analysis of the 1978 survey of young adults, Miller, Suchner, and Voelker (1980) concluded that gender differences in attentiveness were attributable to early sex-role socialization and to a significantly lower level of peer interest in politics among young women. Whatever its roots, this gender difference should be addressed by science educators.

Projecting these patterns of growth into the future, it would appear that the size of the attentive public for science policy will continue to increase gradually, reflecting primarily the proportion of young adults attending college or participating in other post-high school science course experiences. If the trend of the last two decades were to carry forward, the attentive public for science policy would include about a quarter of the adult population by 1990 and perhaps 30 percent by the turn of the century. The total size of the attentive public would expand toward 40 million adults. This growth pattern would continue the current diversity of the attentive public but would gradually increase the proportion of college graduates and advanced degree holders. It would not, however, become grossly unrepresentative of the American people.

Having examined the present trends in regard to the size and composition of the attentive public, it is appropriate to return to the original question of whether there is an optimal size for an attentive public generally and for the attentive public for science policy in particular. At a general level, it would appear that an attentive public should be sufficiently large and broadly distributed to assure that, when mobilized, it could influence enough legislators to effectively veto policies detrimental to its interest. For some issue domains, like agriculture, the geographic distribution of that attentive public and the configuration of congressional committee memberships relevant to those interests combine to require a relatively small attentive group. Other issues, like foreign policy or science policy, lack the regional characteristics of agriculture and must be able to seek to influence legislators from all regions of the country. For "national" issues, a larger attentive public is required to effectively protect its interests. While there is no algorithm to estimate the minimal size of an attentive public, the considerations discussed above can be employed to assess the attentive public for any particular issue.

Applying these considerations to the attentive public for science policy, it would appear that this attentive public already exceeds the minimum size needed to protect its interest. Using the previous estimate of about 34 million adults, a mobilization rate of 10 percent would yield 3.4 million contacts with decision makers and a mobilization rate of 20 percent would produce nearly seven million citizen contacts. For most purposes, this would be a sufficient volume of activity to assure the protection of the status quo. The continued growth of the attentive public for science policy along the patterns suggested above would assure a continuing ability to defend the interests of the scientific community.

It is reasonable to inquire what size attentive public would be needed to modify the status quo in favor of organized science. The question and the issues it raises are much more complex. At the present time, the major issue on the science policy agenda is a larger share of federal resources for scientific research and education generally and for basic scientific research in particular. In the current federal budget-making process, the acquisition of a significantly larger share of funding would require either a higher federal deficit, higher federal taxes, or a share of resources previously allocated to another purpose or program. All three of these options would activate other attentive publics that would wish to either preserve their share of federal funding or avoid new taxation. In the context of high unemployment and other competing social needs, it is unlikely that organized science could generate an attentive public large enough to produce sufficient direct pressure to force a reallocation of federal resources in favor of science. In the context of the pressure controversies over federal spending and the allocation of the federal budget, it is likely that a united science policy leadership group would be more effective in persuading both executive and legislative decision makers of the need for additional resources.

Apart from current federal budget policy, the need for an attentive public for science policy large enough to directly influence the establishment of new policies is not clear. On most matters that are technical in character, science policy leaders are more effective in presenting and negotiating for specific policy outcomes than the broader and more diverse attentive public. In very general terms, for policies other than the acquisition of resources, the primary use of the attentive public for science policy is defensive, that is, to protect against the erosion or reversal of the postwar relationship between the government and organized science.

In summary, there is no single optimal size for an attentive public. A minimal size for an attentive public can be defined conceptually, depending on the regional or national character of the issue and the nature of the policy demands. For national issues like science policy, it is necessary to have a sufficiently large attentive public to be able to generate contacts with a broad spectrum of legislators, especially for the purpose of protecting current policies. It would appear that the attentive public for science policy is, and will remain, large enough for that purpose.

THE UTILIZATION AND EFFECTIVENESS OF THE ATTENTIVE PUBLIC

The most important questions concerning the attentive public for science policy are the frequency of its utilization and its effectiveness in the policy-formulation process. This section will explore the frequency of leadership efforts to mobilize the attentive public for science policy and then seek to assess the efficacy of the contacting and other interventions by the mobilized portion of the attentive public.

Looking first at utilization, science policy leaders have rarely attempted to mobilize the attentive public for science policy. The problem is twofold. First, the nongovernmental leadership of science policy has been successful in obtaining most of the policy objectives of organized science in the postwar years through direct negotiation with decision makers. In the conflicts over the establishment of the Atomic Energy Commission and the National Science Foundation, science policy leaders did appeal for broader public pressure but mainly through disciplinary and professional associations. An attentive public of the current size and scope was not available in the late 1940s and the influence of scientific leaders fresh from their wartime achievements was sufficient for most purposes. The acquisition of additional resources during the 1950s and in the first decade after Sputnik did not require additional public pressure.

Despite the slippage in real purchasing power for scientific research during the 1970s, science policy leaders did not attempt to mobilize broader public support for the acquisition of additional resources. Current dollar increases during this period were relatively large, and the full impact of inflation did not become apparent until later in the decade. The first overt attempt to mobilize broader public support for scientific research and for science education came in response to the substantial domestic-spending reductions proposed by the incoming Reagan administration.

The second part of the problem concerns the attitudes of at least a portion of the science policy leadership concerning the scope of public participation in the formulation of science policy. Even though most science policy leaders agreed with the statement that the interested and informed citizen could have some influence on science policy, only half of the leadership group reported any effort to mobilize nonscientists who were interested in, and informed about, science policy: the attentive public. An examination of the mobilization activities of the major disciplinary societies and professional associations indicates that most of these organized efforts focus on their own membership, with little or no overt effort to identify and mobilize the 30 million attentives in the broader population. The utilization of the attentive public for science policy must await, in part, the development of greater awareness of the existence of these citizens by science policy leaders and a willingness to open the policy-formulation process to nonscientists.

Given the infrequent efforts of science policy leaders to mobilize the attentive public, it is difficult to assess its efficacy. The most recent effort to mobilize attentive participation in the policy process was the activities of the major disciplinary societies and professional associations to protect science funding from prospective budget reductions in domestic programs proposed by the incoming Reagan administration. A major portion of this effort was designed to persuade the members of scientific groups to contact congressional decision makers to emphasize the importance of funding for scientific research. There were only a few efforts to reach out beyond the scientific community itself, and the results of the mobilization effort were correspondingly modest. The data from the 1981 study indicated that only 5 percent of the attentive public reported contacting a public official on a science-related matter in the previous year, and a detailed analysis of the subject matter of those contacts revealed that many of the contacts concerned nonresource issues or were contacts with state or local officials on other matters. A realistic estimate of the mobilization of the attentive public concerning science funding would be 1 or 2 percent. This is a disappointing level of mobilization by any yardstick. The fact that scientific research suffered relatively smaller budgetary reductions than other domestic activities in the first Reagan budget should be viewed as the result of effective lobbying by science policy leaders directly rather than as a measure of the utilization or effectiveness of the attentive public.

A parallel mobilization effort was made to prevent the elimination of federal science education programs by the Reagan administration. In this case, the mobilization effort involved both science policy organizations and educational associations with concerns broader than science. While solid data on the level of mobilization are not available in regard to the science education controversy, it would appear that pressure from the mobilized public helped persuade a sufficient number of House members of the value of the program to protect it within the committee structure. The recommendations from the Committee on Science and Technology and the Appropriations Committee were both favorable to science education. In this case, the Reagan strategy of placing a substitute budget bill before the House of Representatives after the conclusion of the normal committee review process successfully negated the efforts of the mobilized segment of the attentive public for science policy to preserve a federal program in science education. Using the parallel mobilization effort to preserve scientific research funding as a yardstick, it is likely that the rate of mobilization on the science education issue was at the 2 to 3 percent level.

From this brief review, it would appear that there have been relatively few efforts by science policy leaders to mobilize the attentive public in the postwar years and that those few attempts have not been very successful. The high rate of success of science policy leaders in obtaining their major objectives without broader public pressure has resulted in a leadership group largely unskilled in mobilization and an attentive public unaccustomed to being mobilized. If the success of science policy leaders in direct negotiation could be assured for the future, there would be no occasion for concern. That assumption, however, is risky.

The reform of the federal budget process in the latter part of the 1970s effectively changed the resource allocation process into a zero-sum situation. Each sector of society that seeks a share of federal resources must compete against other claimants in *both* the substantive appropriations subcommittee and in the general budget committee. During periods of overall budgetary growth, the competition for resources is vigorous but manageable. During periods of stable or declining budget ceilings, the competition for resources becomes much more intense and hostile. For a variety of political and economic reasons, it is likely that the aggregate level of federal resources, in real dollar terms, will be relatively stable over the next decade and perhaps beyond. It is very likely that the competition for those resources will intensify.

The impact of political specialization on American politics has been substantial and, undoubtedly, will be even greater in the decade ahead. In this political milieu, science policy leaders will need to mobilize their attentive public more frequently and more effectively. The cultivation and mobilization of an attentive public is a continuous long-term process. If the interests of organized science are to be advanced, it is essential that both science policy leaders and the attentive public for science policy improve their understanding of the policy-formulation process and learn to work cooperatively in that process.

Kenneth Prewitt

Scientific Illiteracy and Democratic Theory

In an earlier era, discussions of scientific literacy focused more often than not on whether nonscientists could achieve an aesthetic appreciation of the complexity, beauty, order, and ever-deepening mystery of the natural world as revealed through science. That theme is little heard today. Appreciation and aesthetics have been replaced by issues of economics and politics. The major issue in the current clamor over scientific illiteracy is about the technical skill-level of U.S. workers, or more broadly, about the decline of the United States in the world political economy. We have heard the argument many times.

Although the United States remains powerful, it no longer stands at the top of the various status rankings that measure political, economic, and military clout in the world. It has lost its number one status. The image of a declining world power, being here edged aside by Japan, there by the Soviet Union, elsewhere by Germany or France, has turned national attention to how we might arrest the decline. There is no shortage of answers to why we are slipping. Religious leaders emphasize deteriorating moral standards; business leaders point to excessive government interference in the free market system; labor leaders suggest mismanagement of the economy by corporate leaders; parents complain about the public education system; the political party out of power blames the party in power, and the party in power tries to deflect attention to all of the above. There are even a few persons—by some called realists, by others defeatists—who say that the dominant role enjoyed by this country in the postwar period was a historical accident and not the result of some identifiable attribute of the American people or their institutions.

Science leaders, of course, have now supplied their version of why the United States is slipping: the country, they contend, is underinvesting in research and development. It has failed to maintain scientific instrumentation. It is producing a scientifically incompetent population. It cannot expect to compete in the world economy with second-rate engineers, with outmoded equipment, with an untrained labor force. It cannot compete militarily if military personnel are unable to read technical manuals or operate sophisticated weapons. And if the United States cannot compete economically or militarily, its international political leadership will weaken. The causal connection between R&D investment and level of scientific competency, on the one hand, and the standing of the United States in various global stratification rankings, on the other, has become an article of faith among science leaders. A renewed effort in science education is justified in terms of economic productivity, national security, and international political influence.

This perspective touches also upon the conditions under which science is practiced in the United States, suggesting the additional dimension of scientific literacy. Does the public sufficiently understand science that it will support the kind of science policies agreeable to the practitioners of science? Science in this country has long enjoyed a privileged status, and consequently has historically benefited from constructive science policies, which have ranged from benign neglect to active support. But beginning in the 1970s, a new and alarmist note crept into discussions about the place of science in this society. Scientists were no longer so certain of their privileged status. Public confidence in science appeared to be declining. Scientific expertise enjoyed less automatic deference. Public discussion of the actual or potential harm associated with scientific discoveries and their attendant technologies seemed to outpace references to the benefits of science. There was even talk of limitations on scientific inquiry and more than talk about regulations and accounting procedures. Was the public turning antiscience? asked science leaders. If so—and the evidence was confusing—what could be done about it?

Deepen and extend the public understanding of science was one answer. Public understanding of science, so the argument goes, is necessary if the United States is to have reasonable science policies. Public *mis*understanding of science will work its way through the political process and emerge in such detrimental policies as wide fluctuations in science funding, political rather than scientific criteria for setting research priorities, unrealistic demands for quick practical results, and misguided regulations or accounting procedures.

There is an obvious connection between the two dimensions of the scientific literacy problem. Misguided science policy can damage the practice of science, and if science itself is impeded, the argument goes, the society suffers. Scientific illiteracy consequently poses a dual problem. It can imply that the population, especially the work force, is unable to maintain the scientific-technical momentum necessary for national growth. And it can imply that the public will fail to provide the support necessary for scientific growth.

These considerations are joined by a third, one that lays open an even more complicated issue in the scientific illiteracy discussion. The metaphor here is not economic productivity or public understanding, but a crisis of democracy. What is to be the foundation of intelligent citizen participation in a society increasingly directed by scientific and technical processes often beyond the comprehension of any save the experts? Can the nation's commitment to equal and informed participation in public affairs be sustained if citizens are scientifically illiterate? Many have written that the prospects for democracy are bleak. Commenting on the "baffling subtleties of contemporary science [which] drift further away from the understanding of lay citizens," Theodore Roszak argues that "one cannot go on indefinitely acknowledging that that which makes one's world go round and mediates all reliable knowledge of reality is hopelessly beyond one's comprehension, and therefore one's control."[1] Or, at least one cannot go on this way and still exercise the rights of citizenship in a democracy.

The contemporary scientific literacy discussion raises in a fresh light an ancient issue for democratic theory and practice. Because the average citizen lives a life far removed from the political, economic, cultural, and technical centers that give direction to society, it has always been a theoretical and practical challenge to ensure that the participatory rights bestowed by democracy will be meaningfully exercised. When the directing centers of society are not within the comprehension of the average citizen, public affairs cease to be public and thus slip out from under lay control.

The late twentieth century brings to this old issue a new set of challenges. The lay public increasingly confronts a political agenda that has been fashioned by technical processes that only the experts can understand. Consider the range of questions on which the citizen is expected to have an informed view. Will fluoridating the water system reduce tooth decay? Can statistical modeling estimate the probability of fuel core meltdown in nuclear plants? Are the epidemiological studies sound in concluding that there is no causal connection between Bendectin and birth defects? Can genetic screening of employees reduce the risk of workplace exposure to biochemical dangers? Can radioactive waste material be safely transported and stored? Is insanity a legitimate defense in criminal homicide cases? Has the Green Revolution headed off famine, or has it accelerated the deterioration of the crumb structure of the topsoil? These issues and hundreds of similar ones present models, evidence, and scientific theories that are beyond the comprehension of any save a small number of experts.

I have now briefly alluded to three themes that appear in the current discussion of scientific illiteracy in the American public. The initial theme is the narrowest of the three. It starts from a concern about economic productivity and national prestige, and laments a troubling and worsening situation: the decline in mathematical abilities among students, the lack of preparation in science and math, the declining enrollments in science courses, the empty teaching positions in high-school science and college engineering curricula. These trend lines document the deterioration in science education and in scientific competence even as other trend lines document the rising demands for technically trained workers. The intervention strategy is, of course, focused on the school-age population. Various improvements and reforms are being discussed and in some instances implemented: renewed emphasis on teacher preparation, additional course requirements in science and math, scholarships and prizes to reward scientific and engineering accomplishments, improved classroom equipment and pedagogic aids. But because able and dedicated persons—including several of the authors in this volume—are analyzing this dimension of scientific literacy, it will not receive attention here.

The second theme might more appropriately be called the public understanding of science rather than scientific literacy, although the distinction is fuzzy at times. Public understanding encompasses such issues as confidence in science, willingness to fund basic research, and an appreciation of the conditions under which effective science is practiced. In short, what is at stake is science policy—the ways in which a modern industrial democracy organizes and funds scientific research and training. This theme receives some attention in this essay, although largely as a by-product of my more central concern with the third dimension of the scientific literacy discussion, to which I now turn.

II

Clifton R. Wharton, in anticipating the kind of education that will be needed in the twenty-first century, writes:

> more and more people use modern technology while fewer (relatively speaking) understand how it works. Instead, we are content to be served by cadres of technicians and specialists and, thereby, to cede to them an inordinate, even ominous amount of control over our lives. The great mass of people, including many college graduates, are at least arguably in danger of becoming what a recent rather inflammatory book called "techno-peasants": modern-day serfs, nominally free but disenfranchised by ignorance—and fear—of prevailing technologies.[2]

This passage usefully sets the problem, but it does not tell us what kind of scientific literacy will be necessary if citizens are to avoid techno-peasant status. Although we might wish for a society in which citizens can read *Scientific American,* it would be a wish begging to be frustrated. It is not easy even to find a scientist who can read through *Scientific American.* But if we are not talking about the substantive knowledge commonly assumed by the phrase "scientific literacy," what are we talking about when we worry the issue of democratic participation in a technical society?

This essay will make use of the term *savvy.* We speak of savvy people as those persons who act on the basis of a shrewd understanding of the deeper principles and structures that govern complex situations. A savvy person has those skills and insights that make for survival and success in what would otherwise be bewildering and intimidating situations. Political savvy, for instance, is familiarity with the informal rules, the operating codes, the levers of power, and the folklore of an ongoing political process. Entrepreneurial savvy is the know-how necessary to make the intricacies of the marketplace work on behalf of new ideas and new products.

Perhaps the most suggestive use of the metaphor is in the slang phrase "street savvy" or "street-wise." Street savvy separates the outsiders from those in the know; it reduces confusion and seeming chaos on the streets to known rules, predictable patterns, and understandable hierarchies. It is a second sense about why things happen as they do. Situations that to the person lacking in street savvy are threatening are to the savvy resident the commonplaces of life on the street.

If we carry the metaphor into our discussion of scientific literacy, we can speak of "savvy citizens" as those not bewildered or intimidated by the introduction of new technologies or the arrival of new scientific languages. The scientific-technological underpinnings of the society are familiar turf. By definition, savvy citizens are not outsiders in their own society. Rather than being manipulated (or feeling manipulated) by forces beyond understanding and beyond control, the citizen "in-the-know" can make the system work.

Scientific savvy is hardly a new idea. It is one way of describing the practical knowledge possessed by members of any functioning society. Malinowski, in the opening paragraphs of his classic essay on magic, science, and religion, writes that

> a moment's reflection is sufficient to show that no art or craft however primitive, could have been invented or maintained, no organized form of hunting, fishing, tilling, or search for food could be carried out without the careful observation of natural process and a firm belief in its regularity, without the power of reasoning and without confidence in the power of reason; that is, without the rudiments of science.[3]

Malinowski describes in some detail the scientific principles and technical skills brought to bear by the Trobriander sailors, fishermen, and farmers. Similar documentation has been provided by many anthropologists reconstructing the economies and polities of so-called prescientific cultures. A shrewd understanding of winds and rains and storms, of insects and seeds and soils, and of animal habits and edible plants, has long been the scientific lore of fishing, planting, and hunting cultures.

We make no claim, then, that in introducing the notion of scientific savvy we are suggesting anything new. The newness is not in the notion, but in the content. What is the American citizen's equivalent of the Trobriander's confident understanding of hydrodynamics and equilibrium, such that he would launch his outrigger in threatening conditions? The division of labor anticipated by Adam Smith and sociologically described by Durkheim has moved us a long distance from the Trobrianders, as of course has the exponential growth of scientific and technical knowledge.

In reaching toward an understanding of what will constitute scientific savvy, or street wisdom, for the citizen of an advanced industrial democracy, I want to start with a distinction between the personal and the public. By personal, I mean those things that would help a person effectively apply the principles and products of science in his or her own life. As suggested in this volume by Barton Childs and Faith Hickman, parents, or potential parents, need to understand the benefits that antenatal diagnosis can provide. To generalize the point, the revolution in biology has opened up the whole field of behavioral health, which has brought within the reach of many citizens the prospect not only of longer life, but of a longer, healthier life. To be shut off from these discoveries by scientific illiteracy is to be denied the personal benefits of modern science. Similar examples could be given in such nonhealth fields as computers and electronics, which have laborsaving potentials even greater than the mechanical inventions brought into personal use by the scientific and technical work of the late nineteenth and early twentieth century.

But of course it is not in health-related behavior or laborsaving devices that scientific savvy is of most importance at the personal level. The area that understandably receives the most attention is employment. Persons bewildered by modern technology are denying themselves employment opportunities in an economy resting on an ever more complicated technical base. If the Trobriander farmer needs a workable knowledge of soils and seeds and rainfall, the contemporary farmer in the United States needs a workable knowledge of insect physiology, chemical fertilizers, weather modification techniques, hybrid strands, animal breeding, mechanized and computerized support systems, price supports and parity, and international commodity agreements and trade patterns.

There is no end to examples of how scientific savvy can benefit the parent, the consumer, the employee, the producer, or how the lack of it could limit their opportunities in serious ways. And much of the scientific literacy discussion is pitched at this level. Schools should teach science in a manner that opens up personal opportunities for all their graduates, not just for those whose career choice is science or engineering.

At the personal level, scientific savvy has to do with effectively coping, or with what Mary Budd Rowe in a companion essay has termed fate control. But we are also concerned with the citizen's involvement in public life. In this domain we have a less clear view of what might constitute scientific savvy. It is the question I take up in the remainder of my paper.

III

If the economic and political life of our society rests in important ways on a scientific-technical base, we can hardly claim that public affairs are in the hands of the public when citizens are poorly informed or misinformed about science and technology. The problem with this oft-repeated truism is that it fails to identify the scope and form of information necessary to ensure democratic politics. Or worse, the truism makes the facile assumption that citizens, if only we can get our educational system straightened out, can acquire a substantive understanding of scientific principles and discoveries. This may be true for some of the citizens somewhat, but if the future of democracy rests upon a substantive conception of scientific literacy, the prospects are not good. To franchise only those citizens who can understand the scientific principles behind a missile program, a toxic waste disposal system, a cancer cure, or a genetic screening program, is to franchise only a small part of the populace.

From the perspective of democratic practice, the notion of scientific literacy does not start with science itself. Rather, it starts at the point of interaction between science and society. My understanding of the scientifically savvy citizen—in contrast to the scientifically savvy parent or consumer or employee or producer—is a person who understands how science and technology impinge upon public life. Although this understanding would be enriched by substantive knowledge of science, it is not coterminous with it. This argument can be worked out under three general headings: the political process; public policy-making; and social change. In each instance, I barely begin the conversation, but in each, I hope to indicate that there are some fundamental principles which, if understood by the lay citizen, could help preserve a democratic polity despite the expanding technical content of many public issues.

The Political Process

A familiar complaint is that the issues on the political agenda often have technical dimensions beyond the comprehension of practically all citizens, issues such as radioactive waste management or arms reduction or genetic engineering. What is a democracy to do when the premises of major issues can be understood by only a fraction of its population? About the best that democracy's defenders have suggested to date is to introduce into the political process the practice of countervailing expertise. In technical controversies, if the lay citizen cannot make an independent judgment, the citizen must at least be exposed to the arguments of contending experts. This is the "science court" writ large, a procedure to be used not only for scientific disputes, but also for an increasing number of public questions with technical content. There are two problems.

First, and most fundamental, the practice does not preserve democracy, at least not as we commonly understand it. To reinforce the citizens' reliance on experts is to confirm them in their fear that their practical judgment no longer has much to suggest to the big issues of the day. Roszak, I believe, asks the right rhetorical question of countervailing expertise..

> Does our democracy not continue to be a spectator sport in which the general public chooses up sides among contending groups of experts, looking on stupidly as the specialists exchange the facts and figures, debate the esoteric details, challenge one another's statistics, and question one another's prognostications? It is difficult to see that, in the long run, such a counterbalancing of expertise can be a real victory for the democratic autonomy of ordinary citizens. They remain expert-dependent.[4]

Second, to rely on countervailing expertise is to invite fraud. I have in mind the fakery that travels under the guise of pseudoscience: creation science, astrology, parapsychology. It may be a backhanded tribute to science that even wholly unscientific doctrines cloak themselves in scientific terms. But this indiscriminate use of scientific terms carries a warning to those who would depend on countervailing expertise as a method of preserving lay control in issues such as nuclear siting or fluoridation or school curriculum. Not all citizens know enough science to sort out the reasonable from the unreasonable, geology from flood geology. A political process that invites demagogy, as excessive reliance on expert testimony might, obviously would move us away, rather than toward, democratic practice.

To protect democracy will require framing public issues, even those with scientific and technical content, such that they are accessible to the lay citizen. This will primarily involve bringing to public attention the consequences of science, insofar as those can be known. We might start by examining the phrase "knowledge for its own sake" from a political as well as scientific perspective. The scientific perspective is well known: knowledge lays a foundation for more knowledge. But from a political perspective, to assert that the knowledge process simply feeds the knowledge process is misleading. Scientific knowledge serves purposes that reach far beyond the research life of science itself. These purposes are economic, political, and military. The citizen who takes at face value the "knowledge for its own sake" metaphor is doubly disenfranchised. Such a citizen has only a partial understanding of science and an even more limited understanding of politics and economics. Unless it becomes clear to citizens that the purposes of science—not at the margin, but at the core of the enterprise—are commercial, strategic, and bureaucratic, there can be no democratically involved populace. In teaching this lesson, we need not be cynical about science and about the necessity for free inquiry undirected by immediate purposes. This correct, but partial, image of science can be protected. But it should be joined with another image of science, one much more politically realistic. It is not without political significance that federal R&D spending, for instance, is allocated about one half to defense, one seventh (14 percent) to space, and only about one third to civilian purposes. Nor is it without political significance that about 80 percent of the expenditures are in applied research and development, the remaining 20 percent being in basic research.[5] The federal budgetary allocations reflect judgments about the purposes of science, as do the allocations that characterize the commercial sector.

Scientific literacy, at least from the perspective of democratic practice, encompasses an understanding of the nonscientific purposes of science. For it is on these matters that the citizens are most frequently asked to vote. Scientists do not aid in this cause by claiming that knowledge is something sought just for its own sake.

Policy-Making

A good deal of public policy-making starts from some guess about what the future will look like if some intervention is either attempted or, having been considered, is rejected. To think intelligently about public policy, then, is to think about present trends, extrapolations of those trends, and likely outcomes associated with different interventions. A scientifically literate populace would understand two things about policy-making; first, that the various scenarios of the future with which the policy process must contend are themselves often consequences of developments in science and technology; and second, that our views toward what is and may happen derive from science-based forecasts.

Forecasting is not, of course, the same thing as prediction. Scientific prediction relates the behavior of phenomena to known antecedents or independent variables. A prediction is a scientific statement about cause and effect. Forecasting, which scientists also practice, has to do with the future. More specifically, it has to do with the possible shape of the future if present trends are extrapolated.

Forecasting may or may not have a scientific base. Many forecasts start from religion—Repent, or suffer the wages of sin. Others start from politics—Arm the nation, or risk foreign invasion. Others start from economics—Reduce the deficit, or expect an economic depression. These forecasts are all guesses about the future based on extrapolating present trends and on analogous comparison to historical experience.

It is the mark of a scientific culture that so many of our forecasts today start from scientific findings. Sometimes it is the individual's future that is being forecast—Smoke heavily, and die of lung cancer. Sometimes it is the environment's future—Large-scale consumption of fossil fuels, releasing carbon dioxide into the atmosphere, will raise the average temperature of the earth's surface. There are demographic forecasts about human populations—If forecasted fertility rates and life expectancy changes occur, the earth's population will grow to between 9.8 and 10.5 billion, before leveling off a century hence. There are attempts to forecast technological events, as in the statement of probability of an accident in a nuclear power plant. There are scientifically based statements about the probability of natural disasters, earthquakes and floods, that are ignored at great risk by those in the likely paths of such disasters.

These forecasts have many things in common, not least of which are the high stakes of believing/disbelieving them. The greenhouse effect, for instance, will alter agricultural production around the world, moving the gradient of warm temperature northward and the gradient of rainfall westward. The greenhouse effect will alter the global ecology as the Arctic permafrost retreats and tropical rainfall increases. Human health will be affected as disease agents and agricultural pests migrate and reproduce in unforeseen ways.

Furthermore, the trends on which all these forecasts are based can be interrupted by changing present behavior. This is most easily seen with respect to preventive medicine—Change smoking habits, diet, life-style, and so on now, and benefit forty or fifty years hence. Of course the same preventive action can happen at a collective level, as illustrated in the birth control practices introduced in India or China, the energy sufficiency policies of the United States, or the laws that regulate residence in flood plains or earthquake-prone regions.

The type of forecasts I speak of have three important properties: they are based on a scientific understanding of the natural or social world, or often of the interaction between the two; they address issues of great, if potential, harm or benefit; and the trends that the forecasts are based on can be self-consciously interrupted by changes in individual behavior and social policies.

An important dimension of public policy is dealing with the increasingly numerous forecasts that have these three properties. It is not easy for the lay citizen. The forecasts based on science are often stated in probability terms, so the citizen needs some understanding of statistics. A citizen able to evaluate critically probability statements based on scientific evidence will have enough common sense to know it is not true that "everything will cause cancer if you give enough of it to a rat," and consequently will be able to assess whether to eliminate from his or her personal diet those toxic substances that science has linked to cancer. This does not imply that every citizen need become a statistician. It does suggest that citizens acquire a nontechnical understanding of such basic statistical principles as the law of large numbers and sampling error. Similar to these statistical principles is the companion principle of "all things being equal." Forecasts based on scientific evidence are advanced with the qualification that confounding factors are being held constant, even when it is known that factors exogenous to the model will change and thus affect the accuracy of the forecast. But there could be no statement about the future without invoking the principle of all things being equal. The nonscientist needs to understand this, else the ability to evaluate forecasts will be seriously impaired.

Finally, we take note that forecasts very often are controversial, scientifically as well as politically and economically. They are scientifically controversial because the evidence is less than conclusive. Scientists and scientific evidence will be found on more than one side of the question.

Because science-based forecasts are frequently controversial, we cannot separate scientific savvy from the ability of citizens to engage in moral reasoning. A science-based forecast often must be evaluated against moral standards. Moral reasoning is both prescience and postscience. It helps us ask what it is we want to know; it also asks questions about the human implications of what we have learned. Life-extending technologies in medicine, for example, while resolving certain biomedical questions, raise in the process numerous ethical issues. Science—always—is surrounded by moral questions. This is especially true of science-based forecasts that suggest the need to alter present behavior in order to realize future benefits for oneself or for society generally. Against what moral standards do we restructure social arrangements in the present to avoid some less-than-certain future risk or to achieve some less-than-certain future benefit? Citizens unable to reason morally will be poorly equipped to think about different scenarios for the future. In this sense, there can be no scientifically savvy population that is not also morally savvy, and no democratic discourse that is not also moral discourse.

These comments about forecasting lead directly to our concern with the connection between scientific literacy and democratic practice. Tools, it is said, extend our physical powers, telescopes extend our visual powers, decision-logic extends our reasoning powers, and so forth. Science-based forecasts do no less than extend our powers to make judgments about the kind of world we want to live in—or avoid. Citizens who cannot evaluate the merits and probabilities of science-based forecasts have as surely forfeited a measure of control over their future as if they took no note of the coming and going of political regimes and ideologies.

Social Change

Science—or perhaps here we should speak more directly of technology—for all its consequences in social life, does not introduce change in ways that are linear, predictable, or even much understood. This suggests that a dimension of scientific savvy is familiarity with a few basic principles about how science and technology operate on society.

A useful place to start is with the "bittersweet" principle of technological change. To innovate technologically is not just to offer new social benefits, it is also to impose social costs. Major changes made in the social order will always undermine some social values, harm some social interests, and penalize some social groups. There is no Pareto optimal technological change. At a minimum, persons whose livelihoods were dependent on the now-outmoded technology are likely to suffer. More likely, the transition from the old to the new technology—from carriages to cars, from assembly lines to robotics, from family farms to mechanized agribusiness—will involve many social groups and will sweep across various facets of social life, and not in a manner of immediate benefit to all those affected. The bittersweet principle can clarify public discourse about new technologies. Its acceptance would help the public understand that there will be costs to pay as well as benefits to enjoy, and the public will enter the political debate better equipped to discuss the allocation of the costs as well as the benefits.

Related to the bittersweet principle is the law of interaction effects. In interdependent systems, it is not possible to deal with one large component without disturbing other components. This systems principle is a commonplace to persons who think much about technology and society. It is apparently not a principle generally accepted in American political discourse. Consider the three goals of environmental protection, energy sufficiency, and economic growth, goals certainly comprising a tightly interdependent system. Yet we have a politics in which different, and antagonistic, interests gravitate to one or the other of the goals—environmentalists to environmental protection, pronuclear advocates to energy policy, and large-scale capital to economic growth goals. Politics won't disappear with a population appreciative of interactive system affects, but the terms of political discourse would become more reasonable.

A third principle of technology in society is suggested by sociologist Robert K. Merton, who has called our attention to the unintended consequences of purposive social action. There are unintended, and inevitable, second-order consequences to the introduction of new technologies in society. Again a commonplace. Obviously, the automobile changed how we used space; television has changed the educational process; electronic communication has made possible the growth of multinational corporations, thereby altering the international economic order; robotics will change work habits and the work force. But a savvy public needs to understand the inevitability *and* unpredictability of second-order consequences, which in turn have their own consequences. And so it goes, in an endless spiral.

The bittersweet principle, the interdependent properties of technosocial systems, and the rule of unanticipated consequences combine to caution us not to expect—or even want—a technological fix. Complex problems don't have simple answers. A savvy population recognizes a "fix" for the unrealistic goal that it is. It is the rare problem that is solved; more often, the problem changes form. To control infectious diseases is not to solve the health problem. It is to shift attention to cancer and heart failure. And when these illnesses of late middle life are cured, the health problem will shift to senility and related illnesses of the aged. Society copes with problems; it doesn't solve them. If the public is to become actively involved in debates over the introduction of major technologies—chemical fertilizers, fluoridated water, nuclear energy, genetic engineering—then the minimum literacy necessary is appreciation of this fact.

IV

The public discussion of scientific literacy will be with us for some time, as will be the three dimensions touched upon in this essay. Insofar as the emphasis is on the technical competence of the work force, it will take many years to improve precollegiate and collegiate education, as well as to institute appropriate adult training programs. Other essays in this volume testify to the seriousness of the problem and the difficulty of designing and implementing solutions.

The problems are no less great when attention turns to the public understanding of science or, more appropriately, of the public's understanding of the conditions that ensure the effective practice of science. Part of the difficulty here derives from our inadequate theories about public opinion as it impinges on science policy and science funding. I have argued elsewhere that the public probably knows more about science than scientists know about the public—"the scientific community has often shown itself to be ignorant about the way in which public attitudes are formed and become expressed through the political participation system."[6] Although some renegotiation of the contract between science and society, to use a favored metaphor, is under way, this renegotiation need not damage science. Such basic principles as peer review, minimal restrictions on inquiry, international exchange of scientific findings and data, long-term funding, and the setting of research priorities by scientists can be protected. The place of science in the public's esteem is reasonably secure, and such damage as science is likely to suffer will more likely come from scientists' misunderstanding of the public than vice versa.

The most complicated questions are raised by the third aspect of scientific literacy—what constitutes informed public involvement in the political life of a nation increasingly directed by advances emerging from science and technology. Now we are into the thicket of democratic theory and democratic practice, but around a set of problems not anticipated by our forebears. There are no ready solutions. But unless we begin to sort out the issues involved, our efforts to train a more competent work force or to design workable science policies will only scratch the surface of what should constitute scientific literacy in a democracy. To repeat the argument advanced in these few pages, we need a conception of scientific savvy that will connect today's citizen with the public life of his community in as meaningful a way as did the scientific lore of the hunting, fishing, gathering, harvesting, and farming cultures that preceded modern society. Working out this conception will be difficult enough, but it is only the initial step. A reformulated "theory of democracy" will then have to be put into practice, which will involve the public education system in a task not unlike that of an earlier century, when a mass population was introduced to the norms and rules of democratic politics.

ENDNOTES

[1] Theodore Roszak, *Where the Wasteland Ends: Politics and Transcendence in Postindustrial Society* (New York: Doubleday Anchor Books, 1973), p. 239.

[2] Clifton R. Wharton, Jr., "Curriculum 2001: Education for the 21st century," Chancellor's Report to Faculty Senate of State University of New York, April 10, 1981, p. 3.

[3] Bronislaw Malinowski, *Magic Science and Religion* (New York: Doubleday Anchor Books, 1955), pp. 17-18.

[4] Roszak, *Where the Wasteland Ends*, p. 50.

[5] See *Science Indicators 1980*, report of the National Science Board, 1981.

[6] Kenneth Prewitt, "The Public and Science Policy," *Science, Technology, & Human Values*, no. 39 (Spring 1982):5-6.

PART V

NATIONAL SCIENCE POLICY AND THE SCIENCE ADVISORY SYSTEM

BACKGROUND AND SUMMARY

In Parts III and IV, the role of scientists in American society and government was for the most part addressed in general or theoretical terms. The institutional context in which the modern science-government relationship has been conducted, although occasionally treated in passing, was not the subject of sustained inquiry or analysis. It is obvious, however, that the formal and informal organizational structures for advice and decision-making that have characterized post-war American science have had important substantive implications for U.S. science policy. Indeed, one cannot hope adequately ~~to~~ appreciate the issues that have arisen in the latter context without some understanding of the nature of the processes characterizing the former.

A. THE ORGANIZATION OF SCIENCE AND GOVERNMENT: SOME CONSEQUENCES

The literature focusing on the linkages between the wide range of institutions that have responsibility for decision-making in American science and substantive aspects of contemporary science policy has expanded considerably in recent years. The discussion of "The Organization of Science and Government: Some Consequences" in chapter three of Anton G. Jachim's *Science Policy Making in the United States and the Batavia Accelerator* has, however, certain distinctive features that commend it as a particularly appropriate selection with which to begin Part V. Its chief virtue is that it strikes what is for our purposes an especially useful balance between the general and the specific. In the earlier portion of the chapter, Jachim provides a brief overview of the formal and informal organization of professional scientific affairs and of the organization of government for decision-making in science. His overriding objective, however, is to clarify and assess the bearing that these various "structures and processes" have on that aspect of contemporary American science policy that is of special concern here, namely "the distribution of R & D resources" (p. 28).

To lay the groundwork for his discussion of allocational issues as these arise in the context of research and development in science, Jachim begins by reviewing "the expenditure pattern for R & D for the entire society" up through about 1970 (pp. 28–30), the somewhat "hazy" distinction between "basic" and "applied" scientific research and development (pp. 31–4) and the various rationales that have been offered for public support of each, especially (and more problematically) basic research (pp. 34–8).[1] He next briefly

---

[1] Jachim's discussion in this context includes an interesting and sometimes quite humorous account of the modern art of "grantsmanship" (which consists, in part, of satisfying a patron's value structure in order to get or continue a grant) and the related phenomena of "bamboozling"

Continued

distinguishes between three kinds of scientific professional associations (specialized disciplinary associations, larger associations that attempt to represent science in general, and "ad hoc" associations that represent scientists on public issues) whose leadership, he notes, is in "frequent and regular" contact and in fact "overlap[s] considerably" (pp. 39–41).

The more important portion of the chapter for our purposes begins when Jachim turns his attention to the organization of government for science. In an attempt to "lend some simplicity to . . . some extremely complicated facts and relationships", Jachim proposes to view the formal structure of the government-science apparatus as composed of two parts or systems—the "advisory system" and the "allocational system" (p. 41).[2] The "relevant units" of the advisory system in Jachim's scheme are: the National Academy of Sciences (NAS) and its Committee on Science and Public Policy (COSPUP); the National Academy of Engineering (NAE), which was organized in 1964 under the NAS charter but has "since developed rather independently" (p. 43); the National Science Foundation (NSF); and the Office of Science and Technology (OST), which at the time occupied a role today played by the Office of Science and Technology Policy (OSTP). Jachim offers a summary account and appraisal of each of these units (pp. 41–5). His review of the NSF, which was created in 1950 after a five-year running controversy and has since had "a rather mixed-up history" (p. 44), is of special relevance here.

As initially envisaged, an important part of the NSF's function was to have been "to carry out the work of overall coordination of federal research activity." But it became "quite clear early in the game" that neither the director nor the National Science Board, who share operational responsibility for the Foundation's activities, thought such a mission "possible or even desirable." The broader role first contemplated for it was passed up in favor of a concentration on the support of basic research (the NSF is "the only governmental agency whose budget has traditionally been viewed as essentially a basic research budget") and centralized management of research activity was likewise downplayed in favor of "informal coordination from within the social system of science itself" (p. 44).

The review of the NSF leads quite naturally to a discussion of the "allocational system", made up "of those actions (decisions) of the advisory system along with the actions of administrative operating units, Congress, and the scientific community itself" (p. 46). While "[m]ost writers would wish to distinguish between "big science"and "little science" on this score",[3] Jachim suggests that the similarities in "the nature of the commitment and the decisional rules used" in each of these contexts is ultimately a more important consideration (*ibid.*) Not only is the central premise—that "re-

---

(playing off the communications gap between the scientist and the layman by, for example, fabricating or exaggerating the potential practical benefits of basic research) and "bootlegging" (using funds made available for one purpose of support another).

[2] Jachim adds a third component—"The peculiar situation of atomic energy research and the Atomic Energy Commission"—that need not detain us here.

[3] "Big science," Jachim explains, is "thought to involve the allocation of large, expensive research facilities and manpower." "Little science," by contrast, involves "the allocation of relatively small resources to individual researchers or small groups of researchers to support projects they have proposed."

sources should be allocated to the best (i.e., most competent) researchers"—the same in each case, but those who serve on the advisory panels that choose from among grant proposals in both settings generally are distinguished researchers themselves. The principle that "the best decide what is and who are best" thus tends to prevail throughout the allocational system.

Jachim notes that "The scientific community has been able to protect its integrity fairly well . . . by insisting on autonomy in these advisory-decisional cases" (p. 46). The grant-giving apparatus in agencies like the NSF and the NIH, structured around disciplinary panels, boards, or committees on which they serve in a part-time capacity to weigh the merits of research proposals, institutionalizes the principle of autonomy while assuring leading members of the scientific community a formidable role in allocative decision-making. "In keeping with traditional American normative administrative theory, these panels are labelled *advisory*. Even in the cases that are specifically set up for the support of research, as in the [NSF], the myth continues that specialists have no authority to make decisions" and that only the agency administrators enjoy that prerogative (p. 47). The reality is quite different: "For all practical purposes, "recommendations" (decisions) of these advisory judges are usually final" (p. 48).

The nature of the "peer review" system as it operates through these advisory panels also serves to perpetuate another and closely related "myth", according to Jachim, namely that the "substance of the proposals" reviewed is the key factor in decision-making in the allocational system. Following Charles V. Kidd, Jachim suggests that the formal emphasis placed on the merit of the individual proposal as the object of collective deliberation is a "key adaptation" that science has made in order to make a selection process with decidedly elitist overtones and consequences palatable in a political culture that places a high priority on procedural fairness and egalitarianism: "If . . . the stress were overtly on the status of the individuals making the proposals or on the prestige of the organizations they represent, internal and external pressures might become explosive." In fact, however, as Kidd had observed, "the capacity of the individuals and groups . . . is given much greater weight than one would assume from the written policy statements of the federal agencies" (p. 48).

The scientific community's preoccupation with rewarding quality, which is simultaneously reflected in and reinforced by the decision-making processes that characterize the advisory and allocational systems, has important implications with respect to the overall allocation of R & D resources. In the concluding portion of his chapter, Jachim provides some striking (albeit rather dated*) evidence of the institutional and geographic concentration of scientific resources that results from those processes. A pattern of skewered distribution is discernible in industry, but it is even more pronounced in universities, research institutes, and government laboratories. Jachim notes, for example, that in 1963[4], 85% of all federally-sponsored university research was accounted for by the top 100 universities, while 50% was carried out by the top twenty-four—those in the "top prestige" rankings (p. 58). The concentration of resources for the support of basic research is even greater, generally confined to "a few of the top 24 universities" (p. 59). A breakdown of the distribution of R & D resources by geographical region reveals a parallel (p. 59–60).

---

[4] More current data bearing on the geographic distribution of R & D resources are presented (in tables drawn from an NSF survey) in Part VI of the reader. The Smith and Karlesky selection, also in Part VI, provides a more recent view of the effect on universities.

For a variety of very practical reasons, Jachim contends, the process of the concentration of R&D resources once begun "tends to perpetuate itself if allowed to go unchecked" (p. 58). "That only the best should be rewarded is a strong norm, and the best, we are told, can only be determined through the eyes of the scientific community" (p. 60). Jachim notes, however, that "the autonomy of science is argued for more strongly by those who act as formal or informal leaders than those who remain unrecognized", and that there are some obvious and important differences of perspective on distributional issues—and the "decision rules" developed to deal with the internal conflicts—on the part of the "haves" and the "have nots" of science, respectively (p. 62). His account does much to explain why the allocation of federal funds for scientific R&D has been such a persistent subject of debate in recent decades.

Complaints about the federal system for the support of research have by no means been confined to the "have nots" of science. Indeed, as Donald K. Price notes in the second selection in Part V, a complex of factors—a static or even declining level of financial support, the legal and administrative "constraints" that go with government money, and a change in political attitude on the part of Congressmen and others—have lately tempted increasing numbers even from the ranks of the "haves" of science to "regret the implicit bargain on which massive federal support [of research] has been based since the Second World War" (p. 75).

Price is not unsympathetic in the face of this litany of complaints entered by the many scientists who, while "unable to swear off periodic injections of government grants", have come to "repent of their addiction" (p. 76). But his primary objective in "Endless Frontier or Bureaucratic Morass?" is to suggest that the scientific community itself is ultimately largely to blame for the various "constraints" its members, especially those doing research in basic science, have come to find so objectionable. Restrictions on their freedom to pursue their own scientifically-defined interests were, Price argues, a predictable consequence of what others grew to regard as a failure on the part of scientists to abide by the terms of the "implicit bargain" struck with federal authorities in the post-War era.

The terms of that bargain, based on the assumption (which figured prominently in Vannevar Bush's influential report of 1945, *Science—the Endless Frontier*) that basic research would lead to fruitful practical benefits, provided scientists with ample opportunity to fashion a support system responsive to their primary concerns. As he had in his essay on "The Scientific Establishment" some 15 years earlier,[5] Price emphasizes the distinctiveness of the

---

[5] Reproduced in Part IV.

American experience in this respect: "[N]owhere else did a group of scientists manage to persuade their governments and . . . research institutions not only to give up their scruples against public support of science, but also to work out arrangements for distributing the support that maximized the political influence of those whose authority was based on knowledge rather than on votes" (p. 77).

These arrangements enabled scientists to pursue a three-part tactic designed "to free basic science in several ways from the constraints inherent in the older federal research programs": combining research with university teaching (to provide a "stable basis on which to defend the independence of science"); emphasizing the project grant (appealing to scientists in part because "it offered a tactic to avoid detailed congressional control of funds"); and establishing a pattern of advisory committees that (among other things) allowed scientists to gain substantial control over the distribution of federal research funds without having to accept "the burdens and responsibilities and vulnerable status of a full-time position" in the federal government (see pp. 77–9).

As they eagerly took advantage of the fruits of their political labors, however, scientists tended to lose sight of the fact that the authority they had gained was delegated authority "which depended on the continued confidence among elected politicians" that the terms of the initial agreement were in fact being honored. That confidence, however, was badly shaken during the 1960s, and "each of the three tactics that had given the scientific community such status after the Second World War came to be looked on with cynical suspicion by government administrative officers and members of Congress alike" (p. 80).

The "constraints" about which universities and scientists today complain, Price suggests, were mostly the result of "liberal reforms and egalitarian pressures" that came on the heels of the agitation of the 1960s. In the middle portion of his article, Price analyzes "The Purposes of Constraints on Research" under three headings: "Regulatory Red Tape" (pp. 81–2), "The Attack on Elitism" (pp. 82–4), and "Control of the Substance of Research" (pp. 84–6). The discussion of the attack on elitism, in which he addresses the fall-out occasioned by the growing awareness that "[t]he project grant system, and the 'peer review' method by which it distributed funds, [had] produced a concentration of grants in a limited number of universities" (p. 82), is the most pertinent here.

Price notes with some concern that the procedures devised by the NSF, NIH, and other grant-making agencies to distribute funds more equitably (along with other policy shifts dating from the 1960s) seem to have worked to the disadvantage of leading research universities. But while his suggestion that "Excellence in fundamental research is not something that can be distributed by statute among the states and cities" (p. 83) no doubt captures the sentiments of many scientists, Price acknowledges that the concentration of federal monies in a few institutions has disturbing implications in its own right. The question of whether the number of major centers receiving federal support should be "ten or . . . fifty or a hundred" is an important and legitimate question of public policy, and "there is probably no good way to answer [it] with confidence" (*ibid.*).

On the basis of the concluding portion of his article, however, it seems fair to say that in the final analysis Price would vote to uphold the principle of scientific autonomy in allocational matters and to retain the emphasis on "excellence" as the primary if not exclusive decisional criterion. The principal theoretical obstacle in this context, he had suggested earlier, was that "no one knows how to make and defend a frankly elitist decision about the distribution of federal funds to the best universities, within the American constitutional system" (p. 83). But in his conclusion ("A Start Toward a Strategy"), Price suggests that scientists might yet prove capable of defending traditional arrangements on strictly functional grounds. Such a strategy, he suggests, "would allow scientific institutions to avoid the appearance of pleading for unique privileges of the basis of elite and esoteric status" and "put them in alliance with other elements of society which might see some common interest in maintaining a degree of autonomy in a pluralist society" (p. 90).

While he clearly appreciates the many difficulties it has engendered in the relations between scientists and government, Price does not appear prepared to renounce the notion that support of basic research should be the central objective of American science policy. The authors of the third selection in Part V, Deborah Shapley and Rustum Roy, by contrast, have no qualms about doing so. The central thesis of their recent book, *Lost at the Frontier: U.S. Science and Technology Policy Adrift,* is spelled out in the opening paragraph of the chapter reproduced here: American science in the post-War period has been dominated by a "basis-research-is-best" mindset that has served to "hinder efforts to couple science to the nation's economic and technological needs" (p. 37).

In this chapter, Shapley and Roy set out to show how this "basic-research-is-best" attitude shaped the nation's science institutions and to indicate the variety of ways in which America's post-War science policy has suffered as a result. In the earlier portion of the chapter, the authors provide an overview of institutional developments in the immediate post-War period. It is their contention that it was in the early 1950s, "when [NSF Director Alan] Waterman, the National Science Board, and the Bureau of the Budget made critical choices about the NSF and the growing federal university science system," that "the seeds of our present troubles were sown" (p. 42).

During this period, the "tone and values" of the system were set on a course that would "impede close links between science and its applications" (p. 37). Two salient tendencies emerged. First, "unmanaged diversity replaced [Vannevar] Bush's concept of a national science policy and central planning for science" (p. 43)—a system characterized by a "diffusion of government sponsors" and a correspondingly fragmented set of research constituencies was established. Second, "growth became the system's dominant mode of successful operation and perhaps its only one" (*ibid.*). The arrangements throughout the system institutionalized the notion that "science requires a certain freedom and that scientists are best qualified to run their own affairs", but this pattern of informal coordination of the research enterprise came at a price: "run from below, the system had no internal means . . . of setting priorities" (pp. 43–4).

According to Shapley and Roy, the early NSF leadership bears a heavy responsibility for having set postwar U.S. science policy on this unhappy course. Perhaps the most "critical choice" they made was in "consciously decid[ing] to play a more limited role" than their mandate allowed by declining to assume responsibility (as Bush and others had hoped and expected) for the overlall coordination of federal research activity. By claiming for the NSF "a unique role only in supporting undirected basic research", they "opened the first rift in what became a gulf between basic research and the rest of the enterprise" (p. 40).

Even in the more limited sphere they carved out for it, however, the early NSF leadership had to confront some basic questions about the foundation's role: "Was it to support only elite institutions? Or should it . . . distribute its money geographically to develop top-quality institutions where there had been none?" *(ibid.).* In deciding, in effect, how to define relations between the foundation and emerging constituency, the NSF leadership would impart important messages of "tone and value" to other agencies whose research-sponsoring programs and activities it had declined to oversee directly.

In this context, Shapley and Roy suggest, the early NSF leadership made another "critical choice." NSF policy "was deliberately left in the hands of the university scientists, mostly through peer review" (p. 44). Both the management style ("passive, scientist-oriented, and discipline-oriented") and the substantive orientation (science policy "meant most of all more money" for the support or basic research in their disciplines) of early NSF program managers betrayed tendencies that were to become ingrained features of the system as a whole. "Basic-research-is-best came to dominate the thinking of university people seeking funds, as well as research managers in government giving them out" (p. 49), and "the result has been to reinforce artifical subdivisions of science and put funding officials and university departments in lockstep" (p. 56).

Although it had a variety of disturbing consequences and implications, the problems inherent in the U.S. science system only became apparent to those who supported or profited from it during the 1970s, when "the decline in real dollars going to university research" exposed some serious "structural flows" (p. 52). In their brief review of one of those structural flaws—"weak management"—Shapley and Roy call attention to problems in the peer review system. Earlier they has suggested that the economic prosperity of the 1960s had served to deflect attention from one such problem: "Although in the early days there was so little money that only the elite institutions passed normal reviews", later it became possible to "spread the money around and [to try] to develop research excellence in institutions previously unable to compete" (pp. 44–5). But as resources for the support of research began to level off in the 1970s, the limitations of the peer review system, which (among other things) "provides no mechanism for judgment calls among subfields, or setting priorities among disciplines" (p. 55), became more readily apparent. The fact that NSF program officials continue to exhibit a "passive view of management's role in science" (*ibid.*). in the face of these sorts of problems is but one of the many indications that "the U.S. science system, its structures and values have remained as unchanged as if we were still living in the 1960s" (p. 52).

B. PATTERNS OF EVALUATION AND DECISIONAL CRITERIA IN AMERICAN SCIENCE

As the selections summarized above, as well as numerous other discussions of the American science advisory system, attest, the widespread use of the peer review method as a way of funding science and providing scientific accountability to government and to the public at large has not gone unquestioned. The readings in the next section of Part V focus on the controversy that has surrounded the peer review system, with special attention to the National Science Foundation, which found itself at the center of a intense debate that raged during the mid- and late-1970s—a debate whose reverberations are still being felt today.

The report published by the National Commission on Research (NCR) in May, 1980 entitled *Review Processes: Assessing the Quality of Research Proposals,* is included here principally to help place the dispute surrounding NSF peer review in context. The report describes variations in the types of evaluative procedures used by six federal agencies (NIH, NSF, DOD, DOE, USDA, and NASA) that together provide almost 90 percent of the federal funds expended for research in universities (pp. 3–5 and Table I on p. 6) and usefully summarizes the range of criticisms that have been directed against the peer review system as a whole. Whether at the hands of representatives of Congress, the federal agencies, the university community, or the public press, peer review has been challenged on the grounds that the system: fosters undue secrecy and possible conflicts of interest; discriminates against investigators on the basis of age; results in an inequitable geographic distribution of federal research funds; and institutionalizes "cronyism" and "elitism" (see p. 7).

The NCR report, however, is not only descriptive in nature, and its appraisals of and recommendations regarding the current peer review system are likely to be of interest. The Commission takes the position that there are "built-in safeguards against some of the problems" cited in the preceeding paragraph—conflict of interest, for example, is "minimized by regulations that reviewers shall not evaluate their own proposals or those from colleagues in their own departments and universities", and chances for "cronyism" are lessened by the broad range and regular rotation of panel members (pp. 7–8). "[T]here has been no documentation" to show that age discrimination, bias or cronyism "are, in fact, major problems." As for allegations of "traditionalism and conservatism" among peer reviewers, of an "unwillingness to recognize and recommend support for innovative, high-risk proposals", and of inadequate review given to proposals of an interdisciplinary character, the Commission suggests that while these criticisms "cannot be set aside altogether", they are "not perhaps problems to be solved, but dangers to be avoided . . ." (p. 8).

In light of these (and other) point-by-point appraisals, it is hardly surprising what while acknowledging it "is not immune to faults", the Commission concludes that the peer review system has generally "worked well." Most agencies "have been attentive to developing and testing appropriate safeguards to ensure its integrity and effectiveness" (p. 13). The report recommends, however, that their review processes "be examined periodically, both by internal and external groups", that the agencies themselves initiate retrospective studies to assure their effectiveness, and—perhaps most importantly—that they "make explicit the nature of the underlying scientific and policy considerations used in arriving at funding decisions" (pp. 15–6).

Many of the general claims made on behalf of the system as a whole in the NCR report appear to be substantiated in the first of two following selections that focus specifically on peer review at the NSF. In their 1977 article, "Peer Review and the Support of Science", Stephen Cole, Leonard Rubin, and Jonathan R. Cole present in summary form their principal findings and conclusions from the first phase of a two-part study conducted for the National Academy of Sciences entitled *Peer Review in the National Science Foundation.*[6] They note early on that their efforts "yielded little evidence in support of the main criticisms . . . made of the peer review system. On the contrary, we have tentatively concluded that the NSF peer review system is in general an equitable arrangement that distributes the limited funds available for basic research primarily on the basic of the perceived quality of the applicant's proposal" (p. 34).

After briefly describing the focus of their study (exclusively on 10 NSF programs responsible for funding basic research) and the "qualitative and quantitative sociological techniques" employed, the authors set out to indicate how and why they arrived at their general conclusion. Their principal aim was to assess the validity of two hypotheses used to support the "most fundamental" criticism of the NSF peer review system—that it generally leads to inequitable decisions, and specifically that "eminent scientists have a great advantage in the competition for funds and that less eminent scientists, particularly younger ones, are at a serious disadvantage" (pp. 34–6).

The first hypothesis, the "old-boy hypothesis", focuses on the organizational conditions that purportedly give rise to an unfair distribution of support. The claim here is that NSF program director is "at the center of [an] old-boy network in which reviewers favorably evaluate the proposals of their friends, eminent scholars favorably review the proposals of other eminent scientists, and funds are denied to scientists who are not part of the exclusive old-boy network" (p. 35). To find out whether program directors can predetermine the outcome of the process by "working" the "old-boy network", Cole *et al* set out to determine whether "the proposals of eminent scientists [are] actually rated more favorably by eminent scientists than by other reviewers", gauging the "eminence" of applicants and reviewers "according to the prestige of their current department" as determined by a 1969 survey (p. 36). Their principal finding: "in general reviewers from high-ranked departments were not disproportionately favoring proposals from applicants in similarly high-ranked departments" *(ibid.)*. Their data disclosed "no significant tendency" on the part of eminent scientists "to favor the proposals of eminent scientists" over those of the less eminent, and the authors suggest that this renders suspect the claim that NSF program directors are able to "manipulate" the outcome of the peer review process (on the "unlikely" assumption that they desire to do so in the first place) in the manner alleged (p.37).

---

[6] "Phase One" of the study was published by NAS in 1978, "Phase Two" (authored only by the two Coles) in 1981.

In order to assess the second or "rich-get-richer" hypothesis, which stipulates that eminent scientists at high-ranked departments have an unfair advantage in grant approval "regardless of the quality of proposals", Cole *et al* focus on the question of how the characteristics of individual applicants affects their peer review ratings. (How much weight is attached to these "subjective" factors, the authors suggest, has an important bearing on the validity of the "old-boy" hypothesis which, however, lacks the "conceptual clarity" necessary to permit a thorough examination of the theory.) The authors ranked applicants' status in the social system of science according to nine distinct variables, ranging from where their doctorate was obtained and their current academic rank and department to the number of citations their published work received. "The rich-get-rich hypothesis would suggest the existence of strong correlations between all these variables and the ratings applicants received on their proposals." The results, however, showed only weak or moderate correlations—the most highly correlated variable, the number of citations to recently published work, explained only 6% in the variance of ratings—and on the whole "provide[d] little support" for the hypothesis in question (p. 38). Further inquiry, the authors report, also suggested that reviewers were "not strongly influenced by the professional status of the applicant" but rather and simply "by their perception of the quality of the research proposed" (p. 39).

In short, Cole *et al* regard the results of their quantitative analysis as providing a near-conclusive refutation of the principal charges against the NSF peer review system. To deflate the claims of critics, it remains only to cite evidence that "program directors . . . rely heavily on the evaluations of the peer reviewers" *(ibid.)*, and hence do not exercise their prerogative with respect to final funding decisions in a biased manner. A breakdown of the characteristics of those scientists who received NSF grants in 1975 indicates that there are indeed notable differences in outcomes at the aggregate level—that scientists with, for example, an established track record and ties to a prestigious department "have a higher probability of receiving . . . grants than other applicants do." "Nevertheless", the authors insist, "the granting process is quite open, and there is nothing approximating a scientific caste system" (p. 40). While it might provide limited support for the notion that a process of "accumulative advantage" is at work in the NSF peer review system, the results of their study suggest that on the whole "the scientific enterprise is an exceedingly equitable, although highly stratified institution, in which [those whose work] is most favorably evaluated by their colleagues receive the lion's share of the rewards" (p. 41).

The Coles and Rubin plainly hoped that the results of their study would definitively lay to rest the controversy surrounding NSF peer review. But that hope has not been entirely realized. Indeed, their study and conclusions are challenged on both methodological and substantive grounds in the course of the next selection, "Peer Review at the NSF: A Dialectical Policy Analysis," by Ian I. Mitroff and Darryl E. Chubin.

While they emphasize at the outset that "we are neither anti-science nor anti-peer review", Mitroff and Chubin suggest that "the tenacity of certain values which undergird the very institution of science, precluding systematic investigation of mechanisms by which the institution's autonomy, self-governance, and 'uneasy partnership' with government is maintained", continue to be reflected in assessments which the scientific community itself has offered of the peer review system (p. 200). "The mode of presenting the peer review debate" does much to disclose how that debate is conceived, and it is the author's contention that previous studies have not adequately recognized that in this context (as in other controversial areas of science) "the failure to secure agreement is not because [the] issues inherently defy treatment or analysis, but because various parties, due to their respective social, intellectual, and/or value positions, will perceive the issue in very different ways" (p. 202). The primary purpose of the "dialectical policy analysis" Mitroff and Chubin offer of NSF peer review is, first, to make "as explicit as possibile" the fundamentally different (indeed, "maximally opposed") "background assumptions" of the contending parties to the debate, and second, to critically review the evidence adduced in prior studies in light of this enlarged perspective (pp. 200–2).

Mitroff and Chubin provide a "dialectical representation of the current system of NSF peer review" in Table 1 of their article (pp. 203–4), summarizing the basic assumptions and contentions of those "pro" and "con" on a range of issues that have figured prominently in the debate (e.g., whether the system as a whole is open or biased, whether program managers can "manipulate" the review process, whether NSF should fund more or less research at prestigious universities, etc.). They caution that the intensity of the differences between the two sides to the debate (as reflected both in the table and in selected quotations) should not be allowed to obscure the fact that both sides agree on a number of significant propositions, including, most saliently, that no method superior to peer review as a method for judging the scientific competence of proposers has been found (p. 206). But Mitroff and Chubin emphasize that "substantial agreement on these propositions makes them no less than true or false . . . than the propositions over which there is explicit disagreement. In either case, beliefs may be so deeply held by their proponents that, at least for them, they may be immutable and irrefutable—utterly impervious to evidence. . . . What evidence, if any, exists or could be produced that bears on the various propositions and could alter the beliefs of proponents and critics alike? (p. 207).

It is with this question in mind that Mitroff and Chubin turn their attention to a review and appraisal of the data and facts marshalled in the 1977 article by Stephen Cole and his colleagues.[7] Their reaction: "We think the Cole study begs several questions that are vital to the peer review debate. To compound this error, the authors make some definitive-sounding extrapolations that seem unjustified by their [findings]" (p. 214). The latter problem is particularly manifest, Mitroff and Chubin suggest, in the claim of Cole *et al* that "the peer review rating is by far the best predictor"of the probability or receiving a grant. The acknowledged fact that 89% of the variance of the observed ratings was left unexplained by the nine "social stratification" variables on which they relied would seem to indicate that other factors were at work, but Cole and his colleagues made no effort to explore (or even to speculate as to) what those factors might be. Thus the conclusion, that individuals whose work is most favorably evaluated by their colleagues receive the preponderance of the rewards, "circumvents the questions of why their work is favorably evaluated" (p. 214).

The critical lacunae in the Cole study (also evident, Mitroff and Chubin suggest, in the lack of attention given to the question of where in the distribution of peer review ratings "subjective" considerations tend to prevail[8]) is the failure to consider the effect of the more elusive, less-easily-measured characteristics of applicants and/or reviewers which might influence the final decision to fund or not to fund. Until some kind of experiment is made to test the hypothesis, "the Cole data and analysis cannot be used to ignore or deny the relevance of personal or cognitive attributes in the operation and understanding of the peer review system" (pp. 215–6). While placing primary emphasis on this important methodological/substantive limitation, Mitroff and Chubin "feel compelled" to add to their criticisms of the Cole study that despite claims of complete autonomy from the NSF in the conduct of their research, the authors betray "a commitment to show that even where peer review is not equitable within NSF, the inequity is for the good of science" (p. 216).

In the latter portion of their article, Mitroff and Chubin offer some general reflections on the peer review debate in light of their appraisals of the Cole and Hensler studies. (An assessment of the status of the evidence those studies bring to bear on the various issues in this context, as previously outlined in Table 1, is presented in Table 3, pp. 202–1.) They also map out a preliminary strategy for further research that might provide the data needed to advance that debate beyond its present impasse. It is their contention that the "missing link" is the "need to measure the cognitive styles and backgrounds beliefs of participants in the peer review process" (p. 218). Mitroff and Chubin suggest that the entire controversy "calls for treatment of the issues from more than one theoretical pont of view" and that the "cognitive styles" and "background beliefs" of participants might usefully be analyzed in terms of three models that can be said to undergird the peer review debate, which they label the "Accumulative Advantage Model", the "Political Model", and the "Merit Model" (pp. 219–223). Each of these models holds forth a distinct vision of what factors are most important in determining the outcome of the peer review process, and adherents of each would tend to have a different view of how participants do or should attempt to bridge the tension between "objective" and "subjective" decisional criteria that is inherent in the reviewer role. "Because each, in all likelihood, is partially correct, future studies must establish, for example, under what circumstances each model obtains. What we are advocating is a testing of the alternatives—new data collection and analysis—to expand the empirical base that impinges upon and must eventually mediate the debate" (p. 224).

---

[7] Before turing to the Cole article, Mitroff and Chubin focus on another study of NSF peer review conducted by Deborah Hensler and published by NSF in 1976 (see pp. 207–12).

[8] Mitroff and Chubin note that since performance or "track record" is supposed to be an explicit factor in assessing proposals, attention to the particular attributes of the proposer "has been institutionalized as a (partial) rationale" for making funding decisions at the NSF. The tension between "objective" and "subjective" consideration or criteria is thus "built into the peer review process. Why deny this fact, as Cole seems to do? And why provide no data (for example, from interviews) which might contain clues about the tension—namely, the extent to which perceptions of quality are coloured by particularistic considerations?" (pp. 214–5).

The formal studies of the operation of the NSF peer review system conducted by Stephen Cole and colleagues and by Mitroff and Chubin, as well as the broader assessment of review processes initiated by the National Commission on Research, were all undertaken against the backdrop of a series of hearings on the NSF held before the House Subcommittee on Science, Research and Technology beginning in July of 1975. The sharp criticisms of NSF peer review delivered during the course of those hearings by certain members of Congress, notably by former Representative John B. Conlan (R-Ariz.), as well as a subsequent General Accounting Office (GAO) report based on a two-year investigation of NSF's disposal of grant applications, attracted a good deal of attention in the popular press and in the publications of various scientific associations. Five brief reviews and/or commentaries that appeared in three such professional journals (*Science, Chemical and Engineering News,* and *Scientific American*) between February 1976 and December 1978 have been reproduced in the next section of Part V to help situate the more systematic appraisals in the preceding section in their broader context.[9]

The factors giving rise to the controversy surrounding the operation of the peer review system at NSF (as well as at other research-sponsoring agencies), while often apparently context-specific in nature, both reflect and serve to illuminate some of the general problems involved in attempting to formulate appropriate criteria to guide decision-making with respect to the allocation of government funds for science. The selections in the next section of Part V, by Alvin Weinberg, Michael Reagan, and Harvey Brooks, respectively, provide an indication of how some prominent spokesmen from the scientific community have endeavored to deal with this issue at the broader or "macro" level.

As Alvin Weinberg suggests at the outset of his classic 1963 article, "Criteria for Scientific Choice," at the general or system-wide level, two types of choices must be confronted in attempting to decide how best to allocate support for science whenever total demand exceeds the available resources. The first type of choice involves deciding "among different, often incommensurable fields of science—between, for example, high energy physics and oceanography or between molecular biology and science of metals", the second "among the different institutions that receive support for science from government—among universities, government laboratories and industry" (p. 159). Weinberg's concern in this article is with what he terms "the broadest" of "scientific" (as opposed to "institutional") choices: "how should government decide between very large fields of science, particularly between different branches of basic science?" His purpose is to formulate a set of criteria or scale of values that might help establish priorities to guide decision-making in this context.

---

[9] Readers might be particularly interested in the third and fourth selections, which provide insight into the reactions of then-President of the NAS Philip Handler and former Representative James W. Symington (D, MO.), one-time chairman of the House Subcommittee on Science, Research and Technology, respectively, to the debate and studies inspired by the 1975 hearings.

Weinberg warms to his task by briefly reviewing the "institutional and other devices" developed (as of the early 1960s) by the scientific and governmental communities "for coping with broad issues of scientific choice" (p. 161). He devotes special attention to the panels of the President's Science Advisory Committee (PSAC) in the Office of Science and Technology (OST), which at the time helped the Bureau of the Budget decide what was and was not to be supported. The panel system, Weinberg suggests, suffers from a "serious weakness": it "usually consists of specialized experts who inevitably share the same enthusiasms and passions." Though well-qualified to assess how competently a given proposal within their field of expertise is likely to be carried out, members of these panels exhibit "parochial viewpoints" that render them "much less able to place the proposal in a broader perspective and to say whether [it] is of much interest to the rest of science" (*ibid*).

The basic limitation of the PSAC panel system illuminates the primary problem Weinberg wishes to call attention to, namely that "it is not tenable to base our judgments [of scientific choice] entirely on internal criteria", that is, on criteria that are generated and/or applied only within a particular scientific field (p. 163). Experts in a given scientific field or discipline are uniquely qualified to assess whether that field is "ready for exploitation" and whether particular scientists working in it are "really competent." But these internal criteria—"the ones most often applied when a panel decides on a research grant"—are not necessarily the most important considerations from the broader societal perspective. Society as a whole has a legitimate interest in the question, "Why pursue this particular science" in the first place? Neither the suggestion that "the pursuit of science as such is society's highest good," nor a reliance upon the "internal criteria" that operate within each field, permits a truly informed answer to this question. "Thus, in seeking justification for the support of science, we are led inevitably to consider external criteria for the validity of science—criteria external to science, or to a given field of science" (pp. 163–4).

Weinberg identifies and seeks to make "more explicit" three "external criteria" that are relevant to scientific choice: technological merit, scientific merit and social merit. The first of these is comparatively less problematic ("we have more practice discussing technological merit" and technological aims are "customarily scrutinized more closely" than are scientific aims), and its implications are relatively clear: "once we have decided, one way or another, that a certain technological end is worthwhile, we must support the scientific research necessary to achieve that end" (p. 164). "The criteria of scientific merit and social merit are much more difficult: scientific merit because we have given little thought to defining scientific merit in the broadest sense, social merit because it is difficult to define the values of society" (*ibid.*). Weinberg had already suggested, however, that the scientific merit of a given field "can be judged better from the vantage point of the scientific fields in which it is embedded than from the point of view of the field itself." One valid measure of the scientific merit of a field of basic science, in fact, is its relevance to other fields of science, and this provides a working principle that can inform allocative decision-making: "A field in which lack of knowledge is a bottleneck to the understanding of other fields deserves more support than a field which is isolated from other fields" (p. 165). A "sharpened" formulation of the criterion of scientific merit, Weinberg later proposes, yields the following: "other things being equal, that field that has the most scientific merit which contributes more heavily to and illuminates most brightly its neighbouring scientific disciplines" (p. 166).

Social merit, or "relevance to human welfare and the values of man", is clearly "the most controversial criterion of all", beset as it is by the difficulties inherent in (1) determining who is to define the values "even . . . of our own society", and (2) "deciding whether a given scientific or technical enterprise indeed furthers our pursuit of social values, even when those values have been identified" (*ibid.*) Weinberg notes, however, that some broad candidates—"adequate defense, or more food, or less sickness, for example"—are "rather uncontroversial", and the concluding portion of his essay, in which he endeavors to assess five different scientific fields in light of his three external criteria, suggests that "social merit" is not inevitably so elusive and difficult a criterion as to defy meaningful application when attempting to assign relative priorities for support purposes.

The five fields Weinberg proposes to assess—molecular biology, high-energy physics, nuclear energy, the behavioral sciences, and manned-space exploration—are "incommensurable" (i.e., they appear not to admit of evaluation "on the same scale of values"), but the selection is deliberate: "[C]hoices between scientific fields will eventually have to be made whether we like it or not. Criteria for scientific choice will be most useful only if they *can* be applied to seemingly incommensurable situations" (p. 167).

The results of his individual appraisals of each field indicate the potential utility of Weinberg's criteria for establishing priorities in this context. A comparison of the two fields that are, "by any definition, basic science"—molecular biology and high-energy physics—yields strikingly different results. The former well-satisfies all the external criteria (molecular biology is "profoundly important for large stretches of other biological sciences" and deserves high marks in social and technological ("that is, medical") merit because it promises to provide insights "necessary for the solution of such problems as cancer, birth defects, and viral diseases"), but the latter rates poorly on all three counts ("few discoveries in ultra-high-energy physics . . . bear strongly on the rest of science", and the contributions of the field to human welfare and on technology are "essentially nil"). Hence, while molecular biology deserves strong public support, the justification for underwriting the "terribly expensive" high-energy physics enterprise is relatively slim.

Even in the more difficult cases when assessments in their light do not (as in the first two instances) all point in roughly the same direction, the various criteria can serve an important function in helping to decide whether the trade-offs involved are such as to warrent support of the field or not (and if so at what level). As regards nuclear energy, Weinberg concludes that the field "rates so highly in the categories of technical and social merit and timeliness" as to overcome "its very low mark in two other categories—its personnel and its relationship to the rest of science." Manned-spaced exploration, by contrast, is so remote from human affairs, "not to say the rest of science," that large expenditures of manpower and money do not on balance appear to him to be justified (pp. 167–70).

Needless to say, neither Weinberg's individual appraisals of the relative merit of each of the fields nor the recommendations that follow from his assessments would necessarily command assent from all quarters. While he well appreciates that the attempt to apply his set of criteria will inevitably produce "difficulties in practice" (p. 170), Weinberg's overriding message is that the problem of scientific choice cannot be ignored: "It is as much out of a prudent concern for their own survival, as for any loftier motive, that scientists must acquire the habit of scrutinizing what they do from a broader perspective than has been their custom" (p.171).

Weinberg's essay was destined to leave a powerful imprint on subsequent scholarship on scientific choice. Early on in chapter 10 ("An Approach to the Priorities Problem") of his 1969 treatise *Science and the Federal Patron,* for example, Michael Reagan notes that "Although the advancement of the public welfare is the ultimate ground upon which public support of science rests, we built for some years a protected enclave in which all but criteria of scientific merit were firmly excluded" (p. 273), attesting to the seriousness with which Weinberg's basic warning came to be regarded by many in the scientific community. Time and again in the introductory phases of the chapter, Reagan reiterates Weinberg's main theses: "hard choices will have to be made henceforth both in the development of science and in the application of science to social-technological problems" (p. 272), and in making those choices "non-scientific values and interests are legitimately to be considered along with scientific criteria" (p. 276).

Reagan in fact readily acknowledges his debt to his predecessor, whose article reflects what he terms "a politically realistic view" of the need for "a mixture for scientific and extra-scientific considerations" in establishing priorities for science (p. 275). He suggests, however, that in attempting to develop "a set of criteria to apply to the entire spectrum of R&D, from high-energy physics to the manned space program, Weinberg was perhaps overly ambitious." His own approach to the priorities problem, by contrast, proceeds on the assumption that "there can be no single set of criteria across the board; that we must categorize different areas and develop both substantive criteria and decision-making mechanisms appropriate to each case" (*ibid.*). Expressing the hope that his efforts will provoke others "to correct, amplify, or replace what I say," Reagan accordingly ventures to suggest "a set of categories for priority decisions—a set of categories that also constitutes a ranked priority list for R&D support—and to comment on some of the considerations that an allocations system will have to grapple with" (p. 278).

The four-fold scheme Reagan proceeds to offer—in which first priority goes to "those social objectives which are defined as most urgent politically", second to "science-related educational needs," third to "undirected small scale research" ("little science"), and fourth to "Big Science"—embodies the "fundamental premise that human needs of an urgent, even if elementary level, . . . provide a more fundamental guideline determining what fields to support than do the needs and opportunities of the scientific disciplines considered in themselves" (pp. 278-9). Criteria of social merit accordingly figure most prominently in the first two categories and scientific criteria in the latter two, but the general difficulty of striking a proper balance between extra-scientific and scientific considerations (and the closely related problem of finding an "appropriate mix of those making decisions") will inevitably be reflected within each.

Reagan devotes relatively less attention to the first priority of "urgent social objectives that require science for their achievement." He notes, however, that "our system of technology planning does not always [lead] us to emphasize what we . . . know to be most urgent", and that this impedes the process of defining "social merit" (p. 281). "Scientific criteria" have assumed perhaps too much prominence in this context, albeit largely by default ("we seem to do what the state of the art tells us will be possible, following the path of least social resistence") and because the President and the Congress have not generally provided adequate leadership "in analyzing choices open to us and in proposing social priorities" (pp. 281-2). In principle, however, it should be possible to draw on the help of scientists in arriving at "more conscious appraisals of what we want from science and technology" in meeting important social objectives while simultaneously ensuring that their influence both with respect to setting priorities and in making allocational decisions are kept within reasonable bounds (pp. 282-3).

The problem of ensuring that "scientific criteria" narrowly construed do not overwhelm considerations of "social merit" becomes more pressing with respect to the second priority, meeting science-related educational needs. Reagan argues that while scientists claim to place a high value on the training of future scientists, science education has in fact been subordinated to research support in their overall scheme of values and "demands attention as a more autonomous goal" (p. 283). "The view that . . . assumes that science education needs will be taken care of if all individual university-based project applications are funded" (implicit in the thinking of most researchers and apparent even in major studies devoted to science-related educational needs) is "a myopic one" (p. 286). Yet it pervades the allocational system, resulting in a skewed distribution of educational resources. Reagan notes that while the guidelines used by the NSF in evaluating proposals purport to incorporate reference to a proposal's potential to contribute to instructional programs, "one gets the impression that these ancillary educational criteria are of very minor importance in the evaluative process" (p. 287). To ensure that science-related educational needs are not downplayed out of a selfish concern to "preserve the purity of the project system", "the educational aspects [must] be placed on a par with the scientific ones." Perhaps educational support "should be considered as a separate category to be handled by other types of programs"; at any rate, the connection between research and education needs to be explored "more explicitly and carefully" (pp. 288–9).

Allocational problems loom even larger with respect to the third priority, support for undirected small-scale basic research. After rejecting three different possibilities for determining "an independent total figure for basic research" (see pp. 290–2), Reagan suggests that the process of setting priorities for the support of basic research might "proceed from field-by-field analyses of needs and opportunities; from Weinberg-type criteria questions; from proposal pressures; and from higher education institutional needs" (p. 293). As this list indicates, Reagan envisages a system in which the various components of the basic research budget would be qualitatively weighted primary with reference to scientific criteria, but also in light of "extra-scientific" considerations. Assuming a workable formula could be arrived at, priorities might first be set within each field by panels organized by the relevant agency (e.g., NIH would oversee assessments of proposals in biomedical science) and be further refined through inter-field reviews to compare "the quality of marginal proposals in each field." Finally, the results of the marginal assessment process would be embodied in an overall basic research plan for final review at a higher level in the executive branch, where final funding decisions could be made (pp. 293–5). "An approach such as this would seem to constitute a way of striking a reasonable balance between agency autonomy and over-centralized administrative priority setting" (p. 295)—of avoiding, in other words, the threat that "scientific" criteria might utterly overwhelm non-scientific considerations, or vice versa, in the process by which needs in basic research are assessed and funds distributed.

High stakes are involved in decisions falling within the fourth of Reagan's categories. "The assumption that a major scientific installations will in some (almost mystical) way contribute greatly to a region's economic growth . . . leads to a vigorous political struggle over who will get which Big Science facility" (pp. 297–8). All in all, however, "the choices here perhaps involve a more manageable number of variables than in the complexities of little science" (p. 298). In this context, while decisions involve saying "no" to whole fields in a given year and setting inter-field priorities hence looms as a "more urgent problem", "the integrity of the institutional process by which decisions are made, and in particular the assurance that all affected parties are represented, becomes more important than the [substantive] criteria themselves" (p. 299). The decision-making framework for Big Science "must be more tightly knit than for little science", and attentiveness to "management, budgeting, and administrative politics, as well as science itself" is important to ensure that allocative choices "are not limited to scientific considerations as such" (*ibid.*). While decisions in this context are particularly liable to provoke controversy, Reagan suggests that a sense of perspective must be maintained. The fact that Big Science gets lowest priority in this scheme is justified by virtue both of "its cost in relation to little science" and because it is "less closely involved in the educational function of science support" (p. 301).

The concluding paragraph of Reagan's chapter speaks for itself: "The reader who rejects the priority order given to the four categories may still find the categories themselves useful in organizing thought about the priorities problem. This is certainly the most intractable problem in federal science policy, one that no one has wanted to face. It has only recently . . . begun to receive the attention it demands. Much more hard thought by both scientists and administrators will be needed before a satisfactory priority system is developed" (p. 302).

The concerns expressed by Reagan have continued to attract the attention of scholars in the years since, as is apparent in the closing selection in Part V, Harvey Brooks' "The Problem of Research Priorities (1978)." Indeed, as Brooks notes early in this article, a number of developments in the intervening period—an overall decline in resources available for the support of scientific R & D, public disenchantment with what he terms the "military-space-atomic energy syndrome of the past quarter century", and a sense that scientists themselves have been badly remiss in their responsibility in this context—have conspired to make the task of setting scientific priorities a matter of increasing urgency (pp. 170–2). The political dimensions and implications of the problem have attracted attention on an unprecedented scale: "One of the central issues of the growing debate about scientific priorities is who should be involved in the process, and how? To what degree should the scientific community itself have central responsibility for the determination of the broad strategy of science as well as its more detailed tactics? Increasingly, there has been a debate between the public and its representatives on the one hand, and leaders of the scientific community on the other, as to where the line between strategy and tactics should actually be drawn" (p. 173).

The prevailing system of priority-setting for science, Brooks notes, has been a cause for dismay and concern among all parties concerned. On the one hand, "the scientific community perceives that the strategic decisions about science are governed by budgetary and economic considerations and by political appeal, and that the views of working scientists receive a hearing only at the margins." But while scientists may be chagrined to see "hundreds of millions of dollars committed to technological spectaculars such as Apollo or the SST, while major scientific facilities are underutilized for lack of a few million dollars of operating funds," political administrators often chaff at strategic choices made by scientists serving on study sections and advisory panels at places like the NIH (whose biomedical research program involves similarly vast sums) "on the basis of criteria which . . . are governed more by the internal logic of scientific inquiry than by the possible use of the results." From the the point of view of many politicians and members of the general public, as well as some "nonestablishment" scientists, "the priority setting processes of science [are] dominated by self-serving elites oblivious to democratic accountability and larger human concerns" (p. 173).

In this broad-ranging article, Brooks explores the array of factors (institutional and otherwise) that have shaped the current priority-setting system and the political debate surrounding allocative decision-making in science. In the first section ("The Total Resources of Science and Technology"), he sets the context by addressing the problem of the allocation of scientific resources at its most general level. Brooks lays out two alternative models that might be use in an effort to develop a coherent approach to the "one of the central issues" of science policy: "what proportion of the national product should be invested in R&D, and how this should be divided among basic research, applied research, and development" (p. 174). Under the first or "manpower" model, scientific and technical manpower would be treated as the scarcest resource, and "[s]pending on research and research and development, or possibly on various subcategories of R&D, would . . . be based on the amount of talent in each category" (*ibid.*). In a manpower allocation model, "federal R&D expenditures would serve primarily as a 'balance wheel' to private R&D. Total federal support would be adjusted gradually to bring the total technical man years of effort to some target value related to the total number of trained people available. The problem of priorities would then consist of the selection among competing projects that could be carried out with the residual pool of manpower not fully absorbed by private industrial R&D" (p. 175). After reviewing some of the arguments against an allocation-by-manpower model (which, he notes, "corresponds rather closely with the practice in the Soviet Union"), Brooks concludes that it has "several attractive features, but runs a large risk of inflexibility" (p. 176).

In any event, "the present de facto policy in the United States is that the scarcest resource is not technical talent but discretionary dollars in the federal budget" (p. 176). The present practice is, however, a far cry from what would be entailed by a consistent adherence to the second or "fiscal model" of aggregate R&D expenditures, which implies a global R&D budget in which resources would be suballocated in such a way as to maximize return on investment as a "single-dimensional criterion of merit." No such coherent or coordinated effort is now made to determine R&D priorities and how resources should be allocated. Instead, in the prevailing system, "the trade-offs tend to be made on a sector-by-sector basis rather than overall; thus defense R&D competes with defense expenditures in general; agricultural R&D with the Department of Agriculture expenditures in general, and so on" (*ibid.*). If the manpower model sacrifies flexibility for the sake of stability, the current practice in the U.S. suffers from the opposite tendency. Brooks concludes this section by noting that there is a "growing consensus" within our scientific community that "a better compromise" between these two values is "possible and desirable" (p. 177).

The obstacles that lie in the way of a more coherent R&D policy for science are considerable, however. In the next section of his article, "Truth Versus Utility", Brooks focuses on what are arguably the most difficult problems in this context—the often-opposed perspectives of the scientific and governmental communities as to the proper criteria to be used in establishing research priorities, and the limitations of the peer review system as a method of setting such priorities among or across various scientific fields.

While there is an inherent tension between the point of view of the federal government (which "supports research for essentially pragmatic reasons—because it is an important means by which agency goals can be achieved more effectively or more efficiently") and the scientific community's insistence on the need for a measure of internal autonomy in setting research priorities, Brooks suggests that there has in fact been a broad meeting of the minds on this score. "By now, most politicians conceed the necessary for some degree of self-governance and internal agenda setting in the scientific community, and most scientists concede the necessity for some political and public input to the setting of the general directions and goals of scientific research. The issues are where the lines should be drawn and the appropriate processes by which the scientific and political communities should mutually negotiate the scientific agenda" (pp. 177–8).

Much of the dissatisfaction with the way the scientific community goes about setting that agenda has focused (as we have seen) on the peer review process—"the institutional expression of science's need for autonomy." In Brooks' view, "the legitimate controversy over the merits of peer review applies not to its validity in rank ordering projects or scientists within a field,[10] but to its border use in the formulation of science policy and the allocation of resources *between* fields of research. Peer review is a process which operates must successfully . . . in the "truth" dimension and not the "utility" dimension of scientific choice" (p. 178).

Although the process is inherently ill-suited to the task, peer review has come to assume importance in the setting of priorities among fields because "it is frequently used as a signalling mechanism to indicate to program administrators where the most significant opportunities lie within a much broader domain of science" (p. 179). In Brooks' view, there are number of problems in relying upon "proposal pressure"—which represents "what the scientific community has pluralistically decided is most worth doing"—as an indicator of scientific opportunity. "[T]he mere announcement of the availability of certain kinds of research will attract proposals", and there is a general "tendency to reflect momentum generated in the system by previous actions." Proposal pressure is often difficult to interpret in a comparative light "when projects differ greatly in size or require coalesence of the activities of large groups of people" and because "the sociology of research tends to vary from field to field" (pp. 179–80). All of these considerations, Brooks suggests, combine to make proposal pressure "an imperfect indicator of the grass roots priorities of the scientific community" and one that "cannot be used as the major guide to the allocation of resources, especially among widely different fields." The obstacles are not insurmountable—"When interpreted by scientific administrators with good judgment and wide knowledge of both the substance and the sociology of the scientific enterprise, [it] can be better indicator than might be supposed"—but the limitations of proposal pressure in the priority setting process are significant and must be borne in mind (p. 180).

---

[10] Brooks' discussion proceeds on the assumption that "peer review does intend select the 'best science', and that there exist more or less universalistic criteria of scientific merit which any competent group of scientists will agree on", at least in application to concrete proposals (p. 178).

The use of the peer review system in allocational decision-making implies an "implicit priority for the 'best science' regardless of considerations of social benefits or consequences", and Brooks suggests that "any other scale of judgment implies a conscious decision to support less good science at the expense of better science for reasons lying outside science" (p. 181). But "what should be the role of scientists in dealing with the utility dimension of priority setting, and indeed what should be the role of the public?" The special skill which scientists can bring to bear in this context is "the capacity to predict the probable or possible consequences of proposed research." This is, however, a "peculiarly difficult" role for scientists for a number of reasons—it involves "an inherent conflict of interest, especially in a period of constricted funding", and "as we move from applied towards fundamental research", predicting the possible practical consequences becomes "so uncertain . . . that the only honest thing we can do is to fall back on a generalized faith in the average beneficience of new knowledge" (*Ibid.*).[11] In light of his suggestion that scientists have come to share much of the "pessimism about the consequences of knowledge" that is "part of the general climate of our times", neither Brooks' observation that they are generally uncomfortable with the role they are expected to play in the utility dimension of priority setting, nor his conclusion that "they do not do it very well", seems terribly surprising.

The overriding problem, Brooks goes on to suggest, is that while scientific merit and "utility" are both useful criteria to apply in evaluating scientific proposals, there is an inherent "complementarity" in that "for a project whose utility is very difficult to assess, . . . scientific merit is easy to assess", and vice versa. More importantly, there is "a large gray area . . . where the difficulty of assessment arises primarily from the inability to agree on the relative weights to be assigned to truth and utility in evaluation" (p. 182). It is in this gray area that priority-setting becomes most difficult, and where "the greatest effort needs to be made . . . in raising the level of discourse between the scientific community concerned with truth and the political community concerned with utility" (p. 193).

What background assumptions need to be understood in order to make that discourse effective? In the next section of his article, "Who Decides?", Brooks ventures the suggestion that while "the public, and its representatives through the political process are the ultimate arbiters of research priorities", this "simple statement has little operational significance:" "In hardly any other field of activity is there a bigger discrepancy between constitutional and informally understood responsibilites. In practice, the public must delegate a large part of its constitutional responsibility to the scientific community . . ." (p. 183). In Brooks' view, this is all to the good, because

---

[11] "When scientists are asked to defend their basic research in terms of utility", Brooks notes, "the result is frequently naive and forced, a job of salesmanship rather than thoughtful analysis" (p. 181).

> only the scientific community can really manage science. When the public steps in, through the political process, to manage science in detail, it not only destroys science but does not get the utility it is after. Furthermore, the capacity of the scientific community to subvert attempted political management is much greater than in other fields of endeavor, but the more such subversion is necessitated by ill-advised political management, the greater the mistrust of science. This leads to a dangerous vicious cycle.

But while there is "no alternative to a largely self-governing science", the scientific community is not unaware of the fact that it "must have and deserve the trust of the public" and remain "responsive to the basic aspirations and expectations of the polity." The debate surrounding the conditions governing the delegation of responsibility "hinges on what is basic and thus pertains to the political process, and what is tactical and thus pertains to the autonomous governance of the technical community" (*ibid.*).

The line between these two spheres is often thought of rather simply in terms of "a sort of centralized model of priority setting in science", according to which "the political process should decide the social priorities of the country, and then the scientific community should be deployed to match these priorities" (p. 184). But Brooks cautions that this static model is not very helpful—"means and ends are not so easily separable in the real world", and social priorities are constantly changing in light of new knowledge—and advises that "research priorities can only be arrived at as the result of constant dialogue between the political process and the scientific community." With the growing presence of scientists on congressional staff and the increasing involvement of support agencies (e.g., the Office of Technology Assessment (OTA), the Congressional Research Service (CRS) and the General Accounting Office (GAO)) in the areas of technology and science policy assessment, Congress has in fact been at the center of "a large acceleration of the dialogue between the public and scientists" that may result in a better appreciation of the many variables and factors that must be taken into account in decision-making for science. Brooks regards these developments as "the first phase of an important process, which ultimately will promote as stronger connection between science and overall priority setting in the government, not just the setting of priorities of R & D in isolation" (p. 185).

How effectively that "connection" will be made, however, may depend to a significant extent on developments in the other, less formal or institutionalized phase of priority setting, which relates to "the direct role of the public in guiding research priorities—the much discussed subject of public participation" (p. 185). This topic has received less extensive scrutiny in the United States than in Europe, but Brooks notes that it came to the fore in this country during controversy surrounding energy policy in the mid- and late-

1970s, when concern about the long-term safety of nuclear energy "sparked a wide public debate about alternative energy sources and a shift in R&D priorities toward renewable energy sources and conservation" (*ibid.*). This debate resulted in the introduction of new mechanisms for public participation in science policy-making ("as provided for example in the National Environmental Policy Act") whose principal effect has been "to provide a new forum in which minority views in the scientific community could get a wider hearing" (*ibid.*). The inclusion of "a wider range of scientific views in the political agenda"—and the resulting pressure on the scientific establishment "to reconsider its own opinions and priorities"—has some clear short-term benefits and is probably on balance a "healthy development." But Brooks expresses concern that public participation may become "simply an umbrella for quasipolitical debates within the scientific community" and that this, in the long run, may well "increase distrust of scientists as political actors, and lead to further invasion of lay judgments in the tactical, as opposed to strategical, domain of scientific priority setting" (pp. 185-6).

In the final section of his article, "Social Priorities versus Research Priorities," Brooks suggests that much of the public unease about recent trends in the allocation of resources to federal R&D is symptomatic of a larger problem—"implicit dissatisfaction with political priorities" (p. 186). The criticisms made of the heavy concentration of federally- and privately-supported R&D in the defense, space, and nuclear policy areas raise the question of whether this allocational pattern "is to be regarded as a distortion of R&D priorities, or whether it is to be considered as merely reflecting political priorities determined through the democratic process" (p. 187). Critics who suggest that the former is the case—that "distorted" R&D priorities drive political priorities—tend to hold that, for example, arms spending "can only be gotten under control only by getting military R&D under control" (*ibid.*). But Brooks is "dubious" that such a view, which suggests that it might be possible to "leverage" a change in higher-order policy priorities by concentrating on changing R&D priorities, makes much sense as a political strategy, and he is wary of its possible implications for the research enterprise. A review of aspects of the controversy surrounding federal health care and biomedical research policy during the 1970s, he concludes, seems to support the contentions of those who argue that R&D priorities are more a reflection of political priorities than vice versa and to suggest "that debate over R&D priorities cannot be conducted about R&D as an isolated and separate activity" (p. 189). The important conclusion to which all this points, in Brooks' view, is that "it would be a great mistake to think that we can have much influence on social and political priorities by conscious tampering with fundamental research priorities in accordance with external criteria" (*ibid.*).

SCIENCE POLICY MAKING IN THE UNITED STATES

and the Batavia Accelerator

Anton G. Jachim

SOUTHERN ILLINOIS UNIVERSITY PRESS
Carbondale and Edwardsville

Feffer & Simons, Inc.
London and Amsterdam

3

The Organization of Science and Government Some Consequences

The discussion of the structure of the situation in which science policy issues in question were worked out from 1963 to 1967 continues in this chapter. In their turn the support of R & D in general in the United States, the formal and informal organization of professional scientific affairs, and the organization of government for decision-making on science support issues are discussed—the latter two are closely linked. Last of all, the outcomes of these structures and processes are discussed in terms of the distribution of R & D resources.

First of all, a look at the expenditure pattern for R & D for the entire society is in order. The gathering of data on R & D expenditures and resources is a relatively new governmental activity (carried on mainly by the National Science Foundation) compared with, for instance, the gathering of labor force data. By comparison the R & D data are derived from many different kinds of sources, and are not as homogeneous. These data raise problems for those who might attempt sophisticated analyses. But they are quite acceptable for purposes of description and comparison.

The total annual expenditure for R & D in the United States had increased steadily for twenty years until approximately 1965 when the rate of increase declined, ushering in the current so-called "scientific recession." Total R & D expenditures by all sectors making them—the federal government, industry, universities and colleges, along with their associated federally funded R & D centers and nonprofit organizations—amounted to $5.2 billion in 1953; in 1959, $12.5 billion; in 1965, $20.4 billion; and in 1970, $27.2 billion. The overall rate of increase in expenditure patterns for R & D vis-à-vis the general economy is very revealing. From 1953 to 1958 the mean annual growth rate for R & D expenditures was 15.9 percent, while from 1958 to 1965 it was 9.4 percent. By comparison the mean annual increase in GNP (Gross National Product) from 1959 to 1965 was approximately 6 percent. The ratio of R & D expenditures to GNP went from .007 in 1955 to .02 in 1965.[1] Any sector of the economy that grew as fast as this was bound to attract attention, positive and negative.

Selecting out expenditures for basic research one finds the mean annual growth rate to follow a similar line except to show amazingly that basic research overtook general R & D in growth in the early 1960s. This well illustrates the success of the selling program of scientific leadership which is discussed below. From 1953 to 1958 the mean annual increase here was 14.8 percent, but from 1958 to 1965 it was 6.6 percent. During the period of scientific recession, exemplified by figures from 1965 to 1970, it dropped to 6.6 percent while the overall R & D increase dropped to 5.9 percent.

The contributions made by government agencies to total R & D expenditure shows that growth here was larger than that accounted for by expenditures for many other public purposes. The federal government's expenditures in 1953 were $2.8 billion or 53 percent of the total; in 1965 it was $13 billion or 63 percent of the total; and in 1970 it was $15 billion or 55 percent of the total. The two interesting facts here are the rapid rate of increase to 1965, of course, but also that the governmental expenditures at any of these points in time account for more than half of the total—that is, they account for more than all of the other sectors' expenditures combined. This latter statement holds true even for the recession period exemplified by the 1970 figure.

The analysis of variables other than level of expenditure over this time period reveals a similar pattern. Scientific manpower, for instance, in R & D increased in this period (to 1965) at 7 percent per year, but the total civilian labor force grew only 2 percent per year. Further variables such as rate of growth in membership, number of specialities or differentiation, number of journal publications, and so on, tend to vary together in the same direction. But these other variables are greatly affected by R & D expenditures (especially governmental expenditures). Although one cannot account for a possible growth in quality in this way, and most certainly cannot account for the occasional "great discovery," it seems fairly clear that federal government expenditure is the important independent variable explaining growth.

The real and imagined implications of these expenditure curves began to be noticed in the early 1960s. The BOB (Bureau of the Budget) started a series of annual special analyses of R & D and the Bureau, along with many others, has called for a "rational" approach to basic research expenditure. Of course, the same kinds of moves came from Congress. A report of the House Committee on Government Operations stated, "An adequate decision-making process would use criteria which could be applied in making comparisons, establishing priorities, or choosing among competing claims. The criteria actually employed usually evade any such comparisons."[2] The scientific community recoiled from these, as should become clear later in this book.

Let us move to the setting behind these data. The entire history of science, technology, and government in the United States is interesting in itself,[3] but it is the set of conditions that are involved in current policy problems that are important here. Though it is so largely unrecognized, it is significant to note at the outset that current conditions with respect to funding and levels of expenditure for R & D by the public sector have ties to early attempts of government to support research in the hope of solving serious social problems. At first it was rather indirect. Developmental and experimental work had traditionally been done by old-line agencies such as the Army, Navy, or the Bureau of Mines, as well as by regulatory and law-enforcement agencies as an "overhead" function or indirect cost function toward achieving their formal organizational goals. When we are informed today that all twelve cabinet departments and twenty-seven agencies do some R & D[4] this is often the kind of R & D involved. Aside from the work accounted for in the budgets of a handful of agencies (to be discussed later) only a small portion of this work could be considered basic research. A more direct approach in the earlier days was the establishment of agricultural experiment stations with federal funding at land-grant state universities (also established with the help of federal funding under the Morrill Act) and other locations in various states via the Hatch Act of 1887. The purpose of the Hatch Act was to stimulate agricultural production through these researches. In some policy making circles there is much nostalgia for this approach. The state universities are constantly pressured for this kind of organization and service. The State Technical Services Act of 1965 (PL 89182) seems to have been put together with, if not the Hatch Act, the Agricultural Extension Service as its model.

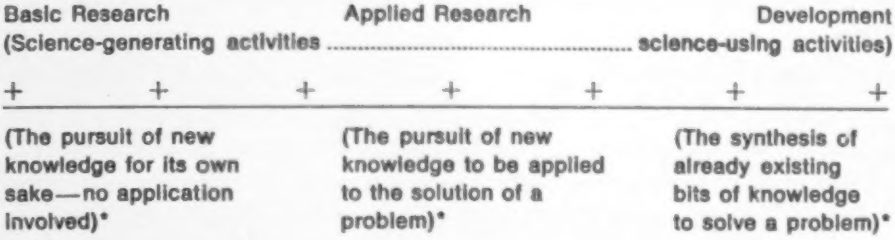
But, as with so many other such relationships, the beginning of modern government-science relationships dates back to the 1930s and the New Deal. In 1937 the National Resources Committee (NRC) produced a study that, implicitly at least, laid a good deal of blame for the Great Depression on the lack of technological innovation in American industry at that time.[5] It also seemed to imply that the federal government should consider subsidizing research so that applications made in the industrial sector of new discoveries would increase and stimulate economic growth. This theme of a strong connection between research and economic growth was introduced in the report of the NRC with regard to *industry* under these difficult conditions and has never disappeared in science policy-making in general.

Again not surprisingly, World War II marks another milestone. A number of events took place during World War II that led to new organizational structures for advice and decision making with regard to war research. War research was coordinated by the OSRD (Office of Scientific Research and Development). The OSRD was established in the Executive Office in 1941 taking a place alongside the other executive control units. When the OST (Office of Science and Technology) was created in 1962, it was also placed in the Executive Office in the hope that it would follow in the footsteps of OSRD. OSRD was headed by Vannevar Bush who also headed a commission that produced a report in 1945 entitled *Science, the Endless Frontier*.[6] With this report was created what might be called the "endless frontier myth." The myth was widely accepted. It implied that if "pure" as well as "applied" research received social support, both moral and financial—but especially the latter—numerous benefits would accrue to society. Up to that time the support of applied work seemed beneficial, and this had been the thrust of the earlier report by the National Resources Committee. There had seemed not much need to go beyond that in the earlier report. The basic and theoretical research that led to the control of atomic energy provided a nice example for anyone who might want to argue against the newer point of view, however.

Let us address ourselves briefly to these concepts. The common distinction between basic (pure) research and applied research and development needs some brief elaboration. Generally the public and many legislators even today view these as all of one kind of thing, especially when increasing R & D expenditures are lumped together as they usually are. That which can be designated as the basic research portion of the national budget, however, is quite small in comparison with the applied research and development portion. Moreover, and not too surprising perhaps, whenever appropriations committees swing the axe to cut R & D budgets the tendency is to trim first that small portion of the budget allocated to basic research. But, one would have to allow, it may be surprising to many (especially many a pragmatic taxpayer) to know that basic research is supported at all.

So the most vulnerable and controversial aspect of R & D expenditures has been that made for basic research purposes. The fact that the Bush Report came out in favor of its support added some justification for such programs as they were to develop, but it did not in any way settle the issue. A lengthy debate has gone on regarding a) the distinction between basic research vis-à-vis applied research and development, and b) the rationale for public support of all of these, especially basic research.

a) The debate on the distinction between basic research and applied research and development does not seem to have advanced the subject much. Not only has it not cleared up the "cognitive" haze, the debate has sometimes confused public policy questions. For the purposes of this section, it is best if basic or applied research are looked at as the activities (involving interactions) of workers (a special kind of professional worker) in a social system. Of course, if we see a man working in a laboratory, taking careful measurements with an instrument, we could not in most cases determine whether he was doing basic or applied research. Much of the concrete activity that goes on in each case is the same. But we do need to make some logical distinctions. Analytically, these activities occur at points on a continuum as are shown in figure *2*. Beyond the similarity in observable acts in any concrete laboratory situation, if we could "get into" scientific minds it would probably also be impossible to distinguish processes of creativity along this continuum.


Basic Research
Applied Research
Development
(Science-generating activities .... science-using activities)
+ + + + + + +
(The pursuit of new knowledge for its own sake—no application involved)*
(The pursuit of new knowledge to be applied to the solution of a problem)*
(The synthesis of already existing bits of knowledge to solve a problem)*

2. Basic Research, Applied Research, and Development Activities

---

* These are essentially the descriptive categories used by the NSF.

Well-known scientists have been able as individuals to go from one kind of work role to another with facility. Many basic scientists became excellent applied scientists and engineers during World War II, while this kind of changing of roles has gone on fairly regularly since then. Perhaps the sophisticated view of these activities is the increasingly accepted one that sees them as science-generating and science-using activities, both of which require creativity in performance. This gets away from the commonplace notion that first there is basic research, and then applications are made. All indications are that this often-cited sequence is not an empirical generalization. These creative activities are really all "mixed up." Ideas for fundamental research may frequently come to scientists who are at work on applied projects and vice versa.

By describing these activities as science-generating and science-using we get close to what is apparently a symbiotic relationship. This is not to say there are no differences. The problems to be investigated in science-generating and science-using activities are of a different order. The demands on creativity in science-generating activities seem to be greater and the research problems there more readily escape common sense. While funds for applied projects are more abundant, peer-group status accrues more readily to those occupied with science-generating activities. But the pure and applied are hopelessly interrelated in the empirical world of the scientific process itself.

We have just noted that individual researchers may, and often do, assume different research activities. The roles of basic researcher or applied researcher, however, are distinctive in important ways. It is most important here to take note of the fact that at the social system level different roles, norms, and values are considered the core of a social system, giving it a strong and relatively self-maintaining identity. This is to say, repeating what was discussed in chapter 2, that a basic research role incumbent looks to colleagues for recognition. Two polar cases illustrate this. For instance, Hagstrom reported from his research that, for the time being, the norms of science are operative in high energy physics research and finds this specialty still at the top of the status system, not only of physics subdisciplines but all empirical science.[7] This he finds to be true in spite of strong strains caused by some bureaucratization of research facilities as well as criticism and pressure from the political community questioning increasingly large expenditures for these facilities. As Hagstrom points out, this is so even in the case of high energy physics research where the researcher is much pressured to satisfy his patron and to view the patron as a client while these basic investigations tend to remain "of no use" (quoting Talcott Parsons). Because of the "exchange" relationships involved, the development engineer at the other end of the scale "acts out" a role that in some respects is more similar to that of a lawyer than a basic scientist. That is, he may rightly take a client orientation. In short, individual researchers move from one role to another. While excellence in creative activity is an aspect of them all, certain differences in behavior may be expected as one moves from one role to another. On the one hand the researcher does directed research and may seek recognition (to keep his contract he must seek recognition) from the client as well as his peers. On the other hand he does undirected research, seeking out recognition only from his peers. Thus we have simply shown that there are important differences as well as similarities in these activities.

b) In this section these concepts are used as they are ordinarily used in program administration, for undirected and directed research. Here these two concepts have special meaning for those who use them in policy deliberation, for example, as funding categories. Typical of the kind of statement one finds in these discussions is one found in the Bush Report which Alan Waterman, the first director of the NSF (National Science Foundation), chose to repeat to his audience: "Basic research is performed without thought of practical ends . . . (it) leads to new knowledge. It provides *scientific capital*" (emphasis added). But he goes on to state (as did the Bush Report) what we must agree is close to an empirical law. "It is important to emphasize that there is a perverse law governing research. Under the pressure for immediate results, and unless deliberate policies are set up to guard against this, *applied research invariably drives out pure*" (emphasis added). And then: "The moral is clear. It is pure research which deserves and requires special protection and specially assured support."[8]

Such a statement is important because it displays one of the main attempts at the justification of support for basic research activities. Namely, these activities provide "scientific capital" or investment capital in technological advancement. Technological advance is based on discoveries in basic science, so goes this approach. And technological advance leads to economic development. Under this rubric the Bush Report goes so far as to state:

> One of our hopes is that after the war there will be full employment. To reach that goal the full creative and productive energies of the American people must be released. To create more jobs we must

> make new and better and cheaper products. We want plenty of new, vigorous enterprises. But new products are not born full grown. They are founded in new principles and new conceptions which in turn result from basic scientific research. Basic scientific research is scientific capital. Moreover, we cannot any longer depend upon Europe as a major source of the scientific capital. Clearly, more and better scientific research is one essential to the achievement of our goal of full employment.[9]

The Bush Report also stated that the nation's health and a secure national defense were essentially the results of the support of R & D. So goes the rationale, increase the support of basic research—speed up technological change—increase economic development as well as other aspects that make for well-being. But one is asked to be patient about the span of time between discovery of a new principle in basic research and its application. J. Robert Oppenheimer once reminded his audience: "We may remember that Rutherford, who first understood nuclear transmutations, and who first induced them experimentally, held the not unreasonable view that no practical use would be found on earth for nuclear energy."[10] To quote Nobel laureate Rutherford himself, "Anyone who expects a source of power from the transformation of these atoms is talking moonshine."[11] In this instance, Oppenheimer went on to say that he would rigidly oppose any movement to limit the subsidy for research to those problems that were "practical," saying that any organizations' laboratory, industry included, should allocate one-fifth to one-sixth of its resources to research problems that are completely unrelated to its mission.

Researchers, many not having the long time perspective of an Oppenheimer and trying to have a grant request accepted by a federal agency, have felt compelled to come up with "applications" for their intended work. We have noted in an earlier section that a basic research role incumbent is constrained to seek recognition from his colleagues. A certain latitude seems to be allowed in these matters, however, when it comes to satisfying the patron's value structure in order to get or continue a grant, and thus to proceed with the investigations. This is, of course, a facet of the modern phenomenon of "grantsmanship." Even in a field as far removed from application as high energy physics, one tries to cite the possibility of utility in terms of the development of instrumentation for experiments that may be used elsewhere and the usefulness of its discoveries for other, more readily applied, specialties. Advocates also cite the usefulness of the training of engineers and technicians who provide instrumentation and help run experiments. Sometimes the communication gap between laymen and scientists allows for a little bamboozling. One American scientist states:

> I have a friend who is a band spectroscopist on the faculty of a large state university. He has been particularly interested in the band spectrum of the element nitrogen. He once said to me: "When the representatives of the state legislature visit me, I always tell them that I'm trying to make better fertilizer." There is, to be sure, nitrogen in fertilizer, and knowledge is power. It is just conceivable that my friend's investigations of the band spectrum of nitrogen may some day affect the fertilizer industry in some unexpected way. But it is undeniable that his interest is in spectroscopy, in and of itself.
>
> In a similar fashion, there is a certain temptation when writing a project proposal for submission to the Army or the Navy, to point out some way in which the work to be done is related to a military or a naval problem.[12]

Nor is bamboozling nationally bound. For instance, several cases have been reported that occurred during the Nazi regime in Germany, that regime being extremely suspicious of basic research orientations. For instance, some scientists went along with a project, the purpose of which was to find out if intersecting infrared rays would explode the bombload of Allied planes in the air. They felt the project was nonsense, but it provided the opportunity to do some interesting research in molecular structure.[13] Another vignette, taken from a series of interviews with captured German scientists after World War II, depicts an admiral visiting a physics laboratory to see what contribution it was making toward the war effort. A physicist showed him a spectrograph and told him, "This apparatus will enable us to produce iron isotopes so light in weight that warships can be built several hundred tons lighter than at present."[14] Nikita Khrushchev showed that he felt he had been bamboozled in his response to a complaint of low levels of research support: "You must never believe all these things which the scientists say because they always want more than they can get—they are never satisfied."[15]

There is ample opportunity to bamboozle when "bootlegging" occurs in budget allocations. Simply, bootlegging means the use of funds authorized under one category for work in another, and it can occur in any segment of expenditures. In this case it usually means that funds formally allocated for other research are used for basic investigations. Bootlegging occurs, practically speaking, in all sectors that perform R & D. It is generally agreed among the scientific community that some of the best research done is done with bootlegged time and funds. In universities, to cite one example of the process, there has long been a practice of using overhead funds, which are supplied by granting agencies on a formula basis to the organization where a grantee is working, to cover administrative costs for research projects in the social sciences and humanities for which outside government agency funds are not available. Likewise, bootlegging occurs in government laboratories, in nonprofit institutes such as the Rand Corporation and even in industrial laboratories.

Bootlegging, and the resulting bamboozling when expenditures must be justified, often takes place at a high level. In recent postwar years over 90 percent of all basic research (in terms of dollar expenditure) has been supported by "mission oriented" agencies, that is, agencies set up to achieve some goal which requires the "application" of science and technology. The NSF (National Science Foundation) which was supposed to support basic research, supported the remaining 10 percent. "Burying" parts of a budget also constitutes bamboozling. Although these percentages have decreased in recent years, large pieces of the basic-research pie have been accounted for by the research division of each branch of the Armed Services. To cite a case in point: The Air Force OSR (Office of Scientific Research) was told by the BOB (Bureau of the Budget) in 1954 that it had no business supporting basic research, with the result that this unit's budget was "moved under a line item for the B-58. And for all the Budget Bureau knew, the $4.7 million it approved was for research connected with the development of this aircraft, clearly within the realm of applied research. But in reality, this was money handed over to the OSR to use, as originally planned, for basic research."[16] Most recently, there have been attempts to curb high-level bootlegging by the DOD (Department of Defense). The recent "Mansfield Amendment" required the DOD to show that research projects were directly applicable to the goals of the department. Even under this provision, however, only a few projects were phased out by the DOD, and there is considerable evidence that the department has gone to great pains to justify questionable projects on this score, while DOD researchers have used problem-solving project proposals to cover what they themselves considered different kinds of work.

There are other attempted justifications for the support of basic research funds besides the promise of eventual payoff in technological innovation and economic development. Michael Raegen has done a systematic analysis of science-government related literature and has found four other categories of justification:[17]

1) The intellectual and cultural values of science.
2) Research as an essential component of graduate education.
3) The high costs of scientific research, and the unlikelihood of other adequate financing [no sector but the government can afford it—it is a *public good*].
4) The political values of science, especially in international affairs.

These do not require elaboration here. All save the first are seen as functions of concrete social problems and are patently reasonable. As noted earlier, although they have used all these ploys, high energy physicists have often been forced to base their claim on rationality and the pursuit of knowledge itself. Their spokesmen have called their research "the highest expression of the culture."

The bases for the support of basic research (science-generating research activity) have been shown, noting that researchers and their representatives generally attempt to make connections, although they are often very tenuous connections, between these activities and the solution of social problems. This can be aided by bootlegging. Scientific leaders (e.g., heads of R & D supporting agencies), in turn attempt to make such connection to a nonscience value system that stresses the solution of social problems (goal attainment for the society) such as national defense, maintenance of public health, and even full employment and so on, in interactions at the boundary where science and the political system meet. When high energy physics research is defended at the boundary, conflicts are very likely to occur because it is the archetype of basic research and sometimes has to be justified by connecting its researches to a value that is even farther removed from the solution of rather immediate social problems, the pursuit of knowledge for its own sake. One should not be too surprised if a little bamboozling occurs as well.

With these underlying expenditure trends, concepts, and relationships in mind, the next order of business is to show the structure of the organization of scientific affairs and then the connection between this structure and governmental organization for R & D.

Researchers themselves work within organizations, of course, the typical units being the university or the industrial or governmental laboratory. But researchers are professionals and the affairs of any profession must be coordinated somehow. In order for the affairs of science to be coordinated, rather complicated organizational structures have arisen that cross the boundaries of the work places. These take the usual voluntary associational form and are found on three levels: a) the specialized disciplinary associations, b) the associations that attempt to represent science in general, and c) the often interdisciplinary types of associations set up on an ad hoc basis, or permanently, that represent scientists on public issues.

a) The disciplinary associations act as communication and thus coordination mechanisms for persons who do or are interested in certain kinds of research, and the primary avenue by which this is done is the association's journal. Perhaps the best known of these associations is the American Physical Society which publishes the *Physical Review* and the sometimes even more important *Physical Review Letters*. These are considered the formal communication channel in physics, although there are many lesser ones. Such organizations will, of course, most often have regional subdivisions and normally will have regularly scheduled conventions and so on, where face-to-face communication can take place in smaller groups. Much scientific business is transacted at these meetings. These associations have from time to time acted as interest groups, looking out, for example, for the members' economic interests much as the American Medical Association is known to do for medical practitioners.

b) There are a few large associations that attempt to aggregate the disciplines and organizational units for more general purposes. These will publish a journal as well. The contents of these journals cover a far broader variety of interests, often reflecting fads in research as they respond to the needs of a wider readership. They will have an editorial board organized along the lines of the disciplines and will have a good deal of the journal space devoted to news, the careers of individuals, public policy questions, and so on. They qualify as the main communication links between science, government, and other research policy units since they are widely read by men of science and public affairs as well. The most well-known of these associations is the American Association for the Advancement of Science which publishes the weekly magazine *Science*. One only need read an editorial or two in *Science* to realize there is no question that the AAAS regularly acts as an interest group.[18] The AAAS holds a rather elaborate annual convention where disciplinary and interdisciplinary face-to-face communication takes place. Along with the disciplinary association conventions and other informal face-to-face conference meetings, scientific business is also negotiated here.[19]

c) The ad hoc public policy associations would today be typified by the numerous environmental and antipollution science groups that have sprung up around the country. There is one famous permanent group, however, that was organized in 1945 for such purposes, the FAS (Federation of American Scientists), originally named the Federation of Atomic Scientists. The FAS is best known for its work for atomic energy legislation in the forties. Approximately three thousand researchers, mostly in the physical science, became members and worked vigorously for the defeat of the May-Johnson Bill which would have continued military control over atomic energy matters. The FAS pushed for a civilian control agency. The FAS also has supported the formation of the National Science Foundation and the strengthening of that agency.

Since the late 1940s the FAS has been relatively inactive, although never completely so, and has had a declining membership. This is the typical fate of reform groups once they have had a significant victory as was the case with the FAS since the May-Johnson Bill was defeated. It has recently attempted to revive itself, to increase its membership, to attempt to appeal to other than physical scientists, and to become active on current issues such as arms limitation, the Vietnam War, the pollution of the environment, and so on.

The leadership of these three types of groups overlaps considerably. Clearly the interaction among these leaders as well as with those who hold formal or part-time leadership positions in governmental or quasi-governmental science organizations (Atomic Energy Commission, National Science Foundation, National Academy of Sciences, etc.) is frequent and regular.

There are certain equally if not more important informal leadership roles as well. Those who are Nobel Prize winners (seventy-six researchers in the United States had received the Prize as of 1969) are undisputed first-place role incumbents here. These persons form a communication loop that overlaps with the nine-hundred-member group that has been elected to the National Academy of Science, the NAS. Although the latter is much larger than the Nobel Prize group, it admits only fifty members each year, and thus is highly selective. The numerous members of the Cosmos Club of Washington, D.C. make up another overlapping communication loop. Although a still larger group, potential Cosmos Club members must go through a rigorous selection process, the main criterion for selection ostensibly being that a nominee in science has made a significant contribution to science and technology. (Scholars and men of letters are also included among the members.) The members of these groups are prima facie leaders in their respective disciplines and specialities. These communication loops overlap as well and there is a coalition process that takes place to form something similar to what de Solla Price has called "invisible colleges."[20] In the resulting communication loops of the invisible colleges research priorities are set, good research and bad research are recognized, a clear status system is maintained, and persons are nominated for positions. Persons are also nominated for prizes, even the Nobel Prize, or for membership in the National Academy, or for places on boards or panels that determine the awarding of research grants as well.[21] The informal leadership is thus heavily involved in performing these and other functions necessary for the solution of internal decision-making problems of science.

The organization of government for science is quite fragmented. In their book, *Science and the Nation*, J. Stefan Dupré and Sanford A. Lakoff state: "In a very real sense, science is now too important for mere cabinet representation. Like economics, it pervades all key aspects of governmental policy and defies organization within a neat executive pigeonhole."[22] In an attempt to lend some simplicity to the following description of some extremely complicated facts and relationships the formal structure of the government-science apparatus is viewed here as the a) advisory system, b) the allocational system, and c) the peculiar situation of atomic energy research and the Atomic Energy Commission.

a) *The Advisory System*. Only the most relevant units of the advisory system will be discussed. Since decision-making systems tend to draw inputs from numerous actors at various levels in the society they cannot all be discussed in any one place and one has to set limits.

Of the especially important units, the only extant organization that dates from an earlier era than ours is the NAS (National Academy of Sciences). It was established by Congress in 1863. The Academy is formally an independent organization, but is supported largely by government funds which come mainly from federal executive units. Approximately 80 percent of the advisory work of the NAS is done for these units. The remaining work load is accounted for by similar services to private foundations, state governmental units, and industry.[23] Since it was established by Congress to provide governmental units with scientific advice, it had adopted the approach of holding its services open for any congressman, congressional committee, administrative agency, presidential unit, and so on, that might require its services. When asked to study a particular problem the NAS has generally dwelt on the relevance for scientific advice of the questions posed to it before taking on the project. Having taken that to be a technical question, the Academy traditionally felt that it had no business accepting or rejecting projects on the basis of their policy implications. This philosophy has recently been severely challenged within the Academy with regard to the policy propriety of problems posed to it by the Department of Defense. One faction has stood behind the old approach which would not allow the rejection of a project if it were feasible while the other has stood for the rejection of projects that have undesirable social consequences from their point of view. Once a project is accepted, however, the procedure is then for the Academy to form a committee, made up of members and/or nonmembers who will serve, to study the questions involved.

In recent years the NAS as a whole has adopted a somewhat more aggressive approach by forming a policy committee, COSPUP (Committee on Science and Public Policy) that identifies on its own initiative public policy questions that have a scientific and/or technical aspect. The Academy then has little difficulty convincing some unit of government to support a study of the problem. A special panel under COSPUP is then formed and it works on these questions. All intuitive appraisals seem to come to agreement that the influence of the NAS has increased greatly over the past fifteen years. But the interaction between the Academy and governmental units is selective and represents rather specialized policy questions (in the day-to-day activity, outside of COSPUP). Moreover, until the early 1960s the Academy worked almost exclusively with executive branch units, thus the Academy's activities also came under some of the restrictions on public disclosure often practiced by those bureaucratic units. If a member of Congress is not particularly involved in these deliberations by way of his committee assignment, for instance, he might never have heard of the National Academy of Sciences. Only a negligible number of members of Congress during any session can be found who have serious scientific or technological training and it is still not unusual to hear a congressman confuse the NAS with National Science Foundation or the American Association for the Advancement of Science.

The NAS is a most prestigious organization, an organization which someone does not merely volunteer to join (one may simply join AAAS). One can safely say that once a researcher is elected to membership in the NAS he has arrived *ipso facto* as a potential leader of the scientific community. At the point of election there is no question that the new member of the NAS is also a member of the invisible college in his research specialty, that is, a leader in that specialty. Upon checking one would find that many, if not most, of the dramatis personae among the scientific leaders in the chapters that follow are NAS members.

But the NAS, in spite of its unusual selectivity, does take the associational form. A discussion of the NAS that implies its members all become involved with the day-to-day aspects of the advisory business of the association would be misleading. As with any such organization there is a staff that does most of this work. The regular staff of the NAS is headed by the NRC (National Research Council). The NRC was begun in 1916 to more efficiently manage the advisory operations of the NAS which were increasing steadily as World War I approached. It has stood as the operating bureaucracy of the NAS since that time. The NRC employs a staff of approximately one thousand, who are instrumental in getting the contracts from governmental units, organizing the study panels, serving as staff members of the panels, and so on. The NRC has as many as 400–500 study panels in existence at any one time.[24]

The NAE (National Academy of Engineering) was created under the National Academy of Sciences charter in 1964. The NAE came about after a long period of agitation by those interested in the engineering sciences who pictured themselves as victims of snobbery on the part of basic research oriented and most often university-based NAS members. They felt they could have a decent level of influence only if they had their own organization. Since 1964 the NAE has developed rather independently, holding its own annual meetings, setting up its own panels, and so on. It has never really found a close working relationship with the NAS.

As will be shown in this book, access to the resources of R & D is increasingly sought in American society. Likewise, access to prestigious or strategic positions is increasingly sought by members of the scientific community. The creation of the NAE is a good example. Pressure has been successfully applied for the creation of an academy for medical sciences, while attempts to set up an academy for the social sciences have not succeeded. These formerly neglected researchers want a share of the NAS services. The NAS also occasionally responds to the increasing pressure to allow the election of more members. New members inducted in this way are usually connected with disciplines or specialties not well represented formerly.

The NSF (National Science Foundation) was established in 1950 after a five-year running controversy. The ONR (Office of Naval Research) acted as a science foundation from the later war years to that time and continued to support much research unrelated to the goals of the Navy thereafter, diminishing its role increasingly as the NSF budget grew. A federally financed national organization that would support research as its major goal was recommended in the Bush Report of 1945. A bill that would have set up a nearly autonomous organization under the informal control of the scientific community was vetoed by President Truman in 1947. The subsequent bill set up the present structure. There is a director appointed by the president for a six-year term who shares operational responsibility with the board of directors, the National Science Board. The NSF has had a rather mixed-up history, partly as a result of its directors' attempts to plot out a role for the NSF that was not the one some congressmen had in mind for it. Among other things, it was initially supposed to carry out the work of overall coordination of federal research activity. This was partly an attempt at rational planning and partly nostalgia for the days when the Office of Scientific Research and Development seemed so effective since it did indeed coordinate war research and move the completion of projects ahead in a startling manner. But it was clear quite early in the game that neither the director nor the National Science Board would be able to do any such thing. One might add that it seems they never thought it possible or even desirable to do such a thing. They preferred informal coordination from within the social system of science itself.

Also the NSF was to become, and it has become, *the* basic research agency. It is the only governmental agency whose budget has traditionally been viewed as essentially a basic research budget. The agency has recently developed a number of supporting activities besides the support of research by professionals, although this has been its major activity as seen by the directors and outside supporting groups. It also funds a number of both pre and postdoctoral fellowships. It supports training programs for science teachers in public schools, junior colleges, and liberal arts colleges, funds the purchase of laboratory equipment in these schools, and so on. It was most recently assigned an unusual applied program that was not initially well received by its supporters, entitled RANN (Research Applied to National Needs), which was supported by "outsiders" (Congress) and did not at all fit the NSF role. (More on RANN in chapter 7.) The activities of the NSF are significant in regard to allocational decisions made among and within the empirical, mathematical, and now, the social sciences, which will be taken up again in the discussion of the allocational system. The NSF has additionally taken on the responsibility for gathering and analyzing data on all aspects of scientific activity that might bear on domestic or foreign policy questions as well as on science policy itself. Its list of publications in any given year is impressive.

The OST (Office of Science and Technology) took over the task of the coordination of research activities from NSF in 1963. It was given a "place" (on the organization chart) in the Executive Office. This was done largely at the urging of Congress whose members wanted to see some control over these affairs. As noted, the NSF, rather arrogantly some congressmen thought, had declined the job. The OST and the Bureau of the Budget (now the OMB, Office of Management and Budget) worked together in attempts to achieve this control. From the literature it seems quite clear that the BOB felt helpless in its management role when it came to R & D appropriations requests before the time it could call on the experts in OST. The combination of these two units for control purposes was quite a new thing. Very recently these relationships changed markedly when President Richard Nixon dismantled the OST in 1973 and once again tried to use the NSF as a planning agency.

The director of the OST was the president's Special Assistant for Science and Technology, a post established by Dwight Eisenhower following the Sputnik display of the Soviet Union in 1957. The Special Assistant also customarily acted as chairman of the PSAC (President's Science Advisory Committee). This unit was previously the Science Advisory Committee of the Office of Defense Mobilization established in 1954. It was upgraded to its title and spot in the presidential establishment in 1957. It brought together outside scientists and directors or secretaries of major executive branch departments and agencies involved in R & D and presumably provided "another voice" on science policy questions. The PSAC was dismantled along with the OST in 1973.

The voice to which the PSAC was supposed to be a rival is the FCST (Federal Council for Science and Technology) which is made up of representatives (though not directors or secretaries) of a large number of departments and agencies that use or generate scientific and technological knowledge. It has various functional subcommittees such as its panel on high energy physics. The FCST was organized in 1959, its establishment having been recommended in the Stillman Report in 1957.[25]

b) *The Allocational System.* The allocational system is made up of the actions (decisions) of the advisory system along with the actions of administrative operating units, Congress, and the scientific community itself. Most writers would wish to distinguish between "big science" and "little science" on this score. Big science, in the first instance, is thought to involve the allocation of large, expensive research facilities and manpower. In common parlance, big science is often used in reference to large facilities that are used in development projects. The most recent example of such facilities are those used by NASA (National Aeronautics and Space Administration) for its space program which is not primarily a basic research program.

Little science is thought to involve the allocation of relatively small resources to individual researchers or relatively small groups of researchers to support projects that they have proposed. The concrete units that are involved in these two kinds of allocations are indeed different in most cases. For instance, the PSAC was not likely to have had anything to do with an individual National Institutes of Health grant; but it was likely to be involved in the decision to construct a new biomedical research facility.

What is more important than big science and little science on the basis of size of expenditure is the nature of the commitment and the decisional rules used in these two cases. They tend to be similar. Central in these rules in either case is the premise that resources should be allocated to the best (i.e., most competent) researchers. Perhaps even more important, researchers who serve on advisory bodies such as the PSAC have at some point been awarded high status in the scientific community because of the quality of their research. Those who serve on panels that choose from among research grant proposals are likewise usually distinguished. The best decide what is and who are best. Moreover, those who serve on advisory panels or units like the PSAC, National Science Board, National Research Council, or perhaps directors of the NSF, can regularly be identified as members of the "invisible colleges" discussed in a preceding section.

The scientific community has been able to protect its integrity fairly well on this score by insisting on autonomy in these advisory-decisional cases. In the words of Leland Haworth: "the scientists of the nation are best equipped to determine what is in the best interest of science."[26] This attitude prevails in allocational decisions.

A large number of departments and agencies support or contract for R & D, but significantly five administrative units have accounted for as much as 93 percent of the total federal R & D expenditures; DOD (Department of Defense), AEC (Atomic Energy Commission), HEW (Department of Health, Education and Welfare), and NSF (National Science Foundation). The AEC has some peculiarities and will be discussed separately below. The others can be discussed as a group.

When the grant-giving apparatus was set up administrators in the NIH (National Institutes of Health under HEW) and the NSF looked for a model for these kinds of programs. They found the most useful one in the major private foundations that had supported research in universities up to that time. Researchers thus act as part-time advisors to these agencies. They serve on panels (or boards or committees) that make choices on the relative merit of solicited and unsolicited research proposals. These panels are organized along the disciplinary lines. Usually only representatives of the relevant disciplines sit on a given panel, although as the cost of the projects they are dealing with goes up pressure to appoint members of other disciplines or laymen increases, which is a regular issue in the advisory system where large projects are regularly discussed.[27]

In keeping with traditional American normative administrative theory these panels are labeled *advisory*. Even in the case of the units that are specifically set up for the support of research, as is the National Science Foundation, the myth continues that specialists have no authority to make decisions. Presumably agency administrators review the recommendations of these panels with the broader "public interest" in mind. Only they have the authority to make decisions. For this reason the aspect of collegial choices of the best is obscure in official agency literature. In this literature there is much stress on the substance of the proposals, while there are regular attempts to connect these objectives with larger social purposes, or there is outright bamboozling DOD style. Charles V. Kidd appears to think that this is one of the key adaptations that science has made in order to cut down on internal conflicts over allocations and to make the process of the selections of the "best" acceptable to laymen who tend to take a more egalitarian view. If indeed the stress were overtly on the status of the individuals making the proposals or on the prestige of the organizations that they represent, internal and external pressures might become explosive. But informal processes are critical. Kidd observes: "Within this somewhat ritualistic performance, the capacity of individuals and groups is in fact given much greater weight [compared with the substantive merit of the proposal] than one would assume from the written policy statements of the federal agencies."[28] We will come back to this point in the discussion of the distribution of scientific and technical resources below. For all practical purposes, "recommendations" (decisions) of these advisory judges are usually final.

The disciplinary panels of the NIH and NSF act very much like the system of referees that operates with regard to the major scholarly journals. The referees read completed manuscripts on research projects and findings submitted to a specific journal for publication. They screen them by accepting, rejecting, returning them to be rewritten, and so on. "The referees express an opinion particularly on two points: whether the claims of the papers are sufficiently well substantiated and whether it possesses a sufficient degree of scientific interest to be worth publishing."[29] Presumably since publication space in the prestigious journals becomes ever more scarce, manuscripts are ranked some way. Thus the access to the recognition that any researcher aspires to is either blocked or opened by the collegial, or peer system of decision making. On the other side (i.e., panels) access is blocked or opened to resources that enable the researcher to begin or carry on with his work by the same decision-making scheme.

Although it is not readily acknowledged, the presence of collegial peer group decision making with regard to the allocation of large research facilities can be observed too. To be sure, these processes are even more implicit than in the case of little science. Since such decisions involve higher expenditures they have tended to be taken out of the interest group vacuum of specialized little science allocations. But recent events suggest that there are more similarities than differences here. It is no accident that the Brookhaven National Laboratory (BNL) was located where the facilities might be administered and used by Harvard, Massachusetts Institute of Technology, Princeton, and the other universities that combined into Associated Universities Incorporated (AUI) to operate the facility. It is no accident that the Lawrence Radiation Laboratory was established at Berkeley or that SLAC (Stanford Linear Accelerator Center) was established in the same general locale. More will be said on these issues in later chapters.

In order to complete this discussion of the allocational system a brief note must be made here on the legal techniques by which government supported research projects go forward before taking up the case of the AEC. The difference between what is called giving grants to sponsor or support research and contracting to procure research is significant in that the former technique is most often used with regard to projects which can be explicitly (publicly) termed basic research in any particular case. This means that the granting agency has the formal authority to support such projects. The difference, however, is more legal and technical than anything else. Prior to the establishment of the National Science Foundation, all federally funded research was done on a contract basis and only after 1957 were other agencies allowed to give research grants. When research is being done on a contract basis a greater burden is placed on the researcher with regard to accounting and reporting, but it seems that investigators and granting agencies got around this in the early days by simply ignoring these requirements or by the agencies writing very vague contracts. Many of the early contract formats were set up, after all, for the procurement of ordinary, mass produced items. But the AEC is quite unusual on this score. It contracts with nongovernmental organizations for the procurement of research, for example, basic high energy physics research. Within the organization of the AEC laboratories though, allocations in terms of time on the accelerator are made in terms of research project proposals made by research teams. In this setting the laboratory director receives the recommendations of a committee of peers who review the research proposals of the research teams. The committee of peers is roughly representative of the university consortium members if the laboratory is managed by a multimember association such as the Associated Universities Incorporated at Brookhaven or is composed of staff members and outside consultants as in the case of single university operated (but federally funded) laboratories. Thus the decision-making scheme is markedly similar to that of project proposals before the NSF or NIH. The techniques then are diverse, but the decision-making rules are similar.

The NSF provides funds to university researchers. The NIH does the same, but also has a large intramural research program. The DOD and NASA have lessened their contributions to the support of basic research in recent years, but they are still significant contributors to that category. They use the techniques of both the NSF and AEC, but also own and operate their own laboratories. Where outside advisory panels are not used for advice on allocational decisions, opinions of leaders in the scientific community are taken into account and/or panels are arranged composed of scientists who are government employees (the latter is frequently used by the DOD). When allocations to big science facilities are in question, formal leaders' opinions are explicitly taken into account.

In the postwar era, policy for R & D or questions concerning the use of R & D were pretty much contained within the executive branch. Leaders of the scientific community have grown to feel quite comfortable in these bureaucratic surroundings where expertise and rationality presumably are prized. It was the executive branch agencies that initially wanted to stimulate the economy by supporting R & D as exemplified by the Bush Report. The executive branch organizations took the initiative in making the first contacts with the scientific community. Until quite recently, congressional interest in R & D has been restricted to the "meat axe" of the House Appropriations Committee and occasionally to the "pork barrel" area with regard to physical plant and payrolls. For some time even the JCAE (Joint Committee on Atomic Energy) was reluctant to do much more than *pro forma* agree to the program requests of the Atomic Energy Commission and, in turn, the Commission's advisory body, the GAC (General Advisory Committee). In recent years this situation has changed considerably. The JCAE has become quite important. There is also now in the House the Committee on Science and Astronautics, a Subcommittee on Science Research and Development which focuses on the NSF, and an Intergovernmental Relations Subcommittee on Government Research which held hearings on R & D support questions in 1963 and 1964. In the Senate a unit was created especially for these questions in the early 1960s, the Subcommittee on Government Research of the Committee on Government Operations. There have been a number of special committees in the past ten years that have dealt with R & D questions while the standing committees have paid more attention to the R & D budget requests of the agencies with which they deal. As ever, appropriations committees have been active.

It is not profound so to say, but congressional and administrative actors tend to come to the bargaining table with different perspectives. In time the administrative units have often come to represent the interests of professional researchers.

c) *The Case of the AEC.*[30] Sponsored atomic energy research was initially carried out under the formal control of the Office of Scientific Research and Development, control later being transferred to the Manhattan Engineering District of the Army. From 1946 to the present it has remained under the AEC. (Other units such as the NSF and ONR sponsor some research in high energy physics, but the AEC accounts for 90 percent of the research falling into that category.) The issue of whether the control of atomic research and other matters related to it should be largely confined to the military or civilian units after World War II was a serious one. One argument was that much of the work up to that time concerned the development of weaponry and was necessarily enveloped in secrecy and the attendant security procedures. This strengthened the hand of the military. On the civilian side lay the argument that there were competing purposes to which atomic energy might be put, that in fact atomic energy concerns were too important to be left to the generals. The Federation of American Scientists strongly espoused the latter argument. The Federation was, in fact, organized specifically to oppose continued military control.[31]

The argument for civilian control winning out, the decision on the legal form that the new organization should take came up. Although the President's Committee on Administrative Management of the late 1930s came out against this form of organization, the still popular (in 1946) independent commission form of administrative structure was heavily supported over a single administrator form, such as the form adopted for NASA a decade or so later. The latter would have given the president considerably more control. Moreover, "three or four or five heads are better than one" has much appeal when the issues are complicated and awesome. The commission form was thought to take these technical but difficult questions out of politics. The Atomic Energy Act of 1946 provided for a five-member commission appointed by the president, one of whom was to be designated chairman. The commissioners are appointed for five-year terms.

As critics of this kind of arrangement often point out, administrative activities under such a formal structure tend to take the activities out of the politics of the president, or whatever appointing executive is involved, but into the politics of Congress and especially organized interest groups.[32] This seems supported by the fact that the Joint Committee on Atomic Energy has repeatedly looked away from recommendations that the AEC be reorganized to the single administrator form, even when the commissioners themselves have recommended it.

The commissioners have tended to have prior experience in some facet of AEC operations and thus have tended to specialize once on the Commission, dividing the programs of the AEC among them and regularly getting involved with day-to-day operations. Most of the big questions are handled by the Commission's prestigious GAC (General Advisory Committee) which has the same advisory responsibility with regard to atomic energy as the President's Science Advisory Committee had with regard to all aspects of science. The GAG and PSAC would also meet together on major issues. The GAC, originally chaired by J. Robert Oppenheimer, had a strong influence on the Commission prior to the defeat of the Lilienthal GAC recommendation against H bomb development.[33] After that the relative strength of the GAC and PSAC vis-à-vis the AEC seems to have changed from one issue to another and to depend on the personal influence of the persons appointed, defying generalization.

Along with the creation of the AEC was created the JCAE (Joint Committee on Atomic Energy). It is generally conceded that the JCAE is an unusually strong legislative committee. The committee has nine members from the House and nine from the Senate. The majority party has no more than five of the nine members from each house as stipulated in the legislation. Members have generally tended to remain on the committee, which is a reliable sign that it has been accorded relatively high prestige and that appointments on the committee are sought. As of December 1966, four members had been on the committee since 1946, while the average duration of the members' tenure ranged from 5.5 to 7 years.[34] Clearly the selection to the JCAE could not be based on professional qualifications, so few of the members of either house having scientific credentials. Geography does seem to be important though, since states in which there are large AEC expenditures have had disproportionate representation.[35]

By all accounts the influence that the General Advisory Committee seemed to have over the AEC in the early years was matched by the influence that the AEC had over the Joint Committee on Atomic Energy. In the early days the JCAE members seemed to have been content to stick with items that they could handle. This meant largely personnel and security matters. All units of government were busy in those days ferreting out "security risks," but perhaps the committee got overly involved in these matters partly in order to cover for its members' lack of expertise on the substantive issues. Because of its relative lack of expertise the committee set out to land a superior staff and it has depended heavily on that staff. The committee has by all accounts, perhaps because of its relatively high legislative prestige, been able to recruit and retain a high quality technical staff throughout its lifetime. This, of course, has not been true for many other committees.

The 1946 act gave the committee a series of bargaining points with which to further strengthen its hand. It stated, for instance, that "the Commission shall keep the Joint Committee fully and currently informed with respect to the Commission's activities," and the 1954 amended act stated, "*all* the Commission's activities" (emphasis added). The amended act of 1954 also gave the JCAE jurisdiction over "all bills, resolutions or other matters in the Senate or the House of Representatives relating primarily to the Commission or to the development, use, or control of atomic energy."[36] These points and other developments have assured the JCAE as *the* committee for atomic energy matters. These matters are not divided among a number of committees as are some others. More important, they have on the one hand excluded the possibility of the AEC building alternative bases of influence in the Congress and have provided the AEC the opportunity of concentrating its attempts to influence Congress on the other.

The programs of the AEC are organized functionally into divisions and offices, there being among them a separate Research Division. The AEC's impact in this area of expenditure has not been much affected by the growth of the National Science Foundation as such other units as Office of Naval Research have, and the AEC is still considered by the Office of Management and Budget and other relevant units the major agency for the support of these kinds of physical research.[37] The magnitude of the AEC's basic research expenditures is noteworthy. On high energy physics alone it spends more money than is included in the entire NSF basic research budget. In an interview with this writer, a leading member of the high energy physics research community explained this simply as a result of the scientific merits of cases at points in time, agreed upon among members of the scientific community.

> I think there are right times for various things to be done and I think high energy physics over the past twenty years has been at the stage where scientists have understood the questions that had to be asked, they knew the techniques through which they could get the data that bore on those questions and usually when you are at that stage you also know ways of putting industry to work producing the apparatus and so on, which means you spend money . . .
>
> If I can define my problem that well I can spend a lot of money very rapidly . . . and I think high energy physics was at that stage . . .
>
> I remember a long time ago . . . Salvadore Luria [a molecular biologist now at MIT who won the Nobel Prize for physiology in 1969] who was at Illinois then, and I was also, and I had sort of a guilt feeling because high energy physics was getting $100 million a year and biology . . . his field . . . was getting peanuts. I asked him how he felt about that and he felt completely satisfied with the situation and confident that the day would come when high energy physics would sort of run out its string—which may or may not be true—but biologists would come to the point where they knew how to do things with

> technology and answer questions that were bugging them and they would begin spending money. I agree with this completely. In high energy physics it just happens over this half century to be the one where we have all the people, the tools and the questions and the understanding just to make enormous progress very rapidly. That's why we're the big money spenders.

The ONR and OSR (Office of Scientific Research-Air Force) contributions to high energy physics are holdover commitments from the earlier era. Most new funding is done under the auspices of the AEC, with some minor exceptions. (An accelerator was recently built, under the direction of Robert R. Wilson, at Cornell University with NSF funds.)

The support of basic research is something that the Commission has taken on itself. The General Advisory Committee and President's Science Advisory Committee had urged the Commission in this direction, while in the beginning, and even now, the Joint Committee on Atomic Energy questioned this role and has never been comfortable with it. On the Commission's side, however, support of the basic research role is clear. A recurrent proposal by the commissioners had been to bring expenditures in the physical science research program up to the level of other AEC program spending. Moreover, expenditures for the physical research program actually increased much more rapidly than did expenditures for the many other AEC programs in the past twenty years. In line with the rationale so often used elsewhere, the AEC chairman recently stated:

> Because all these development programs of the AEC are continually pushing the frontiers of technology, the AEC has always been convinced that substantial participation in basic research is essential. If the AEC failed to participate in basic research in this age, it would first find itself unable to communicate with the expanding forefront of science, and then find itself unable to evolve any radical new systems upon which technological advancement—and even survival of free men depend.[38]

As noted, the connection between basic research and technological advancement is not yet clear, much less the connection between basic research and "the survival of free men." But to some extent the JCAE has gone along. Its members have recently been somewhat concerned, however, that increasing physical research budgets starve other AEC programs.

The management of the Commission's large basic research facilities has raised some significant problems. These appear to be practical problems of administration that arise from the typical method of contracting for management of facilities which is used by the AEC. There are some more fundamental issues involved though as to the question of access on the part of users.

Where facilities are managed by one contracting university the question of access on the part of other university researchers has been raised. In the case of ANL (Argonne National Laboratory), outside of Chicago, for instance, an association of universities, MURA (Midwest Universities Research Association), originally grew up as rival to the University of Chicago (the high energy group at Chicago joined MURA and it became a MURA-ANL conflict—see chapter 4) that managed Argonne. MURA's members argued for their own accelerator, implying often enough that they did not trust the University of Chicago to give them sufficient access to the Argonne facilities when it was proposed that an accelerator be built there. A similar problem arose with regard to the rather proprietary attitude some researchers thought the LRL (Lawrence Radiation Laboratory-Berkeley) management took. With regard to the new NAL (National Accelerator Laboratory-Batavia, Illinois) of which he is deputy director, Edwin L. Goldwasser, when comparing the management of NAL with other laboratories, stated to this writer, "at other laboratories, users [outside university user groups] have had great uneasiness that the program was going to be usurped by the local staff." Recently the AEC has attempted to remedy this by requiring a formula for time that the operating university or universities, the permanent staff and outside user groups may have on high energy particle accelerators.[39]

The problem of the regional orientations of multiuniversity association management is similar to the local orientation of single manager contracting. Recently, attempts have been made to change this pattern by making the management of multiuniversity facilities truly national and to open up facilities to users outside the region where they are placed.

Another aspect of the same problem has to do with the allocation of time on the machine by a central advisory body and the director of the laboratory, or by committees made up of researchers who represent relevant universities in the managing association. Hagstrom's interviewees were not engaged in high energy physics research and so his observations on this score are quite tentative. But he is the only one who has examined the problem. He does state that the fear that this kind of decision making in big science might become bureaucratic has not been supported.[40] There are, to be sure, hard feelings on the part of those groups who think that they were not allocated the time they required to do their experiments properly. This is a field of investigation that is still open to the student of organizations who is interested in the possible cooperation of apparently competing groups in largely research environments that have important bureaucratic aspects.

There are many other aspects of the outcomes of these decision-making processes and structures in the advisory and allocational systems as well as the peculiarities of the AEC. The remainder of this chapter is devoted to discussion of one of these, namely the resulting distribution of R & D resources. The controversies over the management of AEC laboratories and access to their facilities illustrates how distributional questions inevitably enter in.

It should be noted that basic research, applied research, and development activities are carried on in industry, universities, governmental laboratories such as the AEC laboratories, and private nonprofit research institutes. But no matter what kind, the federal government administrative agencies are usually the source of the expenditures.

In 1965, for example, total expenditures by all sectors for R & D were $22.2 billion, $15 billion being accounted for by industry, $3 billion by federal laboratories, and the remainder by universities and institutes. But the major source of these funds was the federal agencies, the source of $14 billion. Basic research expenditures are accounted for largely by universities, institutes, and federal laboratories. While basic research expenditures in any given year amount to a small part of the R & D total, the source of 64 percent of these funds is once again the federal agencies. Much of the remainder has its source in private foundations. Industrial laboratories account for most development activity, while applied research seems to be distributed among all of these sectors.

If the important federal agencies that give grants or let contracts for R & D are ranked on total expenditures one finds that the Department of Defense is first, followed by the National Aeronautics and Space Administration, the Atomic Energy Commission, Health, Education, and Welfare, the Department of Transportation (1966), and the National Science Foundation. The National Science Foundation is the only agency among these formally designated to support basic research, but even though its expenditures have increased recently they have accounted for approximately 10 percent of the basic research budget as noted earlier. The expenditure ranking for basic research differs somewhat from total R & D, but, importantly, the AEC consistently outranks the NSF in basic research expenditures.

What is more significant than that above, the patterns of concentration in R & D resources can be observed in a look at their distribution. First, in the case of R & D expenditures in industry that had their source in the federal agencies, one finds that these expenditures were largely limited to three industrial groups: a) aircraft and missiles, b) electrical equipment, and c) scientific instruments. These industries account for 85 percent of the government-sponsored industrial research. These groups together, though, account for only 17 percent of the annual sales made by all industries. Moreover, the greatest part of industrial R & D activity was carried out in firms in these industrial groups that employed one thousand or more R & D scientists and engineers, that is, they are very large organizations.[41] One study showed that 60 percent of all development contracts awarded were made on a sole source basis—negotiated noncompetitively with a single, large firm. Of a fairly large number of the cases in the remaining 40 percent the contract awards went to the firm that initiated the project.[42]

Further, approximately 80 percent of all industrial R & D expenditures, regardless of source, are accounted for by the largest one hundred industrial firms. To be sure, the size factor is critical here in that relatively small firms do not have slack resources to commit to sophisticated R & D units that may have only a long run payoff. But we do know that the key firms in these three industrial groups continue to exist because their R & D units are respected in cost-plus government contracting circles. In other words, those firms known to do the best R & D work tend to get the big contracts. Small or newly organized firms usually have little chance at these contracts. The "name" firms attract the attention of the DOD or NASA to an unusual extent and as R & D expenditures have recently diminished in the DOD and NASA these firms have attracted the attention of agencies whose R & D budgets are on the increase, such as HEW or the DOT (Department of Transportation). Since administrative units such as the DOD and NASA or HEW and DOT are bureaucratically organized, one should expect that decision making there would reflect economic rationality. Because of the specialized nature of applied research and development work, noncompetition for R & D contracts, which does not hold costs down as well as traditional competitive bidding procurement procedures, has not been inconsistent with economic rationality. In many cases these industrial organizations are geared specifically toward governmental R & D contracts and are dependent on the contracting agencies, even having received subsidies in initial purchases of R & D capital and other capital equipment. In these cases many have seen government agencies rewarding the best where they themselves developed the best.

We noted that on the basic research side expenditures are accounted for largely by universities, research institutes, and government laboratories. Selecting out university research shows a revealing pattern. Approximately two-thirds of all university research budgets combined have their source in federal agencies. The obvious patterns of concentration are, once again, even more important. In 1963, for instance, 85 percent of federally sponsored university research was accounted for by the first one hundred universities, while 50 percent was carried out by the top twenty-four. These universities are in the top prestige rankings. There is no doubt that their governmental research support is related to their prestige ranking, although we do not know which is the independent variable, which is the cause and which is the effect. But a recent report shows that quality in graduate education is closely related to the level of federal spending for R & D at a graduate-training institution.[43]

The concentration of R & D resources is really a process that tends to perpetuate itself if allowed to go unchecked. It is easier for universities, industrial laboratories, or government laboratories to attract established names or the most promising newer researchers if they already have established researchers with whose names candidates can identify. A researcher often accepts a position—sometimes at a lower salary than he might have had in another position—because there are already name researchers in the recruiting organizations. This leads to the concept of the "critical mass" in research or other intellectual activity. This is a concept borrowed from physics which when translated into social affairs means that when a unit reaches a certain size it becomes viable and takes on a new identity. In R & D it means that there is a tendency for researchers to gather together. At some point in this clustering process creative activity is stimulated by their interaction, with research results then coming faster and research quality steadily improving.[44] Below certain research group size minima (different for different specialties) the critical mass effect will not occur. For perhaps this reason the organizations in which the different kinds of researchers work are often clustered. This may be observed by looking at the geographical distribution of scientists, engineers, and other R & D resources.

It is well known that the states of California, New York, and Massachusetts have large concentrations of these resources. Indeed, these three have accounted for as much as 46 percent of the federally sponsored R & D, that is, contracts and grants went to those states. These states account for 22 percent of the population while the Midwest (the East North Central, West North Central, and the West South Central combined) has 38 percent of the population, but has received as little as 17 percent of the federally sponsored R & D. The support of university research shows a more proportional distribution than R & D in general, but there is a heavy concentration in New England. The more even distribution may be accounted for by the fact that each state has some kind of graduate training institution, although the quality varies greatly. The educational establishments were there before large-scale federal spending for R & D began. In those states where there are distinguished graduate training institutions but where other R & D organizations are weak (e.g., the East North Central Region), there has been a serious brain drain problem. Those who receive their Ph.D.'s at these institutions are most often supported in some way by federal funds while in graduate school, but leave the region to take jobs more readily available for scientific personnel in areas of higher concentration.[45]

The concentration in basic research is in a few of the top twenty-four universities. Some geographic regions have few or no universities in this category.

Other counts support the pattern of concentration shown for basic research. In the mid-1960s there were approximately seven hundred members of the National Academy of Sciences altogether. The states of California, Massachusetts, and New York each had over one hundred of them as residents. There were no states that had between fifty and one hundred, New Jersey being next with forty-four. Thirty-seven states had ten or less, which included three states that had two members, eight states that had one member, and fourteen that had none whatever.[46]

From a survey of the 1960 census data the process of concentration for R & D resources can be seen in still another variable, urbanization. Approximately 25 percent of all scientists work in seven metropolitan areas: New York City, Washington, D.C., Los Angeles, San Francisco, Boston, Chicago, and Philadelphia. A survey made by this writer turned up the above, but also showed that fourteen major SMSA (Standard Metropolitan Statistical Area) districts in the United States contained 27 percent of the population but 37 percent of the scientists, and 41 percent of the engineers. In the states with heavy concentrations of R & D, scientific personnel are also concentrated in these SMSA areas. For example, while 50 percent of the general population of Massachusetts lives in the Boston area, 63 percent of the state's scientists and engineers live there. The Chicago area, in a state with low concentration, has 62 percent of Illinois's population while it has 74 percent of the scientists and engineers in Illinois. Further, in states where there is little R & D activity most of the scientists and engineers live in the urban areas that do exist. Denver and Salt Lake City, for instance, are the locations of most of their respective state's R & D resources.

This is no doubt only a partial explanation of the pattern of concentration for basic research. Concentration in the case of university research shows that urbanization is not so significant, since many of the major graduate training institutions of the Midwest are located away from urban areas. The placement of state universities is the function of the urban-rural political split in some of these states. That is, the universities were located where they are when agriculture was most important and state legislatures were truly rural dominated. This is also a function of the fact that small science university research requires fewer supporting facilities, such as high technology industries, nearby. These universities are among the top recipients of federal funds, however, and fall in approximately the same place in the university prestige rankings. One must look to the decision-making processes of the scientific community itself for a more complete explanation of these concentrations.

The bases of support for basic research where there were possible sources of conflict were discussed in earlier sections of this chapter. The second source of conflict in recent years has been the concentration, or skewed distribution, of scientific resources seen above. Although perhaps not apparent at first glance, this issue is in its essentials similar to the first. It is only necessary to note here that the unusual concentration of resources is to some large extent also a function of the scientific community's preoccupation with rewarding quality. That only best should be rewarded is a strong norm, and the best, we are told, can only be determined through the eyes of the scientific community. A cycle sets in. A good example of how this works is provided by the highly stratified prestige system among American universities. Within a basic research community individuals become recognized for their work.

But chances of producing such high quality work are greater if one's laboratory is located in a university of recognized high quality. That researcher, and his organization, attract financial support because of the recognition they both seem to be able to depend on. For these reasons the best graduate students apply to such universities for admission. And in the process of graduate study these students are more likely to be handsomely supported while learning to do research that is likely to be recognized as of high quality. They almost always get the better jobs upon completion of graduate study, having been placed in the mainstream of academic research quality. They and their mentors are much more likely than others to be nominated and selected to leadership positions in professional associations or in Washington agencies and eventually to become members of the "invisible colleges." Bluntly, a review of the names and the individuals' affiliations in such leadership positions as well as those names and affiliations usually connected with the most highly recognized research in any field, many have said, is likely to read like a social register.

The cycle repeats itself with remarkable predictability. Moreover, the allocations of prestige are remarkably stable. In the recent study of quality among graduate (research) training institutions, those respondents to questionnaire items who were established, or promising, researchers fairly regularly ranked the same ten to twenty universities and their respective academic departments in the leading quality-prestige ranks.[47] As we noted earlier, rank order correlations between these rankings and the ranking of research-oriented universities by amount of federal R & D funds received, are very high. According to a recent study in which rank order correlations were run "through the five quality classes there is a linear relationship between the logarithm of federal research and quality."[48]

An administrator from a relatively unrecognized university, for instance, discussed the case of one of his research-oriented faculty members. This researcher repeatedly made proposals to granting agencies which were regularly rejected. Because of his frustration over this issue, he accepted a position at a larger, better-recognized university. Less than a year later, he reported extraordinary success in attempts to get his proposals accepted. Indeed, he had attracted more research support than he could effectively handle.[49] An unusually candid comment by Philip Abelson is revealing.

> A man of proved research productivity in a small school in the Middle West may submit an excellent proposal, but almost invariably

> his proposal will receive a rating below that of a comparable application originating at Harvard. The difference is the impact of the known excellence of the institution.
>
> I sat on a study section at the National Institutes of Health from 1956 through 1959. As was the rule, our group rated grants on the basis of a scale from 1 to 5. The quality of applications originating from Harvard varied considerably yet few if any were turned down, and most received a rating between 1 and 2. Members of the study section were not personally prejudiced in favor of the great institutions, and if anything would have preferred to encourage research at smaller schools. Yet we could not in good conscience produce a different result.[50]

Taking the issue out of immediate controversies shows its generality. On the side of leadership resources Barber noted that leadership was concentrated in the hands of approximately thirty persons during World War II.[51] A recent estimate by Christopher Wright of the population of the overlapping leadership of science numbered the individuals in control from two hundred to one thousand.[52] Talcott Parsons discussed this in 1946 in his analysis of the conflict over the establishment of the NSF. He noted an underlying theme of conflict between aspiring groups within science who "tended to be suspicious of too great a monopoly in the hands of a 'scientific oligarchy' and of a privileged group of research institutions both academic and industrial."[53]

It is of further importance to note that the autonomy of science is argued for more strongly by those who act as formal or informal leaders than those who remain unrecognized. Those remaining unrecognized are often tempted to go outside of science for recognition or to establish bases of power. It is interesting to note, for instance, that the "have-nots" in the sciences, especially the social sciences, urged a single administrator form, or more "political" form, of organization for the NSF while the "have" group favored the independent board form. On this score, the similarities between the formation of the AEC and the NSF are remarkable.

In Merton's review of the empirical studies of the system of recognition within science he notes several unintended consequences of this system of allocation and distribution. He states that once having achieved recognition, for example, the Nobel Prize, the prestige that accrues to an individual does not fall much below that level no matter what his productivity is later on in his career. Recognition continues to be given to those previously recognized.

Interviews with Nobel laureates show that they are well aware of the gross misallocations on this score. Therefore, they sometimes refuse to coauthor a paper when they have collaborated on it with other researchers because they feel they will deprive less distinguished colleagues of recognition, that is, a reader will most likely remember only the laureate's name. In cases of independent discoveries on the same problem by researchers of different rankings, the contribution of a less recognized researcher tends to be forgotten. With regard to the communication system within science (on which the creativity-response by recognition relationship is built), not only is it difficult for the less recognized to have a paper published, it is also unlikely that many within his discipline will read it if he does publish. An increasingly larger fraction of researchers confine the small amount of time they have for reading publications to those books and articles that appear in leading journals and are authored by names.[54]

3. The Organization of Science and Government

1. The reader who is interested in more detailed information and the sources of these figures should consult Appendix B, tables 1–4, and relevant references in the Bibliography.

2. *Federal Research and Development Programs: The Decisionmaking Process*, 89th Cong., 2d sess., 1966, Rept. 34. See also Michael Raegen, "R & D: Suggestions for an Allocational Framework," *Public Administration Review* 27, no. 2, June 1967, pp. 104–11.

3. For the best study of the period before the one under discussion, see Hunter A. Dupree, *Science in the Federal Government.*

4. Michael Raegen, *Science and the Federal Patron*, pp. 84–85.

5. National Resources Council, *Research—A National Resource*, (Washington, D.C.: Government Printing Office, 1938).

6. Vannevar Bush, *Science, the Endless Frontier.*

7. Warren O. Hagstrom, *The Scientific Community*, pp. 167–76.

8. Quoted in "Basic Research in the United States," in Dael Wolfle, ed., *Symposium on Basic Research*, pp. 19–120.

9. Bush, *Science, the Endless Frontier*, p. 2.

10. "The Need for New Knowledge," in Wolfle, p. 8.

11. Quoted in an article in the *New York Herald Tribune*, 12 September 1933.

12. Louis N. Ridenour, "Military Support of American Science, A Danger?" *Bulletin of Atomic Scientists* 3, no. 8, August 1947, p. 222.

13. Samuel A. Goudsmit, *Alsos*, p. 154.

14. Ibid., p. 75.

15. Quoted in Daniel S. Greenberg, *The Politics of Pure Science*, p. 156.

16. Ibid., p. 129.

17. Michael Reagan, *Science and the Federal Patron*, pp. 34–70.

18. In the recent years of what has been called a scientific recession, calls for more explicit activity on the part of scientific organizations on the classic interest-group model have increasingly been made. See, for instance, letters of Gordon G. Power, "Science Is Too Important to Stay Out of Politics," *Science*, 29 January 1971, p. 334, and Ward Pigman, "Science Lobbies," *Science*, 28 May 1971, pp. 896–99.

19. As with many organizations in recent times, the AAAS is going through a painful process of attempting to establish goals, reappraise its role in relation to clientele groups, and so on. See Phillip M. Boffey, "AAAS (I): Facing the Question of What it Should Be and Do," *Science*, 30 April 1971; "AAAS (II): Asking How to Be More Than a Meeting and a Magazine," *Science*, 7 May 1971; "AAAS (III): Is Order of Magnitude Expansion a Reasonable Goal?" *Science*, 14 May 1971.

20. Derek J. de Solla Price, *Little Science, Big Science*, pp. 62–91.

21. The operation of boards and panels, as well as the higher policy advisory units, some have observed, is on an apprenticeship system whereby researchers who serve on agency advisory panels are moved up the ranks. They are, so to speak, trained, that is, socialized in the scientific leadership culture. See the editorial "The Same Old Gang?" *Science*, 23 August 1963, p. 677.

22. J. Stefan Dupré and Sanford A. Lakoff, *Science and the Nation*, p. 73.

23. U.S. Congress, House, *National Academy of Sciences Panel on Basic Research and National Goals: Hearings Before House Committee on Science and Astronautics*, 89th Cong., 1st sess., p. 3.

24. For a more detailed history of the NRC as well as a discussion of current disputes regarding its formal organization and operation, see John Walsh, "National Research Council: And How It Got That Way," *Science*, 16 April 1971, pp. 242–46, and "National Research Council (II): Answering the Right Questions?" *Science*, 23 April 1971, pp. 353–57.

25. President's Scientific Research Board, *Science and Public Policy*.

26. Quoted in Reagen, *Science and the Federal Patron*, p. 99.

27. Lately there has been pressure to add researchers from other disciplines to these panels. It would be interesting to see how such interdisciplinary experiments come out. Specialists usually cooperate only if they see such cooperation as functionally necessary. A change in policy on the makeup of panels gives no necessary evidence that this condition exists. Moreover, whether they admit it or not, members of disciplines tend to view other disciplines as competitors for scarce resources.

28. Charles V. Kidd, *American Universities and Federal Research*, p. 108.

29. Michael Polyani, *The Logic of Liberty*, p. 55.

30. The account in this section draws heavily on Harold Green and Alan Rosenthal, *Government of the Atom*; Warren O. Hagstrom, *The Scientific Community*; and Harold Orlans, *Contracting for Atoms*.

31. These events are discussed in Bernard Barber, *Science and the Social Order*, p. 164. Also see Deborah Shapley "FAS: Reviving Lobby Battles ABM, Scientists' Apathy," *Science*, 26 March 1971, pp. 1224–27.

32. A study that brings this out nicely is Marver Bernstein's *Regulating Business by Independent Commissions*.

33. For an interesting account of this policy conflict which came to be involved in the infamous Oppenheimer security hearings, see Philip M. Stern, *The Oppenheimer Case*.

34. The data were analyzed by Orlans, *Contracting for Atoms*, p. 189.

35. Ibid., p. 164.

36. Excerpts from the relevant acts are reprinted in Green and Rosenthal, *Government of the Atom*, pp. 71–114.

37. Orlans, *Contracting for Atoms*, pp. 43–44, 220.

38. Quoted in Reagen, *Science and the Federal Patron*, p. 44.

39. See Boyd R. Keenan, "High Energy Administration," pp. 250–55.

40. Hagstrom, *The Scientific Community*, 143–47.

41. These data are derived from the National Science Foundation, *Basic Research, Applied Research, Development in Industry*.

42. Edward B. Roberts, "How the United States Buys Research," *International Science and Technology*, September 1964. Reprinted in U.S. Congress, Senate, *Impact of Federal Research and Development Policies on Scientific and Technical Manpower*, pp. 11–19.

43. National Science Board, *Graduate Education*.

44. Actual accounts of the stimulation of creativity because a critical mass had been organized (formally or informally) can be found. On the basic research side, see James D. Watson, *The Double Helix*, where the more than accidental interaction of Watson, Crick, and others working at Cambridge accelerated their work on DNA. On the development side, an excellent account exists of the critical mass effect in the German Army rocket-missile program of World War II, see David Irving, *The Mare's Nest*. Wernher von Braun and his colleagues interacted in this way from the days of their original amateur club, the *Verein für Raumschiffahrt*, to the days of the final development of the V2 rocket at Kummersdorf and later at Peenemünde.

45. Many of the data for this section were originally gathered by this writer and Daniel Alpert for another study. See chapter 1, note 16 (document, pp. 50–58).

46. See table 9, in *Impact of Federal Research and Development Policies on Scientific and Technical Manpower*, p. 484.

47. Allan M. Cartter, *An Assessment of Quality in Graduate Education*.

48. National Science Board, *Graduate Education*, p. 62 n. 13.

49. U.S. Congress, Senate, *Equitable Distribution of R & D Funds by Government Agencies*.

50. Philip Abelson, "Distribution of Research Funds," *Science*, 25 October 1963, p. 453.

51. Barber, *Science and the Social Order*, p. 169.

52. Greenberg, *Politics of Pure Science*, p. 6.

53. Talcott Parsons, "The Science Legislation and the Role of the Social Sciences," p. 657.

54. Robert K. Merton, "The Matthew Effect in Science," pp. 56–63.

DON K. PRICE

Endless Frontier or Bureaucratic Morass?

ENTANGLED IN THE PROCEDURAL CONSTRAINTS that go with government money, some scientists are almost tempted to regret the implicit bargain on which massive federal research support has been based since the Second World War. The political strategy they have followed since Vannevar Bush's classic report of 1945, *Science the Endless Frontier*,[1] may need to be thought through again in the light of new circumstances and new public attitudes.

It may be that the world of science is not unique in this way, but only a few jumps ahead of other institutions in American society. C. P. Snow, we may recall, argued that scientists had the future in their bones. It may unfortunately, in matters of politics, be more in their bones than in their brains, which often contain the same obsolete political myths in which other Americans devoutly believe.

But there is no doubt that the scientific community is ahead of the pack in one respect, and one which has forced it to rethink its relationship to politics. While business leaders speculate about the future limits to economic growth, American scientists have already had to adjust to a static or even a declining level of financial support.

In the scholarly world, it is fashionable here and there to argue that it will be a good thing to disavow materialistic values, at least for the world of business and industry, and to accept with equanimity a static GNP, declining use of energy, and the philosophy of "small is beautiful." It is somewhat more rare to find scientists who apply the same line of argument to the financial support of research. On the contrary, the scientific community generally is aware that the reduction in federal support, and the resulting cessation of its economic growth, make it much more difficult to deal with many problems in the administration of research programs, and indeed in the academic world as a whole.

Perhaps the most troublesome of these problems is the recent increase in legal and administrative constraints that severely limit the autonomy of university administration and the freedom of research workers—constraints that are imposed not in the interest of the quality of research but of short-term practical results, regional distribution of funds, or other criteria more or less irrelevant to scientific excellence. Almost equally annoying to scientists is the change in political attitudes; members of Congress now tend to look on them as just another selfish pressure group, and not as the wizards of perpetual progress.

In general terms, it would be easy to get agreement within the scientific community on the remedy: more generous federal support on a longer-term basis with fewer irrelevant conditions and regulations, and more recognition of the principle that the excellence of research in American universities is a matter of profound national interest.[2] But agreement in general terms by institutional executives does not provide effective political support when their constituency is apathetic or divided.

University research scientists (this paper will concentrate on university research, and neglect the important world of industry, which in recent years has been cutting back on support of basic research) seem in recent years to have taken for granted a degree of freedom and financial support that depended on a novel and fragile arrangement. It has been easy to forget the atmosphere of the time, less than a half-century ago, when the federal government considered research grants to private universities improper if not unconstitutional, and when the leaders of the National Academy of Sciences objected on principle to letting private universities accept government funds.[3]

Many scientists who have become addicted to federal support are now inclined to wonder whether their old leaders were right. While they are unable to swear off periodic injections of government grants, they repent of their addiction, and blame the bureaucratic constraints they must endure on the same three bogies that frightened scientists in the 1930s. These, in caricature, are politicians (by definition bad people), antiintellectuals who wish to impose ideological or theological constraints on research, and a bureaucracy dominated by an overmighty executive.

It would be more plausible, it seems to me, to blame undue constraints on causes that are almost the opposite in nature. This list, equally a caricature and not to be defended literally, would put the main causes thus: first, a political strategy for the support of science that was devised by scientists themselves and based on the experience of private philanthropy; second, the acceptance by political authorities of science as the dominant intellectual approach to public issues, which scientists and other liberal intellectuals agree must therefore be regulated in the public interest; and third, a constitutional structure too decentralized to sustain the integrated and long-term view of public policy which might justify the support of science as an intellectual and educational enterprise.

I would be more inclined to defend this second list of causes than the first, but both have some elements of truth, and neither is likely to serve as the basis of a constructive political strategy, which requires something better than mutual recrimination between the worlds of politics and of science.

As a first step toward thinking through such a strategy, let us recall the way in which the present research support system developed after World War II.

Politics as the Source of the Problem

Politics is often thought of, in derogatory terms, as nothing but the way in which people get power through the process of popular election. That kind of politics had remarkably little to do with the change from 1940, when the federal government paid for one-fifth of the nation's research, to 1975, when it paid for well over half. The growth in support for science was never a big electoral or partisan issue.

But there are other bases for political power. Religion and wealth are among the older ones. And the experience of the past generation or two teaches us that newer forms of social organization are politically powerful, notably the professions and the scholarly and academic institutions. Organized knowledge as a political force should no longer be underestimated.[4]

The conversion of the United States federal government to the support of scientific research after World War II was a brilliant feat of political strategy. Its quality should not be measured in terms of dollars alone. Other countries greatly increased their financial support of research over the same period. But nowhere else did a group of scientists manage to persuade their governments and their research institutions not only to give up their scruples against public support of private science, but also to work out the arrangements for distributing the support that maximized the political influence of those whose authority was based on knowledge rather than on votes. These arrangements depended on a tacit bargain that dealt with both political ends and means.

As for the ends, the Congress had to be persuaded that basic research was a necessary foundation for practical progress, and that applied results would flow automatically from it. With the wartime experience as evidence, this argument, the core of the Bush report, proved convincing. It led to a continuing bargain by which scientists would get support for basic research which government officials would hope would lead to applied research and on to useful developments.

As for the means, it seemed necessary to free basic science in several ways from the constraints inherent in the older federal research programs, such as agricultural research in the state experiment stations and research in government laboratories like those of the National Bureau of Standards. The main constraint, of course, had been the exclusion of private universities from the use of federal funds. But if they were to be included, it was important to give them more freedom than could be provided under the agricultural research pattern, with its emphasis on the distribution of funds among the states by statutory formulas that gave no weight to differences in quality,[5] or than could be provided for the government laboratories, which got their money through line item budgets by grace of congressional committees. Both arrangements led to heavy emphasis on applied rather than basic research.

To get away from such constraints, a three-part tactic was adopted. It was not, I suppose, expressly formulated as a tactic by anyone. But all three of its parts reflected the experience of the scientific leadership of the time with private foundations,[6] which had proved the most effective way of organizing financial power so as to give weight to scientific values. It must have seemed only natural to use similar arrangements to give weight to organized science as a counterbalance to the power flowing from the electorate. What worked with Rockefeller and Carnegie money ought to work with congressional appropriations.

The first tactic was to combine research with university teaching. The United States was not unique after 1945 in increasing support for research and development, but it was unique in the extent to which it supported such work through grants to universities. The separate research institute dominates basic research in the Soviet Union and many other countries; even in the United

Kingdom and Canada the universities get a smaller share than in the United States of the government's total research program (including applied research).

The tactic had its precedents in private philanthropy. Just after the First World War, the National Research Council's negotiations with the Rockefeller Foundation for the support of science turned initially on hopes for a series of special research institutes in physics and chemistry. There were, after all, the models of the Carnegie Institution of Washington and the Rockefeller Institute for Medical Research. But the Rockefeller Foundation and the National Research Council threw their weight behind the NRC fellowship program as the best way of strengthening basic science in the one setting most free of commercial self-interest or political pressure—the university.[7]

The university seemed then, and still seems, the most stable basis on which to defend the independence of science. With independent endowment funds and a senior faculty on permanent tenure, it was in a position to defend freedom of inquiry against popular passions or economic self-interest. State universities, more vulnerable in some ways because of their financial dependence on legislatures, came to imitate the private universities in the security afforded their permanent tenure faculty. (Much later, in 1955, even when dealing with so political an issue as military strategy, it seemed reasonable to try to give the new Institute for Defense Analyses a status independent of the source of its funds by establishing it under a consortium of universities.)

The second tactic was the project grant. It had been developed in various private foundations, most notably the Rockefeller Foundation and the Carnegie Corporation, which shortly began to lose interest in making endowment grants. The main purpose of the project grant was doubtless to make it possible for a foundation with specific purposes to make sure that its funds were devoted to them, and not merged with more general programs, or abused by faculty politics, or neglected by weak university administrations. To the scrupulous foundation executive, the record of universities in using general grants seemed discouraging. The project grant seemed a useful and efficient way to influence the conduct of research in the direction of social purposes, and away from excessive disciplinary specialization.

But a procedural effect was to give more influence to the professional staff of a foundation by comparison with its donor or trustees. In order to make an institutional endowment, a single trusted advisor to a great philanthropist, or at most a tiny staff, could negotiate a major gift and a philanthropist or his trustees could be expected to understand it and make the essential decision. With a project grant dealing with a highly specialized science, most trustees had to leave the judgment to their expert staffs, and have staffs large enough to follow up on the project, or even play some role in its direction. Some of the older generation of foundation executives were contemptuous of this trend, believing that the better role for philanthropy was to build independent institutions and leave the specialized decisions to them,[8] but in recent years the project grant's apparent advantages led the major new foundations, such as the Ford and the Robert Wood Johnson foundations, to adopt it as their main mode of operation.

For the federal government just after World War II, the project grant seemed appealing to scientists because it offered a tactic to avoid detailed congressional control of funds, such as prevented the Department of Agriculture or the Bureau of Standards from becoming preeminent centers of basic research. Just as in private philanthropy, it would be hard to persuade those with ultimate power (in this case, members of Congress) to defer to experts in deciding on broad program grants or institutional grants. But if the grants were to support highly specific and specialized work, Congress could be persuaded that the merit of the project and of the investigator were the only appropriate criteria, and that such merits could be judged only by scientific peers. Moreover, it let the Congress sidestep various troublesome issues: through the project grant, support could be given to universities without adopting a general program of aid to higher education or discriminating between state and private institutions, and above all without facing the issue of church and state. But incidentally, it put the staff members of the granting agencies (themselves under pressure from congressional committees) in a position to review the conduct of research projects. And it probably weakened the influence of university administrations and the commitment of individual scientists to their institutions, by comparison with their commitment to the sources of their research grants.

The third tactic was the pattern of organization, including the personnel system, by which the community of science was to be given a political authority that did not depend on popular votes. The most obvious part of this tactic was the creation of the National Science Foundation and its board, on the model of the private foundation, and the vesting of substantial authority by law in the advisory councils of the National Institutes of Health.[9]

But the most striking shift in political influence to the scientific community did not come from the board or committee pattern of organization. It came instead from the system of personnel, and from the support of the processes of policy planning by part-time advisers under government grants and contracts. Before World War II Congress was still prejudiced against letting anyone other than full-time government officials gain any control over governmental decisions. This prejudice was breaking down slowly. Civilian departments, which before the New Deal had typically no statutory authority to employ part-time consultants, had been lured into using them by the offer of philanthropic funds for the purpose, and had continued the habit with subsequent congressional sanction.[10] But the great increase in the use of consultants and advisory committees came after the Second World War, as those scientists who had served in the war agencies went back to their private status, but continued to maintain substantial influence over government programs in the vast pyramid of committees and advisory councils of which the President's Science Advisory Committee became the apex. Working from a tenured position in a university, a scientist could, on a part-time basis, play a more painless and perhaps a more influential role in public policy by membership on advisory committees, by participation in summer study projects or formal "evaluations" of current programs, and by serving as an individual consultant to short-term and harried political executives, than if he accepted the burdens and responsibilities and vulnerable status of a full-time position.

These three tactics together made a major shift in the balance of political authority between the type of politics based on votes and the type of politics based on knowledge or scientific status. But the authority gained by the scientific community was not of the kind that could be defended as a matter of constitutional right; it was a delegated authority, and it depended on the continued confidence among elected politicians in the assumption on which the tacit bargain was founded—that basic research would lead automatically to fruitful developments.

It is obvious that that confidence was shaken during the 1960s, and not only among members of Congress. The change in congressional attitudes may have been affected somewhat by the general malaise in the intellectual world, some parts of which, including scientists themselves, were beginning to doubt the omnicompetence of science in human affairs. But it probably resulted more from the normal disposition of anyone who lends or grants money to want to know what use is being made of it, and whether the terms of the bargain are being kept. As universities became very big businesses indeed and dependent on research grants for their budgets, and as professors became simultaneously advisers on government policy and salesmen for their project applications, each of the three tactics that had given the scientific community such status just after the Second World War came to be looked on with cynical suspicion by government administrative officers and members of Congress alike. One may speculate whether the general status of basic science might not have been protected better if more professors had concentrated their political energies on advocating the case for general support with fewer restrictions, rather than on advocating particular issues of public policy or soliciting specific project grants.

The problem is of course a political problem, in the general sense of the word "political." But it would be a mistake to think that it would be solved if scientists never took another grant from the government. The patterns of governmental organization and procedures for the support of basic research were installed in government by the political action of scientists, in imitation of the patterns of private philanthropy. As government agencies have tended in recent years to put more conditions on their research grants, so have private foundations. The most restrictive controls are subtle and not reduced to contractual terms (just as the congressional restrictions on executive bureaus are often in the legislative history rather than in the provisions of statutes). Accordingly, I think it would be difficult to measure the degrees of constraint that go with government research grants by comparison with those from private foundations or corporations, but from my own experience I would guess that the private grants are on the average more restrictive; private foundation staffs may have more prestige, and certainly are granted more leeway by their own superiors, than their governmental counterparts. And they are paid more.

The comparison is not very relevant, since the volume of government support is far beyond the capacity of private institutions to sustain. Private foundations have long since unloaded on government their most expensive areas of research support. But if such support could conceivably be transferred back to foundations and business corporations, one must remember that what they too do in philanthropy is now controlled, in some degree, by government as a matter of tax policy, administrative regulations, and contractual conditions. Government agencies now behave more like private institutions than they did before the Second World War, and private institutions are now more like government agencies in their subordination to public policy. Science and technology, by demonstrating the flexible possibilities of administering government programs through grants or contracts to private institutions (remember the atomic bomb and the man on the moon, as does every member of Congress), have had a lot to do with the change, which scientists are less inclined to appreciate than they might be.[11]

The Purposes of Constraints on Research

In the rich variety of American politics, there are plenty of examples of benighted and reactionary politicians who distrust intellectuals and academic institutions. Accordingly, it is a great temptation to blame them for any or all of the constraints which public authorities have placed on university science in recent years. But on the whole it seems likely that the constraints about which universities and scientists complain the most are the result of liberal reforms and egalitarian pressures, which are advocated more generally by intellectual reformers than by backwoods reactionaries. Let us look at the apparent purposes of the several types of constraints.

REGULATORY RED TAPE

The constraints that have been most burdensome in financial and administrative ways have had little to do with, or at least were not intended to affect, the substance of scientific work. They have included the clerical and managerial burdens of social security taxes, equal opportunity and affirmative action requirements, environmental protection, occupational health and safety, and free access to information (such as the proceedings of advisory committees or letters of recommendation) previously considered confidential. These were originally considered liberal reforms. As they come to impinge on universities as well as on industry, it is interesting to observe how in liberal university circles there is coming to be more tolerance for the belief in laissez-faire policies that was formerly considered the mark of benighted reactionaries.

It is hard to imagine that research, whether in universities or in industry, can be completely exempt from regulations that government imposes on other institutions in what has come to be considered the public interest. But it is possible to consider ways in which such regulations may be adjusted to the peculiar circumstances of scientific research or of academic administration. Before the Office of Scientific Research and Development undertook the support of wartime research in universities by the contract device, no one assumed that government research contracts could be let in broad terms, omitting the detailed procedural checks that material procurement contracts contain to protect the taxpayer. But such contracts were devised and used, some of them giving universities as much freedom as grants. As time went on, however, and as competition for funds increased and abuses in university management were discovered by zealous government investigators, the terms of contracts were tightened by various agencies.[12]

It should be possible for government agencies to loosen up the red tape that now entangles the grant and contract procedure, on grounds that the nature of research work is such that detailed requirements are counterproductive and cost the government more in terms of final product than they are worth. But contracting officers themselves are under pressure from congressional committees to prevent misuse of federal funds, and their best protection is often to apply uniform rules, or to base requirements on objective and quantitative criteria. Both tendencies make enforcement easier, and preclude legalistic objections. But while eliminating arbitary authority, they also eliminate the use of good judgment or the consideration of general values that may be significant but hard to quantify. "Zero tolerance" of carcinogens, or uniform statistical goals for affirmative action, are good examples.

It is unlikely that scientists can be totally exempt from the constraints that regulatory red tape imposes on all other parts of society. Their relief may come in part as their institutions prove that they are capable of policing their own performance more adequately, and in part as the government improves the general quality of its regulatory activities, giving more weight to broad judgment and less to detailed controls. The best example of such improvement in recent years has been in an agency that oversees other government agencies: the General Accounting Office, which in 1946 had a staff of 14,904, most of them clerks and auditors and only three percent with professional qualifications, had by 1976 cut its total staff to 5,351 by raising their quality (seventy-two percent were by now professionals) and delegating the detailed checking of accounts to the agencies that it supervises.[13]

If a similar type of improvement can be made in the supervision of grants and contracts with universities and other private institutions, which are equally entitled with federal bureaus to argue that they can spend government funds more efficiently under broad delegation rather than detailed review, it can be done only if the public officials concerned can be less numerous, of higher professional status and quality, and more secure politically in their ability to make discretionary decisions, rather than having to justify their actions by narrow and legalistic procedures.

THE ATTACK ON ELITISM

A more general constraint on the quality of university research comes from the attack on elitism in the distribution of research funds. The project grant system, and the "peer review" method by which it distributed funds, produced a concentration of grants in a limited number of universities. The tactic of the project system had worked; by contrast with the agricultural research program, money had been distributed not by geographical formula but by judgment regarding excellence through "peer review," in the original meaning of "peer." In an American jury trial, the judgment of one's peers means judgment by a random selection of citizens; but scientific "peer review" goes back to the original meaning, in which one should be judged only by those with competence to pass judgment—one's equals.

In reaction during the 1960s against the apparent concentration of federal grants, the National Science Foundation, the National Institutes of Health, and other grant-making agencies devised procedures to distribute funds more equally, such as making institutional or broad departmental grants, or giving funds for advanced fellowship training to institutions rather than directly to individuals who would tend to congregate in a small number of universities with highest prestige. During the same period, various states undertook to shift their expenditures for higher education away from the major research universities, and into the support of community colleges, while federal expenditures for postsecondary education shifted toward remedial and vocational education.[14]

These shifts in policy seemed to work to the disadvantage of the leading research universities. Excellence in fundamental research is not something that can be distributed by statute uniformly among the states and cities. In some fields of knowledge, private foundations had discovered that some subjects could be developed only by a very limited number of universities, and that the effort to spread the work would prevent the development of enough "critical mass" to be effective; certain types of foreign area studies are good examples. If there are about two hundred universities that grant the Ph.D. degree, that is probably more than can be justified on a national perspective. But should the number of major centers, receiving federal support, be ten or be fifty or a hundred? There is probably no good way to answer that question with confidence, but two observations about the process of considering it may be pertinent.

First, professional politicians are not the original source of the egalitarian or antielitist movement. In such matters, members of Congress reflect opinions more than they invent them. The community of science is not homogeneous. Opinions as to the merit of concentration of research resources are not the same at a meeting of the National Academy of Sciences as at the American Association for the Advancement of Science, the one representing the research peerage, the other open to anyone with a twenty-eight dollar membership fee and thus constituting, as Gerald Holton used to remark, the lower house of the parliament of science. And they clearly differ between the Association of American Universities and the National Association of State Universities and Land-Grant Colleges. It would have been a miracle if the concentrating effects of the political tactic of the project grant had not been noted with envy by colleges and universities which were coming out badly in the competitive system, and if they, constituting a considerable majority of institutions, had not been able to muster political pressure in favor of a more egalitarian distribution of funds.

Second, no one knows how to make and defend a frankly elitist decision about the distribution of federal funds to the best universities, within the American constitutional system. To make such a decision requires the existence of a central institution with great discretionary authority, or with a settled national tradition to guide it. In Great Britain, the University Grants Committee has made such decisions, but it was set up so that it was shielded from questioning either by the House of Commons or the Treasury; to imitate its effects, the United States had to adopt an apparently opposite pattern through the project grant system. In California, the state universities get preferential treatment over the state colleges and the community colleges, but the universities themselves are distributed regionally so as to reduce geographical jealousies.

The heads of the major research universities themselves are more aware than anyone else of this point; the recent plea of fifteen of them for more federal research support concluded that is was impossible for it to be expended through institutional grants to "the jostling and ever-changing set of universities that someone thinks are at the top."[15]

CONTROL OF SUBSTANCE OF RESEARCH

Much more dangerous to the essential freedom of science than regulatory red tape, or than pressure for the egalitarian distribution of funds, is the kind of political pressure that threatens to bring about thought control or ideological conformity, by constraints on the substance of research.

Some such constraints arise in ways that harass individuals but are only minor irritants to the scientific community as whole, such as the publicity tactic of a legislator who ridicules, to impress his constituents, the title of a research project that seems absurd or overly technical to the lay reader. Others deal with issues of public health, safety, or morals, as do regulations dealing with DNA experiments, or medical experiments on prisoners, or experiments involving cruelty to animals. These issues may be troublesome and difficult, but generally they do not involve political efforts to control the outcome of research or its inner methodology, or to impose on it a political ideology. And they are stirred up as often by scientists themselves as by politicians, as in the case of DNA.[16]

The greatest challenge to the use of scientific methods has come in attacks on the social sciences, which seem to the layman more obviously to threaten some types of political or religious values. A generation ago, this threat seemed ominous enough. The congressional investigation headed by Congressman B. Carroll Reece from Tennessee undertook to prove that private foundations ought to lose their tax exemption because they supported the social sciences, which were promoting socialism within the United States.[17] These seem, in retrospect, more a farce than a danger, but they were taken seriously enough by social scientists at the time. That was the period in which the National Science Foundation, perhaps recalling the way in which the natural scientists who lobbied for its creation had refused to include the social sciences in their proposal, had smuggled them in under the title of "other sciences," but in severely limited amounts.

More recently, the social sciences have received government support in amounts that are still small by comparison with the physical and biological sciences, but that would have seemed munificent a few decades ago. They still differ from the natural sciences in the political constraints they suffer, but much more as a matter of degree than of kind. All the sciences together are the political victims of their own success, in two ways: first, they come under pressure to contribute more directly to public purposes, and second, they are patronized or punished not for the knowledge they produce, but for the anticipated use to which it might be put.[18]

The pressure to contribute more directly to public purposes comes in part from the effort of politicians to collect on what they consider valid IOUs. Scientists speaking to scientists would urge the support of basic research by mission-oriented agencies (the Department of Defense by contrast with the National

Science Foundation, for example) in order to protect the freedom of scientists to deal with a variety of possible donors. But scientists speaking to congressional committees would naturally argue for the utility of basic research, at least within a relevant discipline, to the practical mission of the executive agency. In the eyes of a basic investigator, of course, any fundamental advance in science might be relevant to almost any mission, while relevance was defined more narrowly by congressmen accustomed to strict construction of statutes by auditors and lawyers. The amendment inserted by Senator Mike Mansfield in the Military Procurement Authorization Act of 1975 required the military services to support only such basic research as had "a direct and apparent relationship to a specific military function or operation." The effect turned less on verbal definition than on the general intent of the legislative history; although the amendment is no longer law, it stills puts a substantial damper on the discretion of military contract officers to contribute to the nation's basic research program.

Even more limiting to the basic research program was the growing tendency in Congress to shift funds in the direction of applied research. This was like the earlier move of private foundations to favor "problem oriented" rather than strictly disciplinary research.[19] Those members of Congress who favored the shift were not antiintellectuals or enemies of science; the Research Applied to National Needs program was pressed on the National Science Foundation by its warmest sponsors in the House Committee on Science and Astronautics, including Representative George P. Miller from California and Representative Emilio Q. Daddario from Connecticut. And such shifts were not forced on the scientific community entirely from the outside; one may recall Edward Teller's complaint that the United States had put proportionally too much emphasis on basic science by comparison with technology,[20] or the great numbers of biological and medical scientists who joined the lobby led by Mary Lasker in support of research dealing with specific diseases, or the ecologists and environmental scientists who linked their research not only to technological development but also to political action.

The danger of political constraints on science now comes not so much because politicians disapprove of the methods of science, but because they take seriously what some scientists tell them about the way in which scientific discovery leads to practical benefits. Once they believe that, it is inevitable that they should try to control practical outcomes by anticipating the effects of research, and manipulating it in one way or another.

In its simplest form, this manipulation consists in voting to support or to abolish research work leading toward intended purposes. Thus at one time in dairying states, research on the economic and nutritional aspects of the use of oleomargarine proved to be impossible in state universities,[21] and thus Congress may increase or decrease support for various fields of research in the light of its estimate of their relevance to specific purposes.

Less crudely and more indirectly, a legislature may seek to alter the structure of government organization in order to favor or punish research with policy implications. When Congress thought, in the fading years of the New Deal during the Second World War, that the National Resources Planning Board's research was leading toward economic planning, it abolished the board. When some congressmen thought that the development of ocean resources was being neglected, they tried to legislate the creation of a special oceanographic unit in the Office of Science and Technology of the Executive Office of the President.

Finally, and still more indirectly, legislative action may undertake to push scientific research directly into political issues, without knowing in advance just how the research will come out. In recent years Congress has by statute required the National Academy of Sciences to undertake more than two dozen major studies designed to answer policy questions. Some may have been voted by members of Congress who did not know what answers they wanted. Others had more obvious motives, as when Representative Jamie L. Whitten of Mississippi, unhappy about restrictions on the use of DDT in controlling the boll weevil, got a statute enacted to require the Environmental Protection Agency to contract with the Academy for a study of its own procedures.[22]

Similarly, many statutes now require that executive agencies contract with research firms to evaluate the quality of their programs; such evaluations may be based on the most modern techniques of social science, but they put a political strain on the ethics of many contractors, who are torn between the obligation of objectivity and the desire to qualify for another contract.

And finally, we move into the stage where science joins the demand for maximum feasible participation in democratic processes. Public interest groups engaged in criticizing or opposing official policies plead for congressional appropriations to support their activities, and those dealing with technological issues now look forward to the new "Science for Citizens" program of the National Science Foundation. This program, which the Senate seeks to expand from modest beginnings, would pay for the work of not-for-profit groups to employ scientists to work on public policy issues. The program is looked on with some apprehension by some members of Congress who fear it will, for example, fund the lobbying by public interest groups against nuclear power plants.[23]

In all these ways scientific research is involved in political controversy, and comes under political constraints. But it is important to note that none of them is the result of an effort by politicians to constrain science in the interest of religious dogma or political ideology. It may be good for the morale of a scientist who is caught up in political difficulties to make speeches recalling Galileo's suppression by the Church, or Lysenko's support by the Party. But in contemporary America, the main problem is just the opposite. Politicians have been persuaded by scientists that political issues can be solved by scientific methods, and hence use political power to increase the proportion of research work that is devoted to applied problems, to increase the amount of support that is given to institutions and programs for political reasons, and to drag (or welcome) scientists into political activity. All this may work against the best interest of basic research, which must be supported on a longer time perspective and with different motives, and for highest quality must depend on the work of a smaller proportion of interested talent. But it is not moving in the direction of thought control in the interest of a coherent ideology, but rather in the direction of dissipation of the energy of scientific institutions by too much involvement in the specialized practical interests that are the stuff of politics.

Bureaucracy and Executive Power

With the shadow of Watergate still hanging over our political memory, it is tempting to blame any political constraint, especially if it affects science or universities, on an overly powerful Executive controlling a disciplined mass of faceless bureaucrats. This conventional diagnosis is particularly inappropriate when dealing with issues in which scientific and political problems intersect, for four obvious reasons, in all of which the United States is nearly unique even among Western democracies.

1. In the United States' career civil service, scientists have higher status than general administrators. Two-thirds of the supergrade civil servants have come up from scientific, technical, or professional backgrounds. Unlike the corps of generalists or administrative lawyers that dominate the civil services of Great Britain or Western Europe, the United States' civil service has no unified administrative corps dominating its departments, and under the top political appointees authority is shared by career officers and short-term appointees from universities and private business.
2. Through specialized committees, Congress maintains oversight over the specialized bureaus rather more intimately than can the president's staff, and regularly amends executive proposals and especially executive budgets. In democratic parliamentary systems, there is no equivalent control in such specialized detail. Hence scientists in the United States, both within the government and outside it, have relatively free opportunity to know what goes on within official circles, and to influence congressional action on it.
3. The Executive does not have control over the organizational structure or the personnel system of the government, and the Congress manipulates both as a means of controlling policy.
4. Within the Congress itself, the individual members and committees do not permit the existence of any leadership (on the basis of party or otherwise) that can control the legislative proceedings, in the way that a Cabinet does in the classic parliamentary system. It is this kind of political discipline within the legislative body that makes possible in some democratic countries the existence of a strong chief executive, and the maintenance under him of a strong bureaucracy.

In its effect on the political status of science, the loose American system is the opposite of a strong bureaucracy. Executive authority lacks the authority not only to give the answer to a political issue, but to define the question. Science is seen as the most reliable basis for solving problems, but scientists are asked to answer not only specific questions to which their discipline may have the key, but broad questions of policy involving other specialized disciplines and a vast range of unscientific value judgments as well. In this setting, it does not matter that the typical complex issue of policy cannot be answered by a single scientific specialty; a congressional committee may insist that a nuclear physicist give it the word on arms control, or a biologist on the morality of abortion. This is an inviting opportunity for an eminent specialist endowed with high moral zeal and no sense of humor.

This system is very good at dealing with narrow and highly specialized problems, but less satisfactory with broader issues involving the coordination or reconciliation of competing values. That shortcoming is apparent not only when science undertakes to help solve issues of general policy, but also when it tries to deal with policy for the support of science. The peer review system, for example, works very well at deciding on project grants within the limits of a given set of universities and institutions, and a given balance of support as among scientific disciplines. It is incapable, however, of dealing with the broader issues that arise in proposals for institutional grants, of deciding between support of universities and special research institutes, or of determining how much money should go to chemistry as against biology or economics.[24] By default, such decisions are made (or recommended) by the staffs of congressional committees or of the Office of Management and Budget.

Science as a critical and skeptical mode of thought, more interested in pragmatic tests than in verbal theories, has probably been the main influence in dissolving traditional authority and in emphasizing the authority of the specialist, in all aspects of American life. When a university promotion is influenced by a numerical count of scholarly papers, when a government agency grants financial support in proportion to items that can be counted rather than on a general estimate of excellence, or indeed when civil service appointments are made by "objective" tests or the president must control his departments more by budget allocations based on statistical criteria than by loyalty to general policies—in all these ways we may observe the American overemphasis on the specialized and the quantifiable. And a good thing too, in my opinion, if we must go to one extreme or the other.

But in political affairs the extremes are just what it is important to avoid. To deal with the big problems of modern society requires the intellectual contribution of scholars and public officials who can take comprehensive and integrated (the currently fashionable word is "holistic") views. If they are to have a chance, it also requires institutional systems in universities, in business, and in government that encourage the broad and integrated view, and the long-term perspective on which it must be based, and the respect for the fundamental values of civilization on which its institutional structure must depend.

The protection of scientific inquiry from improper limitation by governmental authority does not require scientists to renounce an interest in public policy, or the rest of us to adopt a romantic notion that modern problems can be solved either by a return to primitive values, or by popular votes unaided by expert opinion. Facts and values (like policy and administration) are far too intricately mixed, far too reciprocally dependent on each other, for us to guarantee the autonomy of science (or the integrity and impartiality of the civil service) by forcing it to renounce a role in the great public issues of the day.

It helps, perhaps, to make a clear distinction in our thinking among science, scientists, and scientific institutions. Science is a mode of thought that it is important to distinguish from other approaches to human problems, even though it profoundly affects all of them. Scientists are citizens too, and ought to be encouraged to participate fully in politics, to which they may make a unique contribution as long as they make clear the limits of their competence as scientists to answer unscientific questions. But scientific institutions, including universities, may be under a special obligation, if they ask for governmental support, to emphasize the difference between their role and function and those of governmental agencies.

It is on a clear understanding of this difference that the universities' case for generous support and freedom from political constraint must depend. The universities can advance science and educate scientists, and science and scientists can then make great contributions to the general welfare, only if the universities are supported on a steady and long-term basis, and are free from political interference, including the benign type of interference that consists in requests to undertake work that is more suitable for a consulting firm or a government agency.

The protection of basic research against constraints is not mainly a matter of protecting one unitary force, the university, against a superior one, the government. If the institutional worlds of science and of politics have been badly mixed up over the past quarter-century, it has been less a case of violation by superior force than of mutual seduction. To work toward a future relationship of more adequate support, it will not be enough for university presidents to state correct policies. It will be even more important for the university scientists themselves to understand how they got into their present situation, as a guide to knowing how to get out of it.

The problem has not been one growing exclusively out of the demands of government; as the record of private philanthropy suggests, it has rather been one of a growing disposition on the part of many organized sources of support (and of the scientific community's more aggressive salesmen) to emphasize practical results rather than basic research and education. It has not been the assertion of political ideology or religious dogma over objective science; rather it has been the overreliance of political leaders on the fragmented authority of scientific specialties, and on the reductionist methods that they have popularized. And it has certainly not been the result of powerful executive or bureaucratic authority; the same overspecialized patterns of thought that afflict the academic community are built deep into the institutional structure and incentive systems of the Congress and the administration alike.

A Start toward a Strategy

In planning a strategy to reduce undue political or bureaucratic constraints on research, the first question may be whether to emphasize the similarities or differences between the world of scientific research and other parts of society. It is tempting, of course, to argue, and it is certainly partly true, that science, research, and indeed the world of academic and intellectual affairs generally are quite different from the mundane activities that may properly be expected to obey bureaucratic regulations. The practical trouble with this argument is that too many administrators of research and educational programs have been caught behaving like business managers generally, and, besides, the posture of intellectual superiority is not an ingratiating one to be assumed in a congressional committee hearing.

It is indeed possible that the political resentment against such a posture (even if the posture is more imagined by the beholder than really assumed by the scientist) has changed the nature of the relationship between governmental grantor and scientific grantee. It seemed to some during the 1950s that the distinction between governmental agencies and private research institutions was a line of defense that could continue to be held legally and administratively in order to prevent legislative and bureaucratic meddling in scientific affairs. At that time the public versus private distinction seemed a reasonably secure defense, unlike the symbolism of the corporate form of organization, which had been adopted when the government corporation was created in the 1920s to give more flexibility and discretion in the spending of government money.

But now the experience of the government corporation, which soon came to have as much red tape tied to it as ordinary government bureaus, is being repeated in the administration of government grants and contracts with private institutions; legislative conditions and administrative auditors can follow the taxpayer's dollar. Indeed, the atmosphere of the relationship has soured enough so that it may be an actual disadvantage, while facing congressional committees, to be a contract administrator representing a private institution rather than a regular civil servant. Whether or not this is so, it is clear that the same trend of imposing constraints on the spending of government money has operated in the field of contracts and grants to private institutions that formerly operated with respect to grants in aid to the states. It is obvious today that a university president who tried to assert, as a matter of legal right, that constraints should not be attached to the terms of research grants would look as silly as a state governor who still argued for states' rights in pre-Civil War terms.

But what cannot be defended as a matter of legal right may be as a matter of functional necessity. The extent to which authority is delegated in the expenditure of federal funds, or controlled in detail by congressional committees or administrative officials, varies tremendously from function to function. Scientists and educators may well argue, as I would, that it makes no sense for government to grant them money for their essential purposes, and then attach conditions to the grant that make it wasteful or ineffective. The mere fact that a program is supported by public funds does not necessarily require constraints in unwise detail, although it may often do so. If Congress is persuaded that the direction of a research program requires as much professional independence as, say, the direction of the Federal Reserve System or of the Joint Chiefs of Staff, or any other function that they consider professionally complex and important to the national interest, it will find ways to delegate in ample degree. Even some state legislatures manage to do so with respect to their state universities.

The functional approach would allow scientific institutions to avoid the appearance of pleading for unique privileges on the basis of elite and esoteric status. In practical terms, it would put them in alliance with other elements in society which might see some common interest in maintaining a degree of autonomy in a pluralist society. Some such allies would be in private institutions—business corporations or philanthropic foundations that are now willing to admit the need for government regulation, but would prefer to have it in broader and more flexible terms.

More important would be parts of government itself, for the same problems that arise in the relationship of government to private institutions arise also in the relationship of central control agencies like the Office of Management and Budget with operating agencies and bureaus. Both central control and operating agencies might be persuaded that detailed constraints are not only counterproductive but fail to deal with the broader and more important policy issues. If the General Accounting Office could make the change in that direction that it has accomplished over the past decade or two, any miracle can happen. And congressional committees, aware of the extent to which their specialization has obscured major policy issues rather than controlling them, and noting the effects of such an integrating reform as that of the Congressional Budget Office, and the potential of an effort like that of the Office of Technology Assessment, may yet see the need for focusing their attention on major issues and delegating more responsibility for details. It would be naive to expect instant results from such an approach, but even more naive to rely on the approach of promising results that basic research cannot deliver, and asking politicians to appropriate money for grants without then checking up on the results.

As long as congressional and executive staffs have incentives to react only to highly specialized and short-term pressures, no one in the political world can do much to promote the case for the sustained support of science for its fundamental and enduring values. The most effective (though perhaps the most difficult) first step toward a long-range and integrated policy of governmental support for science and for universities might well be to work for a more disciplined system of responsibility within the Congress as a whole, and a more competent corps of generalist administrators (by no means excluding scientists) who could give the planning system of the government more integrity and continuity.

But whether or not this approach would work, it would have the merit of discarding an approach based on an obsolete political mythology. It is bad enough for generals to plan to fight the last war. By now, scientists should understand that if they need to muster political support, it is not in defense of Galileo.

REFERENCES

[1] Vannevar Bush, *Science the Endless Frontier* (Washington, D.C.: U.S. Government Printing Office, July, 1945), report submitted to President Truman. The report should not be blamed for all political tactics since followed in its name.

[2] The case has been argued by officers of scientific societies and educational associations, and most recently in "An Overview: The Federal Government and the Major Research University, Indispensable Partnership for Excellence," in *Research Universities and the National Interest: A Report from Fifteen University Presidents.* To be published shortly; pre-publication draft made available by the Ford Foundation.

[3] These attitudes were responsible for the fate of the recommendations of the Science Advisory Board of 1934–35. The story is told in Lewis E. Auerbach, "Scientists in the New Deal: A Pre-War Episode in the Relations Between Science and Government in the United States," *Minerva* 3 (Winter 1965): 457–482.

[4] Cf. Daniel Bell, *The Coming of Post-Industrial Society* (New York: Basic Books, Inc., 1973)

[5] André Mayer and Jean Mayer, "Agriculture, the Island Empire," *Daedalus* (Summer 1974): 83–95.

[6] In addition to Bush himself (still head of the Carnegie Institution of Washington, which served as OSRD headquarters) the private foundation world contributed to the preparation of the Bush

report through committee members such as Warren Weaver of the Rockefeller Foundation, Henry Allen Moe of the Guggenheim Foundation, and others.

[7] Nathan Reingold, "The Case of the Disappearing Laboratory," *The American Quarterly*, 29 (Spring 1977): 79–101.

[8] Abraham Flexner, for example, as his autobiography makes plain. See *Abraham Flexner: An Autobiography* (New York: Simon and Schuster, 1960), pp. 274–275.

[9] As a stage in the evolution of the idea of a science foundation, one should not forget the short-lived National Science Fund, set up shortly before World War II at the initiative of Herbert Hoover to channel private funds into the support of science.

[10] The experiment of paying for consultants to cabinet members had been made in the early years of the New Deal by the Public Administration Clearing House with the aid of a grant from the Spelman Fund of New York.

[11] This point was argued in my *Government and Science* (New York: New York University Press, 1954), chap. 3.

[12] Bruce L. R. Smith and Joseph J. Karlesky, *The Universities in the Nation's Research Effort* (New York: Change Magazine Press, 1977), pp. 192–196.

[13] This story is told in a forthcoming study of the General Accounting Office by Frederick C. Mosher, of the University of Virginia.

[14] Smith and Karlesky, *op. cit.*, pp. 26–27, 187, 210.

[15] "An Overview," *op. cit.*

[16] Political questioning about DNA followed the Asilomar Conference; the Cambridge City Council inquiry depended on the initiative or advice of Harvard and Massachusetts Institute of Technology scientists.

[17] Tax-Exempt Foundations, Report of the Special Committee to Investigate Tax-Exempt Foundations and Comparable Organizations, House of Representatives, 83rd Cong., 2d Sess., H. Rep. 2681 (1954), pp. 17–19, 56, 60, 67, 73, 200.

[18] For a survey of the recent relations of the social sciences to government, see Charles Frankel (ed.), *Controversies and Decisions: the Social Sciences and Public Policy* (Russell Sage Foundation, New York, 1976). See also *Social and Behavioral Science Programs in the National Science Foundation*, final report of the Committee on the Social Sciences in the National Science Foundation, Assembly of Behavioral and Social Sciences, and the National Research Council Washington, D.C.: National Academy of Sciences,1976).

[19] E.g., *The Report of the Study for the Ford Foundation on Policy and Program* (Detroit: The Ford Foundation, 1949).

[20] Edward Teller, "The Evolution and Prospects for Applied Physical Science in the United States" in *Applied Science and Technological Progress*, Report to the Committee on Science and Astronautics, U.S. House of Representatives, by the National Academy of Sciences (Washington, D.C.: U.S. Government Printing Office, June, 1967), pp. 365–397.

[21] Charles M. Hardin, *Freedom in Agricultural Education* (Chicago: University of Chicago Press, 1955), pp. 119–121.

[22] "EPA Study: National Academy Set to Serve Two Masters," *Science*, 185 (August 23, 1974): 678, 680.

[23] "NSF Authorization Coming up," *Science*, 196 (June 24, 1977): 1423.

[24] This deficiency was recognized in 1965 by the report of the Wooldridge Committee, Biomedical Science and Its Administration: A Study of the National Institutes of Health (Washington, D.C.: The White House, February, 1965), pp. 10–13, 208–211. This was tacitly admitted by the series of studies by the National Academy of Sciences of the financial needs of various branches of science; no effort was made to put the several disciplines in priority order, or give them comparative weights.

LOST AT THE FRONTIER

U.S. Science and Technology Policy Adrift

Deborah Shapley/Rustum Roy

ISI PRESS®
Philadelphia

47-543—87——14

2
The Institutional Factor

WE HAVE DESCRIBED the basic-research-is-best mindset that came to dominate the culture and values of U.S. science after World War II. This chapter explores how this attitude shaped the nation's science institutions in the postwar period and some of the consequences. One way is that a whole generation of scientists has grown up in this system, feeling entitled to receive federal funds in the name of eventually producing applications, but with the real aim of doing basic research to win status according to the profession's increasingly narrow standard. This institutional factor now hinders efforts to couple science to the nation's economic and technological needs.

Much must be done by the way of institutional reform to make U.S. science as productive as possible of both basic knowledge and applications in the years ahead, but institutions are hard to change. One might whimsically compare them to ballistic missiles: once launched they follow their own trajectories; you cannot recall them; finally, it takes a veritable storm to make them change course.

This chapter will trace the growth of the institutional factor and the problems it has created. We argue that current institutional structure and management arrangements impede close links between science and applications. Moreover, the myth of the divorce between basic science and applications hurts both. Individual scientists feel they are fulfilling their profession's highest goals only when they are doing basic research and feel it is proper to ignore whether their work is contributing to some overall applications scheme.

Meanwhile, applied science policy is, quite simply, a mess. The federal government starts and stops applied science and development programs with frenetic speed, with changes in administration, changes at the assistant secretary level, changes in the heads of federal bureaus, or to appease powerful interest groups lobbying through Congress. As a result, while the amount of applied science done in universities is increasing (mainly due to recent federal defense budget increases), much of it is totally fragmented and uncoordinated.

But the profession shrugs off this chronic instability in applied science with the old cant. What does it matter, so long as money keeps flowing for basic research? Isn't basic research "scientific capital," the fount from which technology flows, the wellspring for innovation? All too often, the process by which science becomes technology does not concern today's scientists. And the fact that the incubation period between science and innovation has been set by scholars at 30 years (conveniently longer than the remainder of their careers) relieves them of responsibility, they think, to see that applications come forth. For all they care, the process of innovation might as well be magic. This insouciance is reflected in federal science policy, whose arrangements for civilian applied science and development are woefully inadequate. This basic-research-is-best mentality robs the applied sector of the best brains, care for integrity of the enterprise, and policy attention. Moreover, the administration of basic research is characterized by a weak management style that permeates the applied side as well and permits applied projects to be politicized, turned off, turned on, turned to the left, turned to the right, broken up, and to yield, in the end, only scraps.

This chapter will also discuss the peculiar science policies of the Reagan administration. In many ways the President's science adviser, George A. Keyworth II, epitomizes the basic-research-is-best mindset that we find so insidious. And Keyworth has backed boosterish words with deeds, providing some of the most generous funds for basic science of any science adviser in the past decade—while in effect announcing that more money for basic research will fix the U.S. economy. Meanwhile, he has hacked away at applied projects, demonstration programs, and the federal civilian applied science infrastructure that we argue is invaluable to the orderly transformation of pure knowledge into economic value. Keyworth's maverick views on defense, such as his support for futuristic "Star Wars" antiballistic missile systems, rightly fill many in the science community with doubt. But on the plus side he has also been more willing to experiment with the institutional structure, a gameness we feel is long overdue. However, the story starts back in 1946.

I

Bush's report to President Roosevelt, *Science—The Endless Frontier*, argued for a single independent foundation run by a board of nine qualified citizens. The foundation would sponsor research in the physical sciences, in medicine, and in military matters; it would analyze national policy on research and science education. A separate science advisory board was proposed to evaluate and integrate all technical research sponsored by other government departments. It was a sweeping order which was never carried out.

Neither President Truman nor some in Congress liked Bush's proposal to give a group of nine citizens control over funds outside of normal government channels. They also objected to Bush's plan to have rights to inventions derived from government-sponsored research rest with the individual inventor. Truman vetoed the first version of the bill creating the new foundation.

In the end, the final mandate of the foundation in law differed from Bush's original plan. Besides a powerful board to run the foundation and make policy, the law provided for a presidentially appointed director with whom the board would share authority. Bush had proposed a division of national defense and a division of medical research, but by the time the bill passed, neither was included. The scope of the foundation was also narrowed. Bush had wanted it to "develop and promote a national policy for scientific research and scientific education," but the act limited this authority to basic research and science education.[1]

Nonetheless, the Director and the National Science Board were given a national policy role. They were to "appraise the impact of research upon industrial development and upon the general welfare," and "evaluate scientific projects undertaken by agencies of the Federal Government."

Although the mandate of the National Science Foundation (NSF) retained some of the broad, ambitious themes Bush wanted, the early leaders of NSF and early members of the National Science Board would consciously decide to play a more limited role. They would decline to have the Navy's research program moved under their jurisdiction; they sidestepped opportunities to review other federal programs. When conflicts appeared with more powerful agencies, they demurred, saying that the foundation was only in the business of supporting basic research.

The early debates about the foundation's role are still relevant today. Was it to support only elite institutions? Or should it, as the politically potent land-grant colleges urged, distribute its money geographically to develop top-quality institutions where there had been none? As other research agencies sprang up in other government departments, Bush argued that the need for the Foundation's coordinating role was stronger than ever.

> The recipients of these grants are quite happy, they have several sources of funds and can trade about, but they are likely to become adept at that sort of political maneuvering at the sacrifice of more virile characteristics.[2]

Which is exactly what happened. In time, the NSF avoided involvement in national policy and chose to fill in the cracks left by other agencies; it claimed a unique role only in supporting undirected basic research. Thus opened the first rift in what became a gulf between basic research and the rest of the enterprise.

The chances for central management of postwar science diminished for other reasons as well. In 1945–1946, the atomic scientists, who were now world-famous thanks to the bomb, were fighting ardently for civilian control of atomic energy. A crucial argument in their fight to make the Atomic Energy Commission (AEC) a civilian agency was that atomic research could produce not only bombs but also benefits for mankind, in agriculture, medicine, and industry. Civilian applications could emerge, they argued, only if this research was done in a civilian agency and if there were few barriers between civilian and military atomic research. They succeeded with the passage of the Atomic Energy Act in 1946; the AEC then became the major civilian sponsor of physics research.[3]

Other forceful personalities had plans too. Admiral Harold G. Bowen, a Rickover-type figure, was determined to set up a glamorous research arm in the Navy. Bowen was a forceful officer smarting, first, from his banishment to be head of the Naval Research Laboratory by the regular Navy bureaus, and second, from the way the Office of Scientific Research and Development (OSRD) had mistreated the Navy, in his view, during the war. Bowen and his team of officers toured the campuses of the elite universities and offered them generous terms to come under Navy patronage. They had $24 million in contracts by the time the law establishing the Office of Naval Research (ONR) was signed in August 1946. The sum was handsome: three out of every four dollars that the federal government spent on research were being spent by the ONR. Some in the scientific community and the Bureau of the Budget were concerned about this overwhelmingly military support of university science; later a transfer of much of ONR's program to the NSF was arranged – but never carried out.[4]

The ONR thus became a generous patron of university science and was well liked by those who received its largesse. The careers of many of today's scientific leaders were shaped by the perceptiveness and informality of the ONR. Scientists were poor then, and a few thousand dollars a year from an understanding ONR officer sometimes meant the opening up of a new research area or career path. Many of those we interviewed talked nostalgically about the old ONR as having been the only federal patron that had really understood how to mix the Navy's mission needs with a sharp eye for advancing high-quality basic science.

It was no surprise, then, that when the NSF was formed, the obvious candidate for director was ONR's chief scientist, Alan T. Waterman. It also followed that, when Congress nearly zeroed the fledgling NSF's first budget request, Waterman should have turned to his friends in the military and asked them to support NSF's cause in Congress. In return, Waterman promised he would not poach on the military's research turf.

The same thing happened with medical research. Medicine had been supported on an emergency basis by the OSRD during the war. When OSRD's medical committee dissolved, it turned over forty-four research contracts to the National Institute of Health (NIH), founded in 1930. As the NIH grew in the late 1940s, institutes were added for mental health, dental science, neurological diseases, and microbiology. By 1951 the NSF faced a powerful rival in the NIH in sponsoring medical research. Waterman decided to confine the NSF's role, therefore, to the most basic aspects of biomedicine.[5]

Thus, by the time the NSF opened its doors in 1951, university scientists had fragmented into four overlapping constituencies: the medical researchers who looked mainly to the NIH, a distinguished group at the elite research universities sponsored by the ONR, the physicists sponsored by the AEC, and the group forming around Waterman at the NSF. In addition, the Army and the Air Force were building up their sponsorship of research. There were the older groups, such as the National Advisory Committee for Aeronautics, the U.S. Weather Bureau, and the U.S. Geological Survey, as well.

II

In our view, in the early 1950s when Waterman, the National Science Board, and the Bureau of the Budget made critical choices about the NSF and the growing federal–university science system, the seeds of our present troubles were sown. The tone and values of the system were set on a course that would lead them away from the main ideas in *Science—The Endless Frontier.*

The change was unwitting, and it seemed logical at the time. For example, much of Waterman's and the board's reluctance to play a more ambitious role was due to the problem they were having in justifying the agency's tiny program before its Congressional oversight committees. If their modest initial effort was under so much attack, they reasoned, more ambitious schemes would make matters worse. In addition, their enthusiasm for basic science reflected the general euphoria about science of the 1950s. But from today's vantage point, historic opportunities seem to have been lost.

First, unmanaged diversity replaced Bush's concept of a national science policy and central planning for science. The notion that a committee of nine wise heads could run the postwar science system as the OSRD had managed the wartime one died with the delay in creating the NSF and the emergence of more powerful research patrons in other agencies. Though the NSF was charged with evaluating all federal science efforts, Waterman and the National Science Board let this idea drop in 1954 with hardly a murmur. So much for the institutionalization of a national science policy.

Diversity and lack of central control were not seen as a problem, however, and when the scale of the endeavor was small, it didn't matter. First, the scientists were pleasantly surprised at the government's generosity and were eager to be patronized. The diffusion of government sponsors was justified on the grounds that a single sponsor for physics or biology might be prejudiced and unfairly bias the development of a field. But this reasoning, invoked then and now as an argument against strong research managers, overlooked a key distinction in the Bush report. Bush had advocated the board of nine wise heads to be central planners for science, to do what today's business jargon calls strategic planning. Neither Bush nor anyone else advocated centralized *execution* of the plan. Science thus evolved in an increasingly convenient, diverse—and headless—fashion. As Daniel S. Greenberg wrote of the postwar period, science was unexpectedly rich, but "it was also rudderless in its affluence."[6]

Second, growth became the system's dominant mode of successful operation and perhaps its only one. The system institutionalized Bush's recommendation that science requires a certain freedom and that scientists are best qualified to run their own affairs. And since every experiment would breed four new questions requiring answers, four new projects could be justified to follow on after every one. Thus, run from below, the system had no internal means of judging among the four follow-on projects, i.e., of setting priorities. This flaw remained hidden in the days of affluence. But because of it, the community to this day nearly tears itself apart when funds are tight. (Keyworth, in a promising recent move, has insisted that a few subfields such as planetary science set their own priorities for limited funds.)

Thus, the institutional management pattern tended to be passive, scientist-oriented, and discipline-oriented. These were values emphasized by Waterman during his long tenure as director from 1951 until 1963, as described by NSF historian J. Merton England:

> Convinced that science policy must come from the scientific community, not from Washington, he was really a spokesman for basic science and its protector in Washington, not an instrument serving the short-term goals of a political administration.

And as for the orientation of the NSF program managers, who made grants in biology, chemistry, physics, and so on:

> [T]hey were even more ardent advocates of support for basic science research in their disciplines. Science policy to a program director meant most of all more money for his field of science. Even if he were a permanent member of the staff rather than a rotator on a year's leave from a university, a research program director maintained close association with working scientists throughout the country and regarded them as his colleagues. . . .
>
> The professional staff emphasized high-quality science, that which the peer review system judged most worthy of support.[7]

This philosophy is a far cry from the Bush report, which emphasizes central planning by the board and a scientific community responding to national needs.

The NSF philosophy became the opposite: policy was deliberately left in the hands of the university scientists, mostly through peer review. Although in the early days there was so little money that only the elite institutions passed normal reviews, as the system became richer, it spread the money around and began trying to develop research excellence in institutions previously unable to compete. As one of the managers of the NSF earth sciences program in the 1950s recalled, the reviews immediately showed which proposals were best and which were worst, leaving a large middle group. If the middle group had proposals of equal merit from M.I.T. and from, say, Jonesboro Teachers College, "We gave it to Jonesboro College," he said.[8] The system thus encouraged more and more Jonesboros to aspire to play the basic research game and emulate the high science done at the top-ranked schools.

The growth in federal research funds going to colleges and universities was astonishing. The overall shifts it caused are shown in Figure 5. In 1960 universities and colleges performed an estimated $433 million in basic research coming from all sources. By

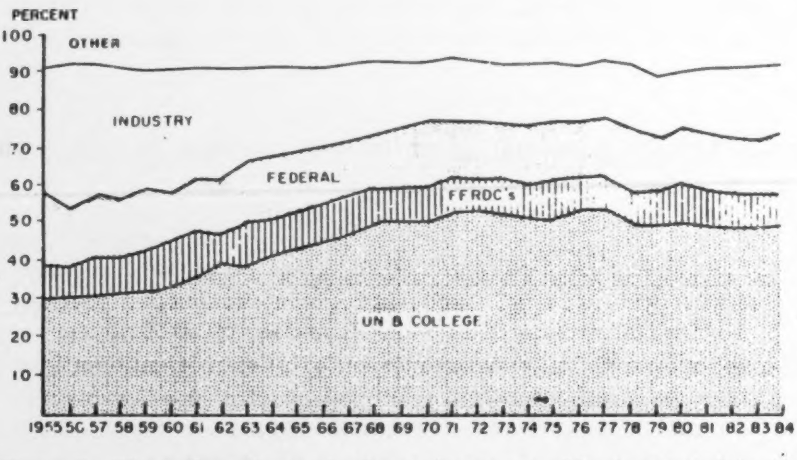


Figure 5 The salient change in the performance of basic research in the United States in the postwar period is the gradual withdrawal of U.S. industry, which performed approximately one-third in the 1950s and early 1960s. Colleges and universities, which performed 30 percent in 1955 (almost as much as industry), performed 50 percent by 1968 and 49 percent in 1984. Chapters 2 and 3 discuss the profound implications of this shift and of corresponding shifts in the performance of applied research for the national technological effort. (SOURCE: Chart adapted from Bruce L. R. Smith and Joseph J. Karlesky, *The State of Academic Science* (New York, Change Magazine Press, 1971), Vol. 1, p. 22, Fig. 4. Recent data from National Patterns of Science and Technology Resources, 1984, National Science Foundation, 1984, Table 8, p. 33.)

1982 that figure had grown in current dollars to $4.9 billion. In constant 1972 dollars the increase was from $622 million to $2.3 billion. When the federal applied research and development budget is added in, the figures are still higher. And development monies were easier to spread geographically. So a system that started as a way to continue modestly supporting fifteen to twenty established institutions became an odd hybrid, part political pork barrel and part peer-judged quality (and discipline-constrained) science. The case for nonstop growth was argued in a series of National Academy of Sciences (NAS) reports on specific disciplines, such as the Pake report on physics. These argued that current funding levels were not enough and that only lack of funds stood between the field's current accomplishments and future breakthroughs.

There was explosive growth in medical research. Federal support enabled the number of teaching hospitals, departments, faculty, and level of research funds to grow. Between 1950 and 1980, the number of medical schools grew from 72 to 112, or 55 percent. The number of M.D. degrees awarded rose in the same period, from 5,600 to 14,900.

This amazing growth reflected many profound shifts in the structure and funding of American medicine. Early in the century, the U.S. medical profession had recognized that the M.D. degree alone was insufficient to keep doctors abreast of new advances; it encouraged M.D.s to stay on for a residency before starting practice. Medical schools were assuming more research duties even before World War II. Then the Bush report and the medical science lobby spurred postwar interest in research-intensive, scientific medicine that gave the schools a powerful adjunct in the NIH. (By 1948 it had so many units added that it became the National Institutes of Health.)

Then came dramatic changes in health care financing in the 1960s: federal reimbursement for patient care under social security (not to mention veteran's care under the Veterans Administration system). These enabled doctor–researchers to offer very sophisticated care that was an adjunct to their research to virtually the entire patient population, at no extra cost to patients themselves. To borrow an analogy from the defense industry, suddenly there was a guaranteed customer for almost any technological advance in medicine. The doctors acquired, so to speak, captive buyers, a guaranteed market "pull."

The postwar growth of federal research patronage spurred different kinds of developments in medical research, some healthy, some not. For one thing, the discrepancy was particularly awkward between the justification for federal support and the researchers' professional desire to go off on their own and do basic research.

The "application" of biomedical research was politically very attractive, namely, that a cure would be found for some disease or that a particular organ of the body, researched massively by the federal government, would stop suffering, so to speak, all across the land. As members of Congress recognized the political appeal of starting new named, targeted NIH programs, their rapid growth became inevitable.

Of course, the medical scientists who benefited from this popularity could not promise that the chances of a cure would improve in direct proportion to the amounts of money voted by happy Congressmen. But they colluded with the politicians because it is true that basic research really does (synergistically, we might add) produce breakthroughs. Such basic research funds, they could argue, would be churlish to refuse. The outstanding case, of course, was President Nixon's "war on cancer" in which the political momentum for a large new program overwhelmed the scientists' doubts that a transfusion of government funds would cause a "breakthrough" anytime soon. All they could conclude was that inducing thousands more researchers into the field through a new program would not hurt, and might help, not only cancer but also other fields. So the scientists took the money and did their research. To the scientists' credit, they tried to reorganize Nixon's plan to ensure that it had more scientific integrity. But still today, the management of the program reflects its somewhat schizophrenic character, with the highest-level advisory board composed of luminaries, such as industrialist Armand Hammer, who reflect the political constituency for a "cure" and a subordinate scientific board that runs the operational aspect of the program.

Especially when it was smaller, the NIH exemplified what we argue are ideal conditions for extracting both applications and good basic science from a single organization: there was both pluralism and synergism, pressure from the top to find applications and enough high-quality science to attract the best people. Moreover, various of the NIH's directors have been strong managers, taking responsibility for the technical integrity of the whole and even, on occasion, saying no to politicians or powerful medical lobby groups. In this sense, the NIH system proves our thesis, as parts of it have been highly productive. But many in the biomedical community criticize a great deal of the NIH's work as pure basic-research-is-best for its own sake, and too narrow and self-centered and divorced from other research and from applications.

Perhaps the most instructive aspect of the biomedical research system from our point of view is the coupling of practical necessity and basic science embodied in the M.D.-Ph.D. system. After the war, more and more M.D.s were encouraged to get the Ph.D. as well. Thus, even while pursuing abstruse research careers, they would not forget their bedside experiences and responsibilities. The M.D.-Ph.D. corner of the overall biomedical research system is credited by many with having made biomedical science so productive of applications. It is one reason for concern about the possible decline in M.D.s who go on for the Ph.D. discussed in Chapter 4.

The political appeal of dollars for defense research is similar to that of dollars for biomedicine. In defense, there are eager politicians voting funds for research and a guaranteed customer in the armed services and intelligence agencies. But as with biomedicine, the result has been a mixed bag. On the one hand, those parts of the defense science community that are synergistic have made tremendous contributions linking basic knowledge to applications (we will see this synergism between defense science and industry in the electronics story in the next chapter). On the other hand, the defense establishment has sometimes been as weak on applications as parts of the NIH. Defense research agencies generally do a good job of moving ideas from the pure knowledge stage to what we would call purposive basic research, and along the spectrum to new hardware and a customer in the armed services. Moreover, it uses a management tool, namely, the classification of all projects under headings of 6.1, 6.2, 6.3, etc., depending on where they fall in the basic-to-applied spectrum, which helps to organize the transition to technology. Finally, under the influence of the university scientists it sponsors, the Department of Defense (DOD) has constructed critical mass centers of high science around the country (the AEC, similarly influenced by its university-based physicists, did likewise).

But DOD is criticized by many thoughtful people for having weak management of the transition to applications of "almost technologies," such as advanced rocket fuels or ceramic engines. This tends to happen with developments which there is as yet no specific customer lobbying for, yet where the underlying promise is clear. As on the civilian side, defense projects at this level often get started, stopped, and redirected arbitrarily, and the linkage with industry (with some stunning exceptions) is uncoordinated. So from World War II through 1970, defense research suffered from the same affliction: strong basic research and sometimes weak management of applications. Following congressional pressure, DOD cut off and transferred much of its basic research in the 1970s—and some people argue that a proper balance between the two has not been achieved in the DOD, even under the Reagan administration, which has poured funds into DOD *development* accounts but has been less generous with DOD *basic* research, as we shall see.

III

The template of this federal institutional pattern was printed on the universities, where the structure, funding, and values of research activity changed markedly after the Bush report and the sprouting of federal research agencies. Basic-research-is-best came to dominate the thinking of university people seeking funds, as well as research managers in government giving them out—even though, paradoxically, the amounts for applied research were sometimes greater and easier to spread around geographically.

Another impact of the basic-research-is-best attitude was to downgrade the role of science in industry, the roles of engineering and of invention. Generations of graduate students were taught that to work in industry had less status than to pursue basic academic research. Echoes of this are heard today as faculty members complain that today's graduate students are deserting academic science for industry. We will see in the next chapter how industrial research, which had enjoyed a coequal status with that done in universities, has been downgraded as the federal–university system has grown.

Even institutionally, engineering was given second-class status. While engineers in the United States have never had the low rank they have in Great Britain (a problem which shows up in Britain's difficulty in producing and marketing advanced technology), engineers in the United States nonetheless have less status than scientists. A symbol of this unfortunate relationship is the story of the National Academy of Engineering (NAE), which finally in 1982, as a result of a strong effort, became coequal with the NAS.

The NAS had been founded as a private organization with a federal charter in Abraham Lincoln's day in 1863. The principal scientists, such as Joseph Henry, represented our practical nineteenth-century tradition of applied science. But they also wanted to distinguish science from quackery and put it more on a footing with European high science. So the NAS evolved into an elite – which is what it was supposed to be, after all – but an elite which, by the mid-twentieth century, represented mainly the basic-research-is-best point of view.

This had consequences for the engineers, for they seemed permanently seated below the salt: they had no academy of their own nor did they, for the most part, qualify for membership in the NAS. But in 1964 they succeeded in having their own academy established.

However, this National Academy of Engineering was not founded as a separate organization with its own federal charter parallel to the NAS, but as a subordinate organization whose right to exist derived from the NAS charter. The NAE elected its own members, ran its own finances, and had its own program until the 1970s, when the program was merged with that of the NAS and they both became run through the NAS governing arm, the National Research Council (NRC). Management of NAS and NAE programs devolved on the governing board of the NRC. But representatives of the NAE were allowed fewer seats on the governing board than representatives of the NAS, so the NAS effectively controlled the NRC and its program. Finally in 1982, after a crisis in graduate engineering education and other issues had galvanized the engineers to lobby for recognition, the NAE won equal status on the board with the NAS. Nevertheless, engineering as a profession distinct from science had a long way to go in getting equal recognition from the general public.

Finally, the system discouraged any university from unifying disciplines. If a department of electrical engineering set out to compete with those at other universities in hiring the hottest graduate students and obtaining federal research grants, it was less likely to concern itself with the manufacturing firm just down the street. Once a department of oceanography set its sights on becoming a national "center of excellence" by Washington's standards, and by the standards of would-be "peers" in other centers around the country, it would become scornful of applied science relating to local fishing problems.

This narrowness is in contrast to the way many of the best U.S. universities traditionally functioned. Ironically, the institutions that maintained their industrial ties came through the 1970s the best—and are poised to remain strong in the future. Just before becoming President of M.I.T., Paul E. Gray gave a talk about the Institute's historic ties to industry. He noted that its 1861 charter says the Institute's purpose is to educate students in science *and its applications*; this set M.I.T. off from many other universities of the day. Elihu Thompson, whose electric company merged with Edison's to become General Electric, was both on the faculty and acting president of M.I.T. When William Lyman Underwood, whose grandfather had founded the country's oldest cannery, asked M.I.T. to study food spoilage, this led to the creation of M.I.T.'s Nutrition Department and to a revolution in the food industry. Even Vannevar Bush, who was M.I.T.'s Dean of Engineering in the 1930s and later the Chairman of the M.I.T. Corporation, tinkered with a little company in Medford that eventually became

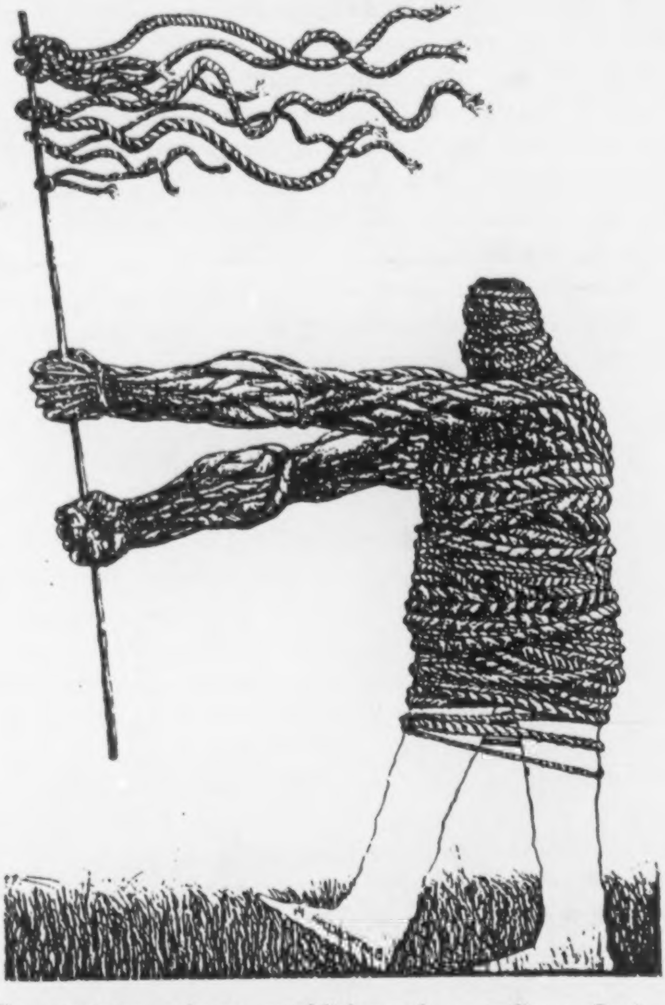
Raytheon. These links, Gray said, gave "rise to powerful intellectual and technological relationships that have influenced the shape of M.I.T. and of American industry in profound ways."[9] Gray's story illustrates our point: M.I.T. maintained a strong philosophy of the importance of *linking* basic and applied science, of tying the university setting to real-world needs. Both M.I.T.'s basic science and the problems it has addressed have benefited. Unfortunately, most U.S. universities lacked such strong, indigenous philosophies. So, over 40 years, as federal support grew, they came to believe in the basic-research-is-best message that the system transmitted on all wavelengths.

IV

This was the institutional setting for U.S. research in 1945–1970—which in retrospect was an aberrant period in U.S. history. At least through 1968, it was a period of unprecedented growth in the gross national product and of unprecedented U.S. hegemony in the world in military, economic, technological, and scientific terms. Characteristically, the academic research community lost no opportunity in implying that federal basic research funding was causing this prosperity. Now, none of these conditions holds, but the U.S. science system, its structure and values, have remained as unchanged as if we were still living in the 1960s.

Indeed, the system's problems were noted at the time. Looking back through the history, we found a fascinating series of justifications and warnings in a collection of essays about the value of research commissioned by the House Committee on Science and Astronautics in 1963. The Committee had asked, rightly, what level of federal funding, and balance among different sciences, was needed "to keep the United States . . . [in a] position of leadership *through* basic research *in the advancement of science and technology and their economic*, cultural and military applications" [italics added]. Most of the respondents conveniently reinterpreted the question to ask, what was needed for the U.S. to improve its basic research?

But a few authors, among them Hendrik Bode of Bell Labora-



The postwar science establishment's paradigm: Basic Research marches valiantly into the future. (SOURCE: Nalimov, p. 222.)

tories, Edward Teller, Arthur Kantrowitz, and Alvin Weinberg, warned that the transfer of basic knowledge to applications was not getting enough attention. Kantrowitz argued that since universities do not invent things, they train graduate students to believe that fundamental discovery, not invention, is the highest goal of science. "This attitude presents an intolerable obstacle to the achievement of excellent university education in applied science," he wrote.[10]

"Most of the superior Ph.D.'s coming from our universities have a primary interest in basic rather than applied science," warned Bode[11]:

> [U]ntil comparatively recent years, science and technology have pursued essentially independent courses. . . . The deliberate application of science to technology is primarily a phenomenon of the war and post-war years. . . .
>
> [T]he deliberate application of science to technology . . . presents a number of problems . . . [T]hese difficulties and unevennesses in the transition from science to technology represent the most critical aspect of the issues raised by the questions of the House committee – the aspect that most needs attention if the country is to maximize the yield from its investment in basic science. . . .

Bode then suggested ways to maximize this yield – through government actions, special curricula, and training for applied scientists who "should be on the same footing as the typical pure scientist," and a general realization of "how much science and technology actually have in common." But the warnings in the volume were drowned out by the politically powerful champions of the basic-research-is-best school, who would be mortified to be "on the same footing" with engineers and applied scientists.

Structural flaws began appearing in the 1970s, when the decline in real dollars going to university research exposed the system's weaknesses. These problems can be expected to remain, despite recent budget increases, if the reform is not undertaken. Somewhat arbitrarily, we divide these flaws into three categories, although they make up a larger, less easily defined problem.

Weak management. The present system emphasizes a model of science proceeding relentlessly, as the cliché goes, according to its own inexorable laws. This positivist-reductionist view of scientific progress is conveniently abstract, suggesting that no human intervention is needed at the managerial level. Peer review diffuses responsibility still more, since reviewers act anonymously and are not held responsible for their decisions. Indeed, they are shielded by anonymity and guarantees of impunity however erroneous—even outrageous—their reviews (we discuss peer review further in Chapter 4). As we have seen, this enables judgments about science to be made only at the level of the sub-subspecialties, provides no mechanism for judgment calls among subfields, or setting priorities among disciplines.

Barry Bozeman and Dong Kim of Syracuse University published an attitudes survey of NSF program officials. Unwittingly, it showed that Waterman's stamp persists. Analyzing survey questionnaires returned by 199 NSF officials, Bozeman and Kim found that the older and better-salaried they were, the less they were likely to believe that private industry was effective, or that research should serve social objectives, and the more likely they were to believe that basic research is an end in itself. Bozeman and Kim concluded:

> Despite massive government science activity, there is strong resistance among many scientists and science policy-makers to planning or directing science. Perhaps more than in other industrial nations, the United States government's policy-making agencies for science have worked in partnership with (or have been coopted, depending on one's view) by the nongovernmental scientific community.[12]

This passive view of management's role in science conflicts sharply with the descriptions we received of how major, top quality science institutions are run. At Bell Laboratories, we were told, basic research is not funded for its own sake. (Perhaps this is a myth, passed along by university scientists who hope that Bell Laboratories fits their favorite assumptions!) Although many researchers there work on fundamental problems, they are *selected* by managers who want their expertise. Federal science managers (with exceptions, as we said) tend to believe they should not play a comparable, driving role.

An unintended result of this management style is that scientists have abdicated authority to the green-eyeshaders at the Bureau of the Budget, now the Office of Management and Budget (OMB). By default, OMB became a potent manager of science through the mechanism of the annual federal budget cycles. If science were setting its own priorities, and making its own long-term plans, the importance of yearly budgets would diminish, as would the scientists' obsession with marginal changes in them.

The OMB has had to force priorities upon U.S. science: sometimes it has forced specialties to pay more attention to national needs, and sometimes it has gutted fields that in its judgment have not been productive. OMB does this only because the scientific community has failed to do these things itself. We argue that while there is some need for stronger coordination at the top, particularly of government civilian applied science programs, also there should be stronger self-management by scientists. The government might stop nitpicking if only scientists showed themselves better able to set their priorities responsibly themselves. Only in this way will the community be able to avoid the fate Don K. Price once described in the title of a paper he wrote in *Daedalus*: "The Endless Frontier Is a Bureaucratic Morass."

Archaic university departments. The absence of strong management within the science system leaves it vulnerable to powerful institutional pressures that are far less worthy. OMB micromanagement is one; another is the ordinary university department, which remains administratively sacrosanct and thoroughly discipline-oriented. Its *raison d'être* seems often to be to milk the federal government for all the money it can get.

The seeds of their influence were sown in the 1950s, when the federal science agencies organized internally into sections of biology, physics, and so on, intended to correspond to traditional departments. These agencies did this to better serve their budding university clientele, but the result has been to reinforce artificial subdivisions of science and put funding officials and university departments in lockstep.

This may be good for science that happens to fall in the mainstream of a traditional field, but it is hard on work that pushes at the fringes. NSF historian England's remark about how much NSF grants administrators identified with their counterparts in university faculties indicates the social underpinnings of the arrangement which became common in other federal science agencies.

Yet it should be obvious that the explosion of knowledge that has occurred as a result of all this research (it is said that 90 percent of the scientists who ever lived are alive today) often blurred or upset traditional disciplinary boundaries. Disciplinary boundaries are but lines drawn in sand. Rich universities (or merely ingenious ones) have been able to set up new departments or institutes (especially in the 1960s, when money was available), to enable merging fields to come together, or to nurture promising offshoots.

The number of such research institutes on U.S. campuses grew explosively from 2,987 in 1965 to 5,024 in 1972, but stabilized at the level in 1975 as the financial crunch set in. These institutes are typically interdisciplinary and relate to national needs—the environment, transportation, and so on. Recent novel examples include the Whitehead Institute at M.I.T. incorporating relevant parts of the life sciences, physical sciences, engineering, and medicine, and the small astrophysics group at the Fermi National Accelerator Laboratory near Chicago.

The departments' power did not matter so much in the 1950s and 1960s, when there was more money for add-ons as new disciplines sprang up. But in the tighter funding environment since, this rigid structure has become a liability. With a diminishing pool of graduate students on which to draw, promising offshoots with no power base are put in head-to-head competition with conventional, established work. An associate professor in biology who wishes to move into the new genetics, but whose department stresses classical biology, cannot go off on his own without taking money and students away from his colleagues. If he wants tenure, which is awarded by the department, he may hesitate before taking away his talent and the money and students which are the department's lifeblood.[13]

Moreover, agencies that divide themselves into neat disciplinary units make a national science policy impossible; for truly *national* science policy, almost by definition, crosses disciplinary lines.

Smith and Karlesky, in their still-relevant 1977 study of the impacts of level funding on university science, explain the stultifying impact of traditional university departments. For example, of the physics departments they visited, some of the "strong" departments (as opposed to ones rated "excellent") were having trouble attracting motivated graduate students. To keep the research going, one department converted itself to an institute-type operation and hired technicians to take the place of students. Some of the "good" and "adequate plus" physics departments were having difficulty justifying their large faculties relative to the small number of graduate students they were attracting. Many were having difficulty getting research grants—but of course trying to stay in the game. Several were going into short-term fusion and plasma physics research because money was available. (It is not obvious that, in a "good" or "adequate plus" physics department, this helps plasma physics, fusion research, or the parent university. All it does do is perpetuate the department and the faculty.)[14]

Indeed, a standard refrain that Smith and Karlesky heard from university scientists and department heads was how terrible it was that they were having to turn to applied research to keep going. The prevalence of this complaint, even at mid- and lower-ranked schools, shows how much the entire system, including the Jonesboro-type schools, has come to share the basic-research-is-best mindset, and how deep are the expectations of a perpetual handout to play the high-science game.[15] Another telling moment was when Smith and Karlesky were told by departments that had received NSF Science Development Grants to improve the "excellence" of their research (i.e., to aspire to the high science game) how hard they were being let down now that their Science Development Grants had ended and they had to compete openly for funding.

Smith and Karlesky's profile of the system in the late 1970s concluded that the cutthroat competition for funds was causing scientists to avoid taking risks; they were not giving creative chances to the younger scientists, thus compromising their future. What Smith and Karlesky described was not only a phenomenon of the 1970s, for the rising costs of scientific instruments and a likely decline in student population will create similar conditions for the rest of the century, even if budgets continue their present modest rise.

If we could design the system from scratch, we would not allow university departments in the traditional disciplines to gobble up new research topics and reshape them on their own Procrustean beds. Instead, we would use the enormous leverage of federal funds to encourage reform in universities to emphasize fluidity, creative, risk-taking work, and teamwork among specialists in different disciplines. We would try to enable university researchers to move back and forth between basic and applied problems.

In a reconfigured system applied work would not be done in a fragmentary way to keep the faculty employed until they can resume the high-science game. Instead, the majority of mid- and lower-ranked colleges and universities would reconfigure themselves to do *good* long-term applied science, concentrating on fields needed by local industry.

Confusion about freedom and accountability. The third structural issue we will take up in detail is the scientists' confusion about freedom and accountability. *Science—The Endless Frontier* argued that science could be entrusted with taxpayers' funds because it would (a) be self-governing and (b) give back results in terms of inventions and new industries, as it had during the war. This philosophy has worked best from OMB's viewpoint in mission-oriented engineering projects such as the Apollo program and some of the better DOD projects such as the Poseidon-Polaris program. There has been likewise a steady flow of improvements in the parts of medical research that have evolved in close alliance with clinical problems. Many of these efforts have been, for practical purposes, self-governing—often because they were led by project "champions" who fended off bureaucratic and political intrusions. The old Bush contract between science, government, and society held.

But some research universities define freedom very differently. "Freedom" means freedom to pursue one's research whims, so long as they pass muster with those doing like-minded research. Freedom also means freedom from other standards of judgment, such as whether work in a subspecialty contributes to anything outside, or whether the work has useful applications in the real world or to national needs. Such judgments would be considered by many scientists today as an infringement on their "freedom."

In short, most prefer to "escape to the endless frontier" (a quip which happens to be the title of another Don Price paper). But the word accountability means something entirely different to the local federal accountants who reside on every major campus. To them accountability means: hours spent on a project, keeping strictly to budget categories, accounting for broken glassware. Accountability does not mean whether the goal of the grant has been achieved.

Our critique, then, is that these long-standing structural weaknesses have fragmented university research into ever-more irrelevant parts in the name of basic-research-is-best. The system's defenders argue that university research should be isolated from tests of relevance to other fields, industry's needs, and society. While we agree that universities should take a more reflective view, this is no excuse for doing second-rate science because it follows the path of least resistance provided by the system.

A key development that may change the system is the new partnerships between universities and industry which have sprung up in recent years. Much hailed as a new source of income to universities (because the sums are sometimes large by ordinary grant standards) and publicized for the legal and ethical issues they raise (which are often satisfactorily solved), these partnerships have become a new vogue. To us they are welcome, and our reform of the system would have many more such partnerships and even allow industry in a region to help set university priorities.

But this reform is a long way off. Despite the hoopla, even our top universities receive only 3 to 4 percent of their income from industry, according to a 1982 National Science Board report. Only universities with long traditions of industry service are doing better: Carnegie-Mellon ranked first with 17.1 percent; next came the University of Rochester at 11.9 percent; Penn State, Georgia Tech, and the University of Southern California had 10 to 11 percent. But Harvard had only 4 percent; Princeton, 1.5 percent; and the University of Chicago, 0.7 percent. By 1983 industry contributed only 5 percent of all university research support.[16]

V

Many scientists may dismiss our critique on the grounds that the Reagan administration has been a booster for science. After a shaky start in fiscal 1981, the administration has continued increases in the basic research funding of the Carter administration, as Figure 6 shows. It can also be argued that the administration's economic policies, which have been to encourage research and development (R&D) investment, fuel an economic recovery (albeit through historically high deficits), and restore business confidence, have done more for the national R&D picture than any tinkering with the federal science budget could. Finally, Keyworth, the President's Science Adviser (though one of the few scientists *not* from the university world to hold the job), has turned out to be a public booster of science and technology—boosterism, to be sure, of an old-fashioned kind that would seem more appropriate for his boss, but boosterism nonetheless.[17]

For U.S. scientists to accept that U.S. research is healthy because this trend line is working in favor of university basic research is a narrow and probably irresponsible view. Who can be sanguine when the numbers of U.S. citizens opting to get the Ph.D. in the hard sciences is dwindling? The thousands of foreign graduate students now swelling our schools are welcome—especially as they compensate for level overall college enrollments—but this is not quite the same. And what does industry offer that university science does not to attract science graduates in such droves? The answer may be job stability, better pay, and perhaps the hope of a more rational career ladder ahead. These demographics, discussed further in Chapter 4, make up the human factor that should trouble U.S. scientists today. And they are structural problems that more funds for basic research cannot, in the end, cure.

Moreover, the Reagan administration has chillingly favored clamping down on academic freedom in the name of national

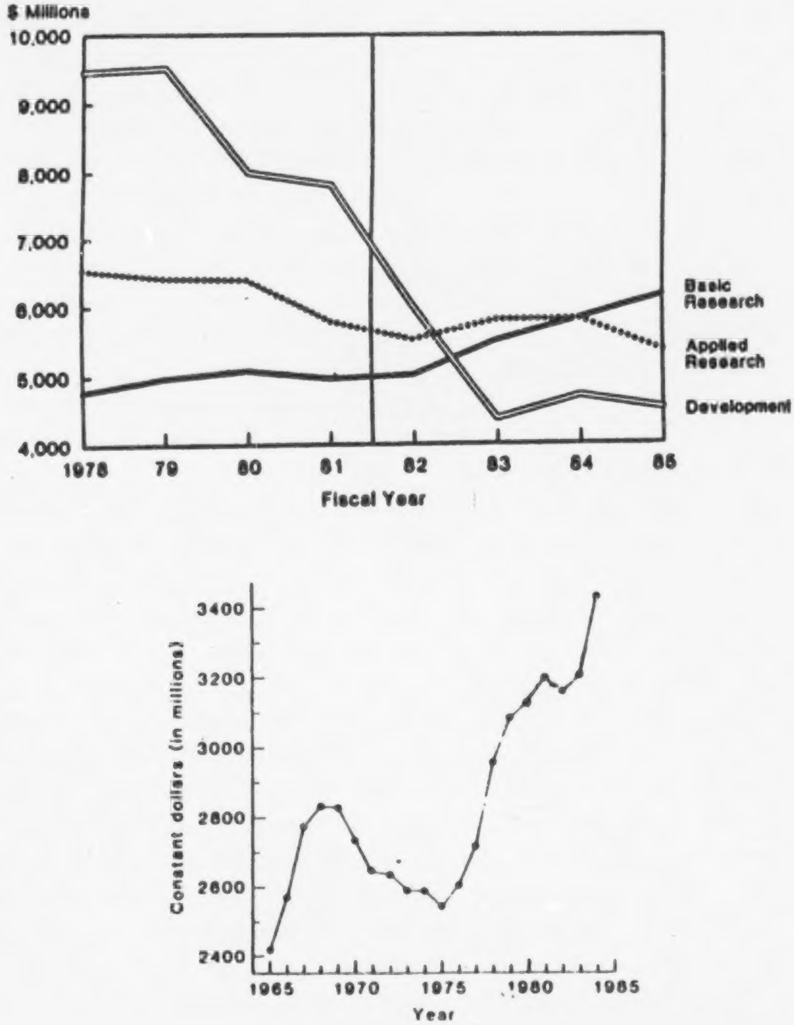


Figure 6 The science policy of the Reagan administration epitomizes many postwar trends in federal research and development funding. (*top*) The administration's insouciance for the fate of development projects and applied science is shown, in terms of federal research and development obligations in civilian areas, 1978–1985. Data in constant 1983 dollars. (*bottom*) Federal funding for basic research 1965–1985 in constant 1983 dollars. Recent increases are evident. (SOURCE: Office of Science and Technology Policy.)

security—even to the point of trying to stop a number of unclassified papers from being given at a large international meeting. While it has shown signs of backing off from a plan to restrict unclassified research that was thought to fall in "gray areas" of possible military significance, the administration's attitude may nonetheless be having a "chilling" effect on university research.

Thus, Keyworth's tenure has been a mixed bag. On the one hand, there is boosterism for basic research; on the other, a blithe willingness to axe federal applied science development and demonstration programs, including many of obvious, long-term, bipartisan importance, such as the Department of Energy's conservation research or the National Bureau of Standards' fire and building studies. As he wields the scalpel, Keyworth declares that if there is worthy development work to be done, private industry should do it (defense projects should be the exception, he adds).

With reference to our theme in this essay, Keyworth represents a step backwards, to the old basic-research-is-best attitude which assumes that American research must be all right if basic research has funds. In his cutting of federal applied science, he is ignoring the need for linkage with the federal science system.

On the other hand, his willingness to experiment with the institutions of the postwar science establishment is long overdue. At first Keyworth sounded as though he would close down or spin off the national laboratories—the large federal centers that sprang up in Bush's day and live on, like woolly mammoths foraging for food as the glaciers advance. He has encouraged the new trend toward industry-university partnerships—which, despite much publicity, are only a tiny fraction of the university research picture. He has responded to calls from Congress to do more in engineering by establishing engineering centers run by the NSF (meanwhile fighting off attempts by Congress to give the NSF a major science educational role). There is a new panel headed by David Packard on the structure and funding of federal science. These experiments we applaud, for they offer a faint glimmer of hope that old institutions and values, entrenched for 30 years, may change after all.

NOTES

[1] J. Merton England, *A Patron for Pure Science: The National Foundation's Formative Years, 1945–57* (Washington, D.C.: National Science Foundation) 82-14368, p. 109.

[2] *Ibid.*, p. 100.

[3] Alice Kimball Smith, *A Peril and A Hope* (Chicago: University of Chicago Press, 1965), pp. 273, 274, 267, and Chaps. 13 and 14.

[4] Daniel J. Kevles, *The Physicists, The History of a Scientific Community in Modern America* (New York: Alfred A. Knopf, 1978), pp. 354–356, 359. Author interview with Willis H. Shapley, formerly of the Bureau of the Budget.

[5] Daniel S. Greenberg, *The Politics of Pure Science* (New American Library, reprint Plume Books, 1967), pp. 133, 134; England, *op. cit.*, pp. 141–155.

[6] Greenberg, *op. cit.*, pp. 133, 134 and p. 121.

[7] England, *op. cit.*, pp. 348, 349.

[8] *Ibid.*, p. 361.

[9] Paul E. Gray, "M.I.T. and Industry: A Partnership," in *The Decades Ahead, an M.I.T. Perspective*, Proceedings of a symposium for senior executives, 24–26 June 1980, Industrial Liaison Program of M.I.T., p. 130.

[10] A. Kantrowitz, "Leadership in Applied Physical Science," *Basic Research and National Goals* (Washington, D.C.: National Academy of Sciences, 1965), pp. 143–44.

[11] Hendrik W. Bode, "Reflections on the Relation Between Science and Technology," *Basic Research and National Goals* (Washington, D.C.: National Academy of Sciences, 1965), pp. 74, 75.

[12] Barry Bozeman and Dong Kim, "Governing the Republic of Science: an Analysis of National Science Foundation Officials' Attitudes about Managed Science," in *Polity*, Vol. XIV, No. 2, Winter 1981, pp. 183–204.

[13] Bruce L. R. Smith and Joseph J. Karlesky, *The State of Academic Science* (New York: Change Magazine Press, 1971), Vol. I, p. 142.

[14] *Ibid.*, pp. 102–117, esp. 113, 114.

[15] *Ibid.*, Chap. 4.

[16] *Science Indicators 1982* (Washington, D.C.: National Science Board, 1983), pp. 107, 127.

[17] See, for example, text of Keyworth remarks at R&D Policy Colloquium, AAAS, 29 March 1984, and remarks at National Academy of Sciences Dinner for Nobel Laureates, 8 March 1984.

B. PATTERNS OF EVALUATION AND DECISIONAL CRITERIA IN AMERICAN SCIENCE

REVIEW PROCESSES:

Assessing the Quality of Research Proposals

National Commission on Research (U.S.)
May 1980

Introduction

Following World War II, the Federal government began new, major programs to develop the basic and applied sciences and to encourage able young people to become scientists. Many programs were expanded in response to the challenge of the Soviet launching of space satellites in the 1950s. University science departments and medical, agricultural, and engineering colleges were the natural homes for much of the research the government wished to support—for research to be conducted by their faculties, for equipment, for laboratories, for faculty development, and for predoctoral and postdoctoral fellowships. Before World War II, basic science research in the United States had tended to be supported by universities themselves, private foundations, and modest government funds. Basic research was not conducted, as it was and is in many other countries, in government institutes and centers largely independent of universities. Most of our basic scientists were professors in universities; their instruction of advanced students was mainly in research. It was of the universities, which already had well established science faculties and facilities, then, that the government thought when it sought to expand existing support of basic research and training. More science could be conducted in the universities; young scientists could be prepared in them—both programs would answer the nation's need.

For three decades, the Federal government has continued major support for basic science in universities. Urgencies which first led to the increased support have passed, but new urgencies have occurred from time to time; there have been changes in emphasis because of national concerns and because of the promise of different fields in the sciences themselves. Support also has been continued because of the wide realization that it is in the national interest to have basic research conducted, and conducted largely in the universities: research leads to improvements in agriculture, in the diagnosis and treatment of disease, in technologies which increase productivity and lead to new products; it contributes to the prestige and security of the nation. The government's continued investment in university science has been very successful, and the prospects for university science still seem to be very strong.

The Federal government had developed its programs so that they were appropriate to the universities and the ways in which the universities conduct research. University researchers are professors; they teach as well as conduct research, and while they may instruct their graduate students as they conduct research, many of them also teach undergraduates and have responsibilities in the broad work of the whole university. Government support thus had to be arranged in programs which recognized the several responsibilities of university scientists—and which also recognized that the basic research of scientists in universities is planned and conducted by them *as individuals*.

There have been—and perhaps there will continue to be—strains in fitting the government's interests and procedures to the universities' concerns and ways of doing things. But one of the most successful instruments for directing support to individual university scientists has been to have ranking scientists assist government agencies in the review of research proposals and to recommend to federal agencies those proposals which are likely to result in the best science and, therefore, are the most worthy of support. The procedures for these recommendations are called "peer review." Their advantage and effectiveness are that in open competition, they lead to the support of the research which scientists themselves think is best—and they give to scientists the assurance that the system which supports their research is fair and discerning and directed toward concerns which they share as scientists.

Because the quality of peer review is of great importance to the government and to university scientists and because it has been criticized by some members of Congress and some members of the academic community, it has seemed to the National Commission on Research that peer review deserves special study. This report presents the Commission's views on the methods used to select research to be performed in universities and supported by the Federal government, with special emphasis on the peer review system. It addresses the difficulties of choosing and funding the best research proposals and nurturing a large and increasing community of scientists, while ensuring equity among investigators. Peer review is recognized by Congress, the agencies, and university scientists to be one source of scientific accountability for the public funds awarded to researchers. However, the widespread acceptance of peer review as a way of funding science fairly and effectively and as a means of providing scientific accountability has not gone unquestioned. The government, for example, is concerned with whether the taxpayers are getting value for their dollars and whether there is any evidence of favoritism in the reviews conducted by its funding agencies. These points also have been raised within the university community.

This report begins with a description of the systems of review used in agencies whose funds make up almost 90 percent of the federal research dollars expended in universities. It continues with a discussion of the criticisms of peer review that have been publicized and examined. Next, the report presents the conclusions reached by the

National Commission on Research. Finally, there are recommendations for improvements in the proposal review processes.

Since most of the work sponsored currently by the federal agencies at universities is in fields of science, this report often specifies "science" and "scientists" instead of "research" and "investigators."

Systems of Review

Some federal agencies use internal review exclusively; some use a combination of internal and external reviews; and some use an external review process exclusively. Program officers in the agencies award funds on the basis of the outcome of the review process: the quality and promise of the research and its priority among the proposals reviewed as acceptable. In making funding decisions, program officers and other agency officials include their own evaluation and assessment of the proposal's relevance to the agency mission, its balance, and other factors.

Internal Review

Agencies employ full-time, trained scientists on their staffs to administer their research program spending. In those instances in which these scientists are responsible for the technical review of research proposals, the agency is said to have internal proposal review. Criteria for excellence are generally the same as in agencies with external review. However, there is usually a stronger mission orientation in agencies which use internal review. Program officers, as the scientists usually are called, decide which research will receive funds by considering which proposals are the most promising and how well the proposed research fits the agency mission. Proposals selected for funding must be approved also by several levels of research administrators within most agencies.

Internal reviewers have the option of seeking an outside opinion on the quality of a particular proposal. The outside opinions usually are requested from scientists in other programs within the same agency, in other agencies, or in the university community.

External Review

In external review, research proposals are reviewed by scientists who are not employed in a government funding agency. While efforts are made to choose reviewers from different parts of the country and from special segments of the scientific population, the primary criteria are scientific competence and accomplishment. Ideally, an external review group represents the judgment of the finest scientists in the nation; their recommendations are to assure that the federal funds are used to support the best scientists and the most promising research.

The words "peer review" have a widely accepted special meaning: review by scientists who are actively engaged in research, who are not employed by a funding agency, and who have the research experience and achievement which will permit them to make discerning judgments on the scientific merits of proposals. In this context, *external review* is in essence *peer review*.

While the reviews which are conducted in some agencies by competent staff scientists can be considered to be a review by peers, the university community is sometimes wary of these reviews. Researchers, in the universities and elsewhere, prefer to think that their peers are those who are engaged in research currently--for they are aware of the rapid rate at which science moves and changes direction and of how quickly those who do not engage in research can be left behind and even fail to be informed about the promise in new developments.

External review may operate in either of the following ways, or in a combination of both:

1. Peer reviewers, selected by agency staff members, are sent a proposal for evaluation. The accompanying letter and form provided for the reviewer describe what is expected from the external reviewer. Table I (p. 6) refers to this method as "mail review."

2. Peer reviewers are selected to serve for a fixed term and meet as a group several times a year with agency staff members to evaluate proposals that have been circulated well in advance of the meeting. In special cases dealing with a complex or interdisciplinary proposal, an *ad hoc* group may be convened for more limited tenure. Table I refers to this procedure as "panel review."

The evaluations which result from the external review systems are used in different ways by the agency program officers in deciding which proposals will be funded. While peer reviewers assess the scientific quality and research feasibility of proposals, they also advise agencies on the appropriateness of the budget request to the research described, but funding decisions are the responsibility of the agency, not of the peer reviewers.

Choice of Review System

Different federal funding agencies use different review processes, or use the same basic review system but alter its procedures in some way. Each agency, of course, chooses a review system which it believes will lead it to support the best proposals, and each believes that it chooses the best research projects by the review system it employs. What, then, is the basis for the decision to use external (peer) review or another system in different agencies?

There seem to have been no formal criteria for the decisions to use peer review or another system. Internal and external reviews have grown out of the traditions of different agencies and have developed along with the administrative systems in the agencies. In some, where research interests are focused and continuing, internal review usually has been favored. In others, where there are a variety of research interests and a large number of applications, and in those which want their reviews to be closely comparable to the reviews which are conducted in the universities and the scientific community generally, peer, or external, review is the process of choice. Peer review has been legislatively mandated recently for a small, competitive grant program in the Department of Agriculture, and a dual system has been mandated for the National Institutes of Health and the Alcohol, Drug Abuse, and Mental Health Administration.

Review Processes Used by Federal Agencies

National Institutes of Health

The National Institutes of Health (NIH) and the Alcohol, Drug Abuse, and Mental Health Administration (ADAMHA) use a dual system of peer review. First, proposals are evaluated for scientific quality by panels of experts in the specific field, known as Study Sections or Initial Review Groups. There are currently 54 such panels which meet three times each year at NIH and review approximately 25,000 proposals annually. The Study Sections are managed by a division of NIH which is separate from the program divisions. After open discussion, the members of the Study Section (12-20 scientists) vote individually by means of a numerical rating if a proposal is approved, or by indicating disapproval. The Executive Secretary (an NIH staff member) then determines the overall priority rating from the individual numerical ratings and submits this together with a summary statement to the second level of peer review, the National Advisory Council.

Each Institute of the NIH has its own Advisory Council, which is made up of ten or more scientists and physicians and which includes nonscientist, public representatives as members. Each Council considers the priority scores of the proposals reviewed by the Study Sections with respect to need and relevance to the categorical mission of each Institute. The Advisory Councils must vote for approval (or disapproval) of each of the proposals under consideration at regular meetings sometime after receiving the recommendations of the Study Sections. No funds can be awarded without the approval of an Advisory Council with the exception of the following: (1) current NIH fellowship program; and (2) requests of under $35,000 in the National Cancer Institute and the National Heart, Lung, and Blood Institute. In most cases, the members of the Advisory Councils rely on summary sheets prepared by the Executive Secretary of each Study Section for information regarding each proposal. In addition, the Executive Secretaries of the various Study Sections meet with the Councils to supply other relevant information at the request of the Council members.

National Science Foundation

The National Science Foundation (NSF) reviews about 26,000 research proposals annually for a program budget that is substantially less than that of the NIH. The National Science Foundation uses external reviewers in the following ways: (1) mail review, (2) panel review (comparable to a Study Section in NIH), or (3) a combination of both. Some program managers rely almost exclusively on mail reviews and others almost exclusively on panels that meet and consider all proposals in open discussion. Reviewers are experts in appropriate disciplines chosen by the program directors to advise NSF on the technical and scientific worth of proposals. Funding decisions are made by responsible NSF staff using the evaluations provided by the reviewers.

In 1979, the average number of NSF reviews per proposal was 8.5 overall, ranging from a low average of 5.2 in the Mathematics, Physical Sciences and Engineering Division to a high of 12.7 reviews per proposal in the Biological and Behavioral Sciences Division. The former division did 79 percent of its reviews by mail, and the latter division used the mail and panel combination for 94 percent of its reviews.

Department of Defense

The Defense Advanced Research Projects Agency (DARPA) supports research and development programs that are high-risk and at the forefront of technology. It can be characterized as having minimal administrative constraints on proposal review. Three other branches of the Department of Defense (DOD) also support basic research in universities. Scientific officers in the Air Force Office of Scientific Research (AFOSR) and the Office of Naval Research (ONR) are responsible for the technical review of proposals. The Army Research Office uses external review exclusively. A few programs within AFOSR (chemistry and mathematics) and ONR (biological sciences) use an external review system regularly, but scientific officers in their other programs may choose external reviewers to assist in the evaluation of certain proposals at their discretion. When employed at DOD, external review is a single system of peer review involving either mail review or panel review.

Management of most external review groups at DOD is accomplished in conjunction with other individuals or organizations. The Chemistry Division of AFOSR contracts with a coordinator each year; the Mathematics Division of AFOSR is affiliated with the National Mathematics Society for purposes of proposal review; and the Army Research Office is advised by a committee of the National Research Council. Once the technical reviews have been submitted, there are several internal layers of priority rating and approval within each of the DOD branches that must be negotiated before a proposal receives funding.

Department of Energy

The Department of Energy (DOE) has review procedures which differ according to whether the research is to be done in their national laboratories or elsewhere. The Office of Basic Energy Sciences and High Energy and Nuclear Physics and the Office of Health and Environmental Research initially screen proposals internally and then have them reviewed externally for technical quality. If a proposal comes from a principal investigator at a university, an attempt is made to assign reviewers from the academic community. A peer review panel system similar to that used in NSF will be used in the new DOE Program for Biological Energy Research for evaluation of proposals.

Department of Agriculture

Most research funds from the Department of Agriculture (USDA) have been allocated to state agricultural experiment stations on the basis of legislatively mandated formulas. Utilization of these funds for support of basic research has varied in the different land-grant universities. Recent legislation (Public Law 95-113) has established an additional, small, competitive grants program to encourage basic science research in universities and makes USDA the lead agency for food and agricultural sciences.

In its programs of direct funding of agricultural research to universities (other than the mandated formula grants), the USDA uses peer review procedures. With the inception of the recent mandate for peer review in one program (see above), the department is attempting to define and develop a system of external review that would apply across programs.

National Aeronautics and Space Administration

All unsolicited proposals from investigators at universities are processed through the Office of University Affairs at the National Aeronautics and Space Administration (NASA). Proposals are evaluated by NASA program offices in Washington, D.C., and/or field installations which the Office of University Affairs determines may have an interest in them. The Office of University Affairs is responsible for notification of acceptances or rejections and for negotiations between NASA's Procurement Office and the universities regarding the funding instruments.

Summary of Systems

Review systems of the foregoing agencies are summarized in Table I on the following page. As was noted earlier, these agencies provide almost 90 percent of the federal funds expended for research in universities.

TABLE I

Review Processes Used by Several Federal Agencies

Agency	Review System	Processes	Choice of Peer Review	FY 1979 Expended Research Funds to Universities
Department of Agriculture	External for basic research proposals. Formula funds to agricultural experiment stations sporadically reviewed internally and externally.	Mail review when external review used.	Usually used, except for new competitive grants for which use is legislatively mandated.	$ 206,000,000
Department of Defense				364,000,000
Air Force Office of Scientific Research	External only for certain programs (chemistry, mathematics). Mostly internal.	Mathematics: mail reviews; chemistry: panel.	Traditionally used at discretion of scientific officer.	
Army Research Office	External and internal followed by internal funding decision.	Mail review.	Committee of National Research Council selects reviewers.	
Office of Naval Research	Internal, except in Biological Sciences Division.	Mail review when external evaluation used.	At discretion of scientific officer on individual proposal basis. Traditionally external for Biological Sciences.	
Department of Energy	External in some programs; internal in others.	Mail and/or panel when external.	Program director's discretion.	292,000,000
National Aeronautics and Space Administration	Internal.	Mail if external review needed.	Reviewing office or field installation through Office of University Affairs.	144,000,000
National Institutes of Health	External, dual system.	Technical review by Study Sections meeting as panel; then panel review by National Advisory Councils.	Legislatively mandated.	1,770,000,000
National Science Foundation	External.	Mail, panel, or combination of mail and panel, depending on program.	Traditionally used.	602,000,000

Issues

Problems of Review Processes

The need for increased support for basic science has received increased attention in the past several years from Congress and particular emphasis by President Carter in his Science and Technology Message to Congress in March 1979. This increased interest in scientific research, combined with a needed assurance that the government is getting value for its research investment, has been accompanied by an increased concern for the quality of scientific review systems.

Congressional oversight committees have scrutinized the peer review system in NSF; the Office of Management and Budget has studied peer review in NIH; the General Accounting Office has examined peer review at NSF; and several internal evaluations of peer review have been conducted at NIH and NSF. The reasons for focusing on these agencies are (1) that they provide most of the funds supporting research in universities; and (2) they both use peer review. Internal review systems in other agencies have not been scrutinized to the same degree, possibly because fewer dollars are involved or perhaps because the research programs of those agencies are a small fraction of their overall missions.

The fundamental issue in supporting research is to choose among proposals those that are deemed most likely to produce research of high quality and to advance basic knowledge leading to application. A practical question in selection is how to be assured that research yet to be conducted will be worthwhile. Here, agencies rely on the experience, knowledge, and insight of reviewers. Well chosen scientific peers are appointed because they know their fields, because they have participated in their development, because they have insight into those aspects where new growth is needed and can be expected to occur, and because they can make critical assessments of the ability of applicants to succeed in new research.

Normally, agencies will establish minimum requirements for the acceptance of research proposals, such as (1) a revealing and feasible research design; (2) assurance that the investigator has the competence, resourcefulness, and persistence to conduct, complete, and report the research; (3) evidence that the investigator has or can get the resources and facilities to conduct the research; and (4) evidence that the research will be of value.

Some items on such a list of minimum requirements are easier to establish than others (and easier to defend against criticism). In some cases, some of them are difficult: measures of risk are not always clear; some proposed research may be "break through or bust," fruitful or isolated. Agencies, especially in response to criticism, may be led to make their selections very conservatively. But if it were a checklist matter to choose among applicants for support, there would be no need to have any sort of scientific review. There are unpredictabilities in the development and application of science. These cannot be resolved finally, though in one area of science or another they can be reduced occasionally, especially as reviewers reflect on the ultimate assessment of quality and merit of research as determined by (1) the acceptance of the research findings by other scientists; (2) the impact and usefulness of the findings in furthering new research in the same or related areas; and (3) the utility of the research findings in application of the basic knowledge for development of practices or technology useful to society. However, these assessments can be made only retrospectively.

Enterprises which involve judgment need to be examined periodically and account taken of valid criticisms. An effective stimulus to improvement is criticism. It is therefore appropriate, considering their scale and importance, that review procedures—particularly peer review—be under the scrutiny of Congress, the funding agencies, the scientific community, and the public.

Criticisms of Peer Review

Critics of peer review have included some members of (1) Congress, (2) the federal agencies, (3) the university community, and (4) the public press. The Office of Management and Budget focused on *conflict of interest* in the peer review system at NIH in 1973. The issue of *age* of principal investigators whose proposals are funded has been raised. Congress has been concerned about the *geographical distribution* of federal research funds and the *secrecy* of the peer review system. Some university scientists have expressed concern about *cronyism* and *elitism*. Other criticisms have come from combinations of the above interest groups. Many studies have been conducted for Congress regarding the peer review systems in NIH and NSF specifically in response to these criticisms.

There are built-in safeguards against some of the problems cited in the preceding paragraph. Conflict of interest, for example, is always possible in a system where scientists are involved in the conduct of research, design of research programs, and evaluation of research proposals. However, it is minimized by regulations that reviewers shall not evaluate their own proposals or those from colleagues in their own departments or universities. NIH has a system of declaration which is designed to minimize any likelihood of conflict of interest on the part of members of its Study Sections and National Advisory Councils. NSF also has conflict of interest rules.

Chances for cronyism are lessened in peer review systems by the broad range of persons reviewing proposals and by the regular rotation of members from panels. Verbatim reviews sent to principal investigators on their request (or, in some agencies, sent with the approval or declination letter) minimizes the potential for favoritism, while at the same time, anonymity reinforces the potential for candor. Cronyism is minimized also by the provisions mentioned above, that reviewers shall not evaluate their own proposals or those from their departments or universities.

Inequities

Many of the criticisms of peer review are charges of potential or actual inequities. These include cronyism and conflict of interest addressed above, and age discrimination and lack of appeal procedure in case of an adverse decision. Age discrimination, bias, and cronyism are possible as problems, but there has been no documentation to show that there are, in fact, major problems. NSF has instituted a formal appeals procedure in which requests for reconsideration of declined proposals are presented to an Assistant Director (first-level appeal) and then, if necessary, to the Deputy Director (second-level appeal). In 1979, there were 54 first-level requests and 10 second-level requests from a potential pool of 12,476 declinations, though, of course, these figures do not reveal how many investigators resubmitted revised proposals rather than complain about their initial rejection. NIH is considering instituting an informal appeals procedure to supplement its present resubmission policy.

Inadequacies

The inadequacies of the peer review process which have been cited most often by critics are as follows: traditionalism and conservatism of peer reviewers; inability or unwillingness to recognize and recommend support for highly innovative, high-risk proposals; elitism in selection of peer reviewers; lack of elitism in selection of peer reviewers; inadequate review of proposals which are of an interdisciplinary character or which involve large, complex programs; inadequate consideration of past performance of an investigator in evaluation of scientific promise of a proposal; and inadequate protection against possible plagiarism.

With respect to the criticisms of elitism or lack of elitism in selection of peer reviewers, the most emphatic response has been that the essence of a successful peer review process is to have the most competent and talented scientists serve as peer reviewers. This sort of elitism is not only desirable, it is essential to fair and accountable reviews. In order to be more sure, however, that competent and talented scientists are not overlooked as potential reviewers, NIH, as described below, has initiated a widespread search for membership on Study Sections and Advisory Councils.

Commission members met with the senior staff at NIH and NSF about the related criticisms directed to the (1) traditionalism and conservatism or peer reviewers; (2) inability or unwillingness to recognize and recommend support for innovative, high-risk proposals; and (3) inadequate review of proposals which are of an interdisciplinary character or which involve large, complex problems. NIH and NSF claim that there is no validity to criticisms (2) and (3), though they recognize that these criticisms sometimes have to do with award policy and with the first criticism, about traditionalism and conservatism. On that issue, NIH and NSF acknowledge that there is sometimes a tendency for individual peer reviewers to be too conservative. But because of the briefing and emphasis which the staff of both agencies give to peer reviewers to avoid such practices, the problem was considered to be manageable. The three criticisms, then, considered singly and as related, cannot be set aside altogether. They indicate, not perhaps problems to be solved, but dangers to be avoided by encouraging breadth as well as scrutiny in reviews.

With respect to adequate protection against plagiarism, no reliable information is available to indicate whether this represents a real or potential problem. The requirements for adherence to practices designed to avoid conflicts of interest by peer reviewers and the knowledge of the contents of a given proposal by all the members of a panel would make a peer reviewer vulnerable to a serious charge if information in a confidential proposal were plagiarized. It would seem that a greater risk of plagiarism exists when peer review is done by mail.

The listed inadequacies represent opinions for which there is little definitive and documented evidence. The factors which go into making individual decisions about innovativeness, high-risk versus sure bet proposals, significance of past research performance in relation to promise of a research proposal, etc., are so many and varied that one cannot expect peer review or any other kind of review to result in uniform judgments by all reviewers.

Use of Results of Peer Review

Federal agencies which fund research have various objectives, differing from agency to agency and perhaps changing with time. Even though basic, much research serves perceived scientific, technological, medical, and social goals stipulated by Congress or set by the agencies. The Commission believes that these legitimate objectives should be invoked only subsequent to objective appraisals, often by the peer review process, of the scientific merit of proposals.

When factors other than simple ranking of scientific merit are appropriate, their use should be noted with candor within the agency and in dealing with the scientific community. In particular, it is unrealistic to assume or claim that the peer review process has a proven capability for more than assessment of scientific merit. Broad agency objectives need to be met by councils such as those of NIH, by program officers, or by agency officials who can make judgments which transcend the scientific review process. In turn, occasional perceived shortcomings of such overriding judgments should not be attributed simplistically to the peer review process.

There are now more meritorious research proposals submitted than are eventually supported. There is low likelihood of new, major funding in proportion to the number of such quality proposals. Therefore, ratings and rankings developed in review processes should not be used mechanically, or in any other routinely simple way, to make awards. Peer review processes should be continued and refined wherever possible. Agencies should endeavor to develop and make explicit relevant policy considerations that are taken into account in judging between proposals and particularly in deciding which research to support among high ranking proposals.

The days of simple division—what is supported is good science and what is not supported is weak—are gone. As much good science as possible should be supported. Funding policies, based on sound review processes, should be developed through explication of other rationale and consequences. Such openness would help to remove the mystery from the decision-making process while maintaining flexibility and enhancing the credibility of the peer review process.

Related Studies

As was noted earlier, a number of studies dealing with peer review in NIH and NSF have been conducted in the past decade. Some of them were conducted at the request of Congress, and most of them were financed by the agency being studied. "Appendix II: Selected Bibliography" lists many of these studies. The Commission has examined these documents and in some cases discussed the contents with their authors. Further discussions with persons in government and universities were held. Based on these deliberations, the Commission has concluded that the studies correctly determined that the criticisms do not form a pattern and, in most instances, arise out of isolated cases.

Several of the studies made recommendations for improvements in the operation of the peer review systems which have been implemented by the agencies. No such studies have been conducted on internal review processes.

Other Issues

Identification and Selection of Peer Reviewers

Identifying and selecting scientists to serve as members of a peer review panel or to perform mail reviews are the responsibilities of the staff members in each agency. In 1979, following a lengthy and detailed review of its peer review operation, NIH began a new system of nominations for both Study Section and National Advisory Council membership. Requests were printed in the *NIH Guide for Grants and Contracts*. More than 8000 nominations for the expanded Study Sections were received, of which approximately 3500 were considered to have appropriate qualifications and credentials. (The current pool size of established biomedical scientists is estimated to be about 75,000.) Approximately 845 scientists are involved in serving as regular members of Study Sections and about 500 to 600 as *ad hoc* reviewers (who may serve on special Study Sections formed when special reviews are needed or when conflict of interest may arise). Members of Advisory Councils number 162, including scientists and nonscientists. Turnover of membership is 25 percent annually. However, while membership is limited initially to one term of four years, reappointment can be made of previous Study Section or Advisory Council members after a lapse of one year. Prior to 1979, NIH had identified potential Study Section and National Advisory Council members through the advice of professional staff, current and past members, and other informal methods. The decision to broaden the base for nominations was made in response to recommendations of the NIH Grants Peer Review Study Team for a greater number of Study Sections and a larger pool from which to draw their memberships.[1,2] Primary responsibility for choosing the most competent reviewers still rests with the Executive Secretaries of the Study Sections, who now have a larger identifiable pool from which to draw.

Peer reviewers for proposals to NSF are identified by program officers, division heads, and others at NSF; current and past reviewers; and sometimes even the proposers (principal investigators) suggest appropriate reviewers. Selection for *ad hoc* mail or panel reviewers is made by program directors from computer lists compiled from various resources. There are 87,800 names in the pool. The average number of reviewers for each proposal at NSF is 8.5, usually not more than one of whom might have been suggested by the principal investigator.

When peer review is used at DOD, the reviewers usually are identified and selected by the scientific officers. The Chemistry Division of AFOSR, however, contracts with an external coordinator to identify and select the reviewers. Reviewers for proposals submitted to the Army Research Office are chosen by members of a special committee of the National Research Council.

At times, agencies will provide site visits by teams of reviewers from appropriate disciplines in cases involving either several disciplines or large and complex programs. There is some difficulty in finding peers to evaluate interdisciplinary proposals. Special attempts are made at both NIH and NSF to review such proposals adequately and equitably. NSF is beginning an internal study of its peer review process, including a Foundation definition of "interdisciplinary" and possibilities for saving time in the entire process.

Time and Cost Factors

Time Demands on Reviewers. A widespread complaint of university investigators engaged in laboratory research in the natural sciences is that they do not have enough time to do research. Within the university, considerable amounts of time are spent in classroom and graduate student teaching, departmental and university committee work, fulfilling increasing administrative regulations requiring written documentation relating to employment and termination of employment of research personnel, equipment purchase and maintenance, fiscal management of research funds, etc. To these time demands are added the considerable time needed for writing applications for research funds and progress reports and preparing manuscripts for publications. Success in research productivity results in additional demands such as reviewing manuscripts for professional journals, presentation of lectures at other institutions and scientific meetings, and frequently service in connection with professional scientific organizations. On top of all of these time demands, productive investigators are asked to serve as peer reviewers, as a member of a panel or by mail or both. Surprisingly, a considerable number of university investigators manage to do all of these and do them well.

The time spent by external reviewers evaluating submitted proposals is considerable. Prior to the new system of identification of nominees for membership in NIH Study Sections and National Advisory Councils and the addition of approximately twenty new Study Sections, the Grants Peer Review Study Team had identified heavy workloads as a significant problem for peer reviewers.[1] They had estimated in 1976 that the load was 120 hours of preparation for each reviewer three times a year, plus up to five days for each review session. Because of this, some qualified peer reviewers were beginning to decline invitations to serve on the Study Sections and National Advisory Councils. The increased number of Study Sections and the enlarged pool of nominated peers will help to alleviate this workload problem, but its effect, if any, on the quality of the reviewers is unknown. NSF has minimized this problem, primarily because it uses mail review predominantly in its peer review. While information is not available on the workload of the individual reviewer, an editorial in *Science* estimated that in U.S. science as much as 575 man-years were invested in reviewing research proposals in 1978.[3]

Length of Time Between Submission of Proposal and Final Decision. One of the criticisms of the competitive grant and contract system is that researchers spend a great deal of time writing proposals and then have to wait six to nine months for approval or declination from the agency. These criticisms are directed especially toward agencies in which peer review is used. They are sound in that review processes are indeed extensive, the number of staff and reviewing bodies is limited, the number of applicants is ever increasing, and the provisions that are made for careful and fair review are complex and time consuming. Consequently, there seems to be little that can be done to reduce the current lag between submission and approval or disapproval of proposals.

In the case of NIH, for example, the structured schedule of meetings of the Study Sections and Advisory Councils necessitates deadlines for receipt of applications. The deadlines must be sufficiently far in advance of the scheduled meetings of the Study Sections to permit reading and processing of applications and assignment by NIH staff to an appropriate Study Section. Sufficient intervals of time are needed then for (1) processing of applications (about 8000 to 10,000 for each deadline); (2) distribution to members of Study Sections; and (3) reading and preparation of summary assessments by the Study Section reviewers. Following a meeting of a

Study Section, further time is needed to prepare summary sheets and priority scores in preparation for use by the subsequent meetings of Advisory Councils. Following the meeting of the Advisory Council, the staff is finally in a position to advise the applicants as to the fate of their applications. Inquiry as to whether the overall time involved could be shortened by the addition of more staff at NIH was answered by indicating that very little time could be saved. A similar situation obtains for NSF when panels are involved in review.

With respect to peer review by mail at NSF, the time elapsing between mailing a proposal to a reviewer and the return of the review averages two months. Additional time is required for (1) processing the initial proposal upon receipt; (2) preparation by the program officer of a detailed critique after receipt of all of the reviews from the reviewers; and (3) review by the different levels of NSF staff. If further mail review is required, an additional two months may be added before a final decision can be made.

The long lag time between submission of an application and final decision creates serious difficulties for the investigator in planning for continued support for research personnel and recruitment of new personnel. In the case of a declination, an investigator may have to stop doing research and dismiss research personnel unless local or other funds can be found for interim support.

In the case of agencies using primarily internal review procedures and dealing with much smaller basic research programs, decisions may be reached in a much shorter time. However, it should be recognized that in some of these cases the applicant has had to submit beforehand an informal preliminary proposal which is evaluated as to suitability for consideration by the agency. If the initial proposal is considered to be of interest to the agency, a formal proposal is then requested, and it may be acted upon within a week or two. Information on the average total time involved between submission of an initial, preliminary proposal and final action on the formal proposal with internal review is not available at this time.

Monetary Cost. Monetary cost for peer review at NIH for the year 1979 was about one percent of its external research budget of $1.8 billion for new and competing grant applications. The Chemistry Division of AFOSR contracts with an external scientist to operate its peer review process; the cost is about one percent of that division's research budget. Within the NSF, peer review by mail is less expensive than peer review by panels. NSF spent approximately $18.2 million for peer review in FY 1979, which is about two percent of its total budget for that year. The NIH and NSF figures are not directly comparable, since the NSF figures include staff costs as well as the reimbursement and honorarium costs for panel reviews and the related costs of mail reviews. NIH figures do not include staff costs.

Some agency officials believe, "The true cost of peer review is not in the dollars we spend but in the labor we extract from the scientific community." Investigators believe peer review participation to be an obligation as well as a privilege, so they contribute their time and efforts to the review process. Most peer reviewers receive no compensation for mail reviews and only travel expense reimbursement and a minimal honorarium for panel work.

Summary

The ultimate assessment of quality and merit of research can be made only retrospectively. However, review processes should be examined periodically and account taken of valid criticisms to provide bases for judging the effectiveness and equity of the processes by which proposals are chosen for funding by federal agencies. The Commission states its conclusions and offers its recommendations for improvement of the examination of the review processes in the next sections of this document.

Conclusions

The Commission's deliberations regarding review processes yielded the following conclusions:

1. Scientists, universities, and the government all want a system of review and award which ensures equal opportunity to all investigators, minimizes error, assures fairness, selects the best research, and can be managed with reasonable costs of time and money. A system of review, however, which is totally free of error, differences of judgment, or personal preferences probably cannot be designed.

In submitting a basic research proposal to a federal agency, a scientist is offering his or her best research ideas for consideration. The proposal is the result of much thought and effort. It is, therefore, of the greatest importance to the scientist, particularly in a situation of keen competition for limited funds, that the proposal be considered fairly and critically by the most competent of his or her scientific peers.

It is equally important that the public, represented by Congress and the agencies, be assured that the money invested in research has funded the best research available. Elmer Staats, Comptroller General of the United States, maintains that peer review, where used, is the basis for scientific accountability for research funds.[4]

While it is recognized that there are rational bases for having some of the federal funding agencies use review processes other than peer review, it should be noted that the trend is in the direction of peer review rather than other review processes. Several federal funding agencies have completed studies of their research programs and have concluded that improvements could be achieved by using peer review of proposals. As noted earlier, the Department of Agriculture has been given appropriations by Congress to add a small, competitive grant program that will be reviewed externally and that will emphasize basic science and broaden the base for agricultural research. Other agencies which have initiated external review processes are the Biological Energy Research Program of the Department of Energy and the research programs of the Veterans Administration.

2. The Commission concludes that the plurality and diversity of review processes available are appropriate, provided that equality of opportunity is assured and that the quality of proposals selected is high.

On the whole, the Commission concluded that the systems of proposal review currently being used by the government agencies that sponsor research in universities are appropriate. These review processes for selection of high quality research are not identical, nor should they be. Agencies have chosen the systems that fit their needs according to the character of each agency, its mission, the type of research to be supported, and the oversight functions of Congress.

3. The Commission recognizes the criticisms of peer review and agrees that the peer review system is not immune to faults. Still, the system has worked well, and most agencies have been attentive to developing and testing appropriate safeguards to ensure its integrity and effectiveness

Fundamental criticisms need special attention, and safeguards against abuses of the peer review system must be expanded. NIH and NSF have examined and continue to study their systems, but not all agencies using peer review have been as attentive. Examples of safeguards already in use include rotation of membership on NIH Study Sections and Advisory Councils, rules regarding conflict of interest in NSF and NIH, and access to peer reviews by investigators through the Sunshine Laws.

Criticisms that are made by investigators who believe their proposals did not receive appropriate review must be handled on an individual basis. The formal appeals procedures of NSF and the informal appeals procedures at NIH are examples of how these systems might work.

Another aspect of the funding process which has been criticized is the use which is made of the results of peer review. Such results are perceived by some as used too mechanically; others hold that funding is not distributed as widely as it can and should be if it is to sustain a large research community. While little evidence has been presented in support of these criticisms, the Commission notes that such perceptions persist and warrant continuing attention.

4. The Commission believes that deciding the direction of U.S. science through the open competition of unsolicited proposals, and especially through the selection of proposals through peer review, is of great importance to the success of U.S. science and, therefore, deserves support.

Recommendations

1. The Commission recommends that the review systems used in the different funding agencies be examined periodically, both by internal and external groups, to ensure that they are working effectively and equitably.

Review systems are one source of scientific accountability and must be equitable and effective in so functioning. Peer review and internal review systems may develop weaknesses if they themselves are not reviewed carefully.

Extensive and intensive studies of peer review have been undertaken in both NIH and NSF. Similar studies should be conducted of internal review processes.

The Commission recommends not only periodic internal studies of review systems, but also congressionally financed studies. Vigilant surveillance over review systems is needed either by a special charge to the individual congressional oversight committees or by a separate body established for that purpose.

2. The Commission recommends experimentation within the funding agencies to refine the review processes and recommends that legislative or administrative restrictions which limit such experimentation be modified.

The periodic examinations presented in Recommendation 1 will suggest procedural modifications that would improve the review systems. For instance, the Commission believes considerable time and perhaps cost could be saved for all concerned if small, short-term grant applications to NIH could be reviewed by a procedure other than the dual peer review system mandated by the NIH legislation in Public Law 93-352. Certain exceptions are made already, but it may be necessary to amend the law to permit flexibility for the Director of NIH to initiate and implement experiments. Other experiments involving cost and time savings in other agencies should be considered after careful examination of the review systems.

3. The Commission recommends that funding agencies initiate retrospective studies on how effective their review processes have been in assessing the quality and impact of the research they support.

This recommendation stems from studies prepared for the President's Biomedical Research Panel in 1976 as well as from a recent, four-year study of the NSF peer review system supervised by the Committee on Science and Public Policy (COSPUP) of the National Academy of Sciences. Such retrospective studies could suggest improvements in the predictive accuracy of the review of proposals.

Studies should address the following issues:

1. Has the judgment of what is the best science remained the same, or have the criteria changed over time?
2. How does the retrospective assessment of results compare with the prospective assessment of the proposals for research?
3. Have agency program directors maintained high levels of competence in their program fields; to what extent have their priorities been similar to those of active researchers in the field?
4. What has been the justification for apportioning funds, and has this level of funding proved adequate for the research to go forward?
5. Have the programs been responsive to innovative, high-risk proposals, to young investigators, and to interdisciplinary areas?

4. The Commission recommends experimentation by federal agencies in which peer reviewers provide ratings for several aspects of a proposal as well as the overall rating. This would assist program officers in knowing what qualities and in what measure a peer reviewer has assessed the overall proposal and would assist program officers in making the funding decision.

Often peer reviewers disagree with one another about the quality of certain aspects of the same proposal. Categorical ratings as well as overall ratings would allow better comparisons among the reviews of the same proposal and among reviews by the same reviewer. When program officers make the funding decisions, they must sort through the reviews for salient points on which to base their impressions. One of the additional duties of the program officer in some agencies is to choose peer reviewers for proposals, and the information provided in a categorical rating system would help agency personnel to assess the character of the reviews from individual reviewers.

Categories should include the criteria deemed important to the individual agency, such as the following:

1. feasibility of research design;
2. past research performance of the investigator;
3. available resources and facilities;
4. expected contribution of the results to the field; and
5. innovativeness of proposal.

COSPUP has made a similar recommendation regarding the NSF peer review system, and the Commission is in general agreement with that suggestion.

5. The Commission recommends that funding agencies make explicit the nature of the underlying scientific and policy considerations used in arriving at funding decisions.

There is a perception that the clustering of meritorious proposals around a narrow range of priority ratings is being resolved by arbitrary numerical cutoffs. The Commission recognizes that factors in addition to the peer review rating must be taken into account in reaching a funding decision by a given agency. Such factors would include relevancy to the mission of the agency, impact on a newly emerging area of research, unique circumstances to pursue an area of research, as well as scientific, technological, medical, and social goals stipulated by Congress or set by the agencies. While a legitimate and rational remedy for the difficulties caused by limited funding and the large number of meritorious applications would be an increase in funds to support more of the meritorious proposals, it must be recognized by the scientists, on the one hand, that considerations in addition to pure scientific merit are proper and necessary in resolution of this situation. On the other hand, the funding agencies must recognize that the use of factors in addition to priority ratings must be based on sound scientific and policy considerations.

Established 1845 SCIENTIFIC AMERICAN October 1977 Volume 237 Number 4

Peer Review and the Support of Science

A statistical analysis of the evaluative procedures on which the National Science Foundation bases its funding decisions provides no evidence to substantiate recent public criticisms

by Stephen Cole, Leonard Rubin and Jonathan R. Cole

For more than 25 years the National Science Foundation has played a major role in the expenditure of public money for the support of science in the U.S. Currently the NSF accounts for about 20 percent of the funds distributed by the Federal Government for basic scientific research and more than 30 percent of the Federal funds allocated for such research at universities. The NSF awards its grants on the basis of a decision-making process commonly known as peer review. The term is derived from the fact that the Government officials responsible for deciding which investigators receive grants rely on the evaluations of other investigators in the same discipline.

In recent years the peer-review system has been attacked for a variety of reasons by certain members of both the scientific community and the Congress. Hearings on the alleged inequities of the peer-review system were held two years ago by a subcommittee of the House Committee on Science and Technology.

In an effort to assess the validity of the public criticisms of the peer-review system raised in the Congressional hearings and elsewhere we have been engaged for more than a year in a sociological study of the operation of the peer-review system at the NSF. This study, which is being conducted for the National Academy of Sciences, is supported by grants from the NSF; we have nonetheless had complete autonomy from the NSF in conducting our research. Our results to date have yielded little evidence in support of the main criticisms that have been made of the peer-review system. On the contrary, we have tentatively concluded that the NSF peer-review system is in general an equitable arrangement that distributes the limited funds available for basic research primarily on the basis of the perceived quality of the applicant's proposal. In particular, we find that the NSF does not discriminate systematically against noneminent scientists in the ways that some critics have charged. This is not to say, of course, that there are not errors in individual cases.

How does the NSF peer-review system work? To begin with, a scientist who wants to obtain NSF funds prepares a written proposal describing his past research, his qualifications and the new research he intends to do if he receives funds from the NSF. This proposal is usually submitted to the NSF through the scientist's institution, in most cases a university.

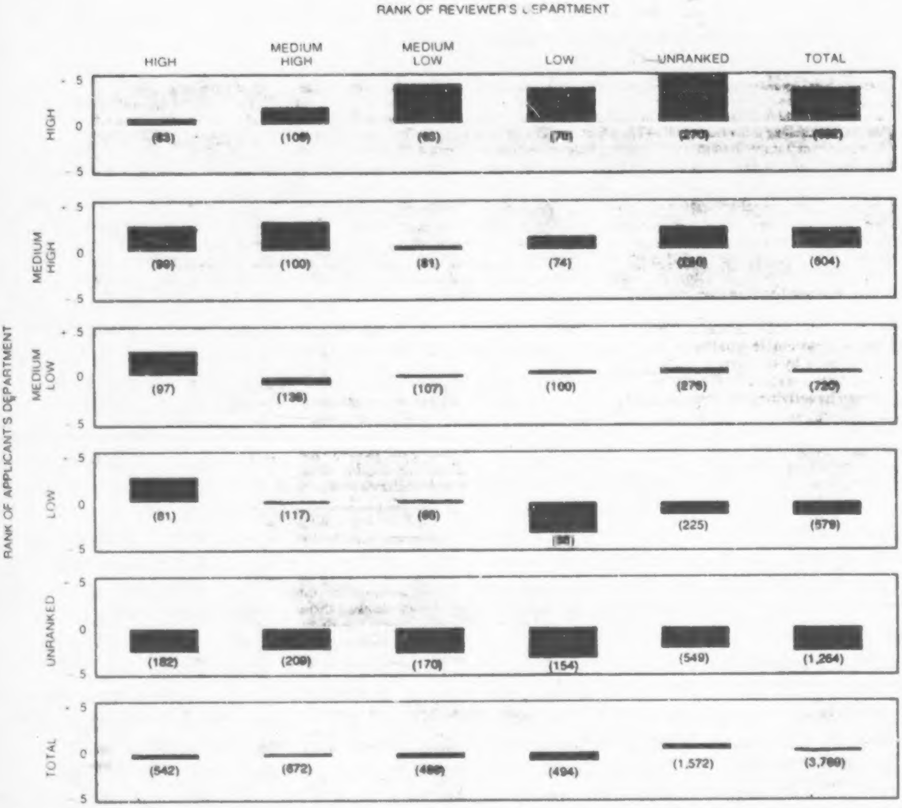
The staff of the NSF is divided into approximately 80 program areas corresponding to the various scientific disciplines and subdisciplines. (The chemistry section, for example, is divided into eight different programs.) When a research proposal comes to the NSF, it is assigned to the appropriate program and is thereafter handled by an employee of the NSF called the program director. On receiving a proposal the program director generally looks it over to determine its specific subject area. He then selects a number of reviewers who are sent the proposal by mail. The reviewers are asked to rate the proposal as being excellent, very good, good, fair or poor and in support of their rating to present written comments evaluating the proposal. In some programs an independent evaluation of the proposal is also made by a panel of scientists who meet with the program director three times a year in Washington.

The NSF explicitly states to its reviewers the criteria that should be applied in evaluating the proposals. The main criteria are (1) the significance of the scientific investigation described in the proposal. (2) the ability of the applicant to carry out the proposed research and (3) the capacity of the applicant's institution to support the type of research in question. Where all these factors are roughly equal. another set of criteria. including the geographic location of the applicant's institution. may be considered. Heavy emphasis is placed on the quality of the work described in the proposal and on the past research performance of the applicant.

The most fundamental criticism made of the NSF peer-review system is that it leads to inequitable decisions. Critics charge that scientists who are most capable of advancing science are sometimes denied grants and that scientists who are doing less significant work are given grants. Former Representative John B. Conlan of Arizona. for example. asserted at the Congressional hearings that peer review is essentially an elitist system run primarily for the benefit of a clique of eminent "old boys." He said: "I know from studying material provided to me by the NSF that this is an 'old boy's system' where program managers rely on trusted friends in the academic community to review their proposals. These friends recommend their friends as reviewers.... It is an incestuous 'buddy system' that frequently stifles new ideas and scientific breakthroughs. while carving up the multimillion-dollar Federal research-and-education pie in a monopoly game of grantsmanship."

Critics in and out of Congress maintain that the main organizational condition that gives rise to this unfair distribution of support is the extraordinary power in the hands of the program directors to decide who should get funds. The program director is alleged to be at the center of the old-boy network in which reviewers favorably evaluate the proposals of their friends. eminent scientists favorably review the proposals of other eminent scientists and funds are denied to scientists who are not part of the exclusive old-boy system.

Further abuse is said to be possible because the reviews received by the program director are only advisory. leaving him free to ignore them. and because the program director can predetermine the outcome by selecting a biased group of reviewers. The critics argue that knowledgeable program directors deliberately select reviewers who will be either hard or easy on a particular proposal. Even if



STATISTICAL ANALYSIS of 3,769 peer-review ratings given by various mail reviewers to 1,200 applicants for basic-research grants from the National Science Foundation in the fiscal year 1975 was aimed at testing the "old boy" hypothesis, which holds that the proposals of eminent scientists are apt to be rated more favorably by eminent reviewers than by other reviewers. The ratings in the 10 different program areas studied were first converted into standard scores in the following manner: Within each field the mean rating was set at zero, and the rating received by an applicant was then expressed in terms of the corresponding number of standard deviations above or below the mean rating. A high number means a comparatively favorable rating, and vice versa. Both the applicants and the reviewers were separately classified according to the prestige of their current academic department, as determined in an independent survey. Thus the entry in the upper left-hand corner of the table signifies that there were 83 reviews by reviewers in high-ranked departments of proposals submitted by applicants from high-ranked departments; on the average these reviews yielded ratings that were .05 of a standard deviation above the mean. Since it appears that proposals from applicants in high-ranked departments are actually rated lower by reviewers from high-ranked departments than by reviewers from lower-ranked departments, in this sample at least the data offer no support for the old-boy hypothesis. The analysis does show that applicants from high-ranked departments are slightly more likely to receive favorable ratings than are those from unranked departments, but there is no evidence that this outcome is the result of inequitable treatment.

the program director feels compelled by the reviews to support a proposal he dislikes, he can effectively stifle the research by reducing the size of the budget. The program director can supposedly do so because there are no effective checks on his power either inside or outside the NSF. In short, there is no appeals system to challenge the decisions made by the program director.

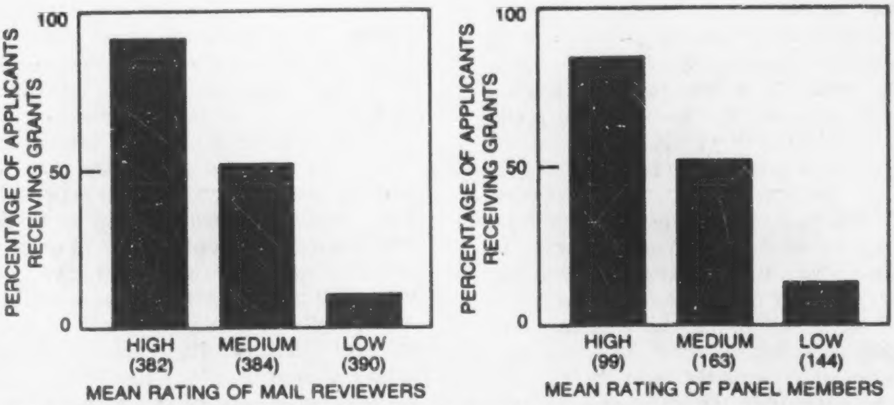
Critics assert further that the NSF cloaks its activities in secrecy in order to protect the old-boy system, refusing to allow Congressmen or others to see verbatim reviews or to learn the names of the reviewers of particular proposals. This protective shield of confidentiality enables the old-boy system to function unchecked and prevents effective oversight of the NSF by Congress. The ultimate consequence is that the peer-review system actually stifles innovative research, since the eminent scientists who serve as reviewers are likely to reject ideas that differ from their own.

In our study of the peer-review system we decided to limit ourselves at first to an examination of how peer review works in just those NSF programs responsible for the funding of basic research. We have not studied peer review in the NSF's applied-research programs or in its educational programs. Furthermore, we chose a sample of only 10 basic research programs for detailed study: algebra, anthropology, biochemistry, chemical dynamics, ecology, economics, fluid dynamics, geophysics, meteorology and solid-state physics. Because our intensive analysis included only about an eighth of the NSF's basic-research programs our results may not be generalizable for the entire organization. We are currently conducting follow-up studies of other programs.

Our investigation has combined both qualitative and quantitative sociological techniques. We began by conducting 70 in-depth interviews with scientists involved at all levels of the peer-review system, including program directors, former program directors, mail reviewers, review-panel members and supervisory-level NSF officials. We also scrutinized more than 250 specific research proposals, read all of the peer-review comments on those proposals and examined all of the correspondence between the applicant and the program director. In some cases in which our analysis of the applications raised specific questions about how the peer-review system worked in that particular situation we went back and reinterviewed program directors with the files in hand.

In addition, we conducted a quantitative analysis of 1,200 applicants to the NSF in the fiscal year 1975. (Roughly half of the applicants were ultimately awarded grants.) The purpose of the quantitative study was to identify those characteristics that were correlated with the receipt of a grant from the NSF. Were Representative Conlan and the other critics of peer review correct in their assertion that eminent scientists have a great advantage in the competition for funds and that less eminent scientists, particularly younger ones, are at a serious disadvantage? We shall try to answer this question by summarizing below some of the results obtained so far in our study.

One of the main charges of the critics is that the NSF program director can predetermine the outcome of the peer-review process by sending a proposal to scientists who he knows in advance are biased either in favor of the proposal or against it. We shall call this view the old-boy hypothesis. Presumably the proposals of eminent scientists who are members of the old-boy net-



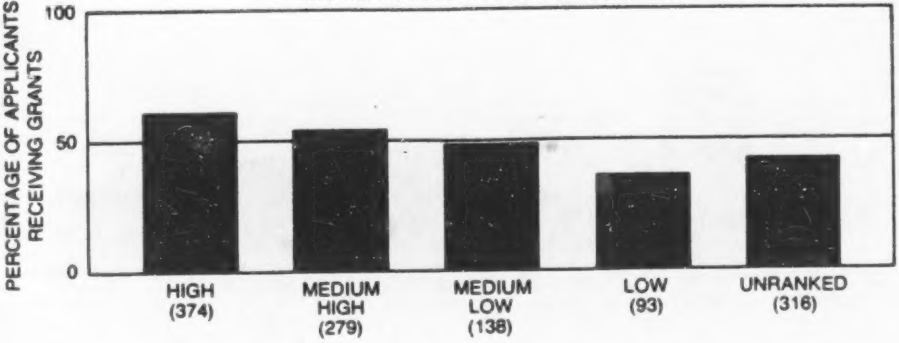
NSF PROGRAM DIRECTORS appear to rely heavily on the evaluations of the peer reviewers in deciding whether or not a research proposal is to be funded. As the chart at left shows, among the 382 applicants who received comparatively high ratings from the mail reveiwers 92 percent were awarded grants, whereas among the 390 receiving low mean ratings only 10 percent received grants. Similarly, as the chart at right shows, among those proposals that received comparatively high ratings from an independent panel of peer reviewers 84 percent were funded, whereas among those that received low panel ratings only 12 percent were funded. Evidently peer-review ratings are the most important determinant of the program director's decision.

work are sent to other eminent scientists who give their eminent colleague a favorable evaluation. In return, of course, the reviewers expect reciprocity when their proposals are sent to other members of the old-boy club. Equally important, the proposals of less eminent scientists, who are not part of the network, are sent to scientists who will give them lower evaluations than they deserve. Although we have no direct evidence that the program directors either do or do not select reviewers with a certain outcome in mind, we can see if the outcomes are consistent with the old-boy hypothesis. Are the proposals of eminent scientists actually rated more favorably by eminent reviewers than by other reviewers?

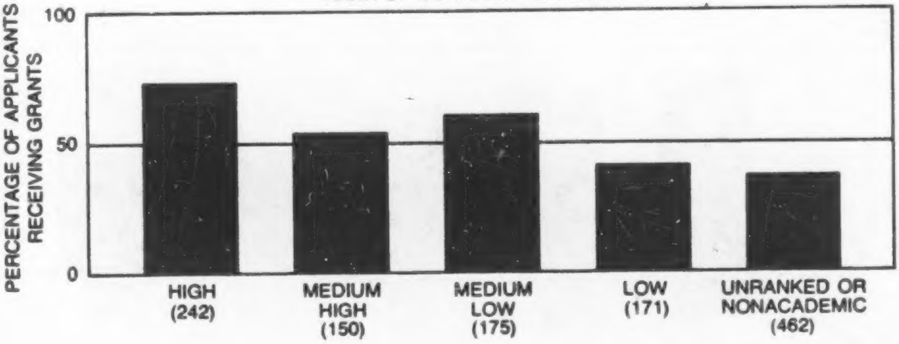
To test this hypothesis we classified both the applicants and the reviewers according to the prestige of their current academic department, as determined by a survey conducted in 1969 by the American Council on Education. The ratings given to the applicants by the reviewers in the 10 programs we studied were standardized separately before being combined into one large table [*see illustration on preceding page*]. For example, there were a total of 83 cases in which an applicant from a high-ranked department had his proposal reviewed by someone who was also from a high-ranked department. The number associated with this particular applicant-reviewer pair (+.05) indicates the average rating (in standardized units) given by high-ranked reviewers to proposals from high-ranked applicants. The higher the number, the higher the rating.

In general we found that applicants from high-ranked departments received slightly better reviews of their proposals than applicants from medium-ranked and low-ranked departments. Furthermore, it appeared that high-ranked reviewers tend to be slightly more lenient with proposals than low-ranked reviewers are. These results, in and of themselves, cannot be interpreted as offering support for the old-boy hypothesis. For example, the fact that eminent scientists tend to get higher ratings could simply be a result of the higher quality of their proposals or of the belief on the part of the reviewers that the eminent scientists are in fact better able to carry out the proposed research.

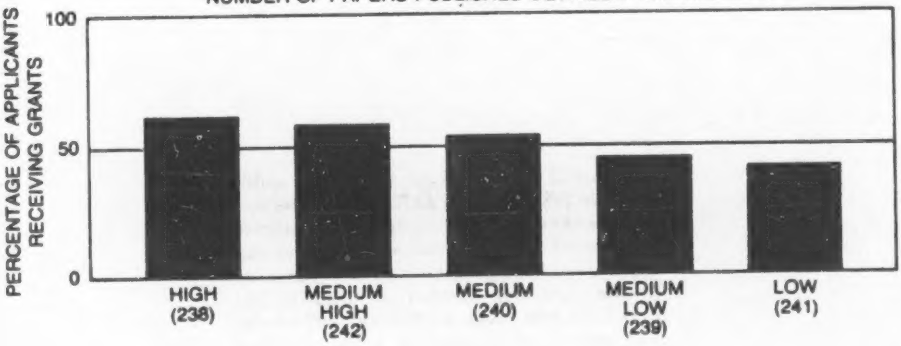
In order to explore the matter more deeply we next conducted a statistical analysis of variance that compared the observed mean rating for each applicant-reviewer pair with the expected mean rating, assuming no bias. The results of this analysis indicated that in general reviewers from high-ranked departments were not disproportionately favoring proposals from applicants in similarly high-ranked departments. We conducted this analysis separately for each of the 10 programs. In only one



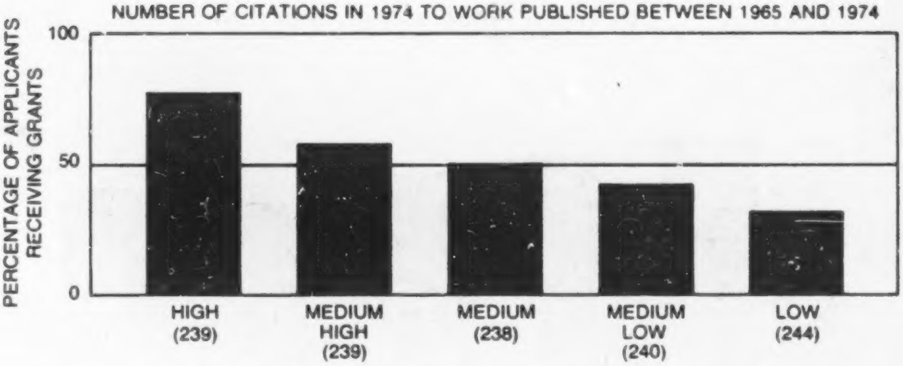
RANK OF PH.D.-GRANTING DEPARTMENT

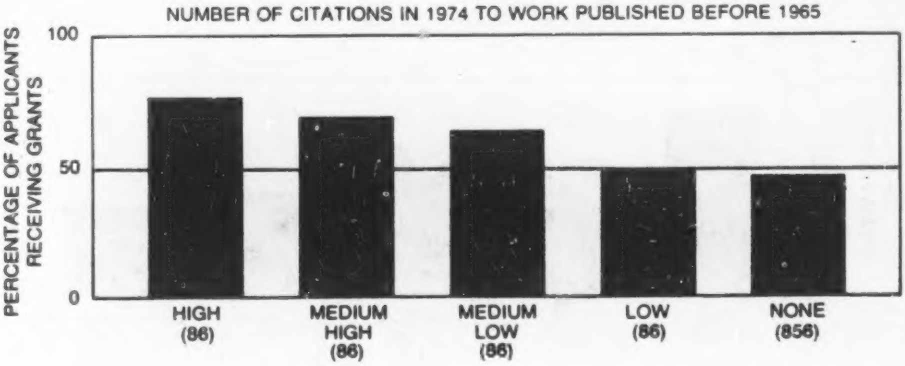


RANK OF CURRENT DEPARTMENT



NUMBER OF PAPERS PUBLISHED BETWEEN 1965 AND 1974





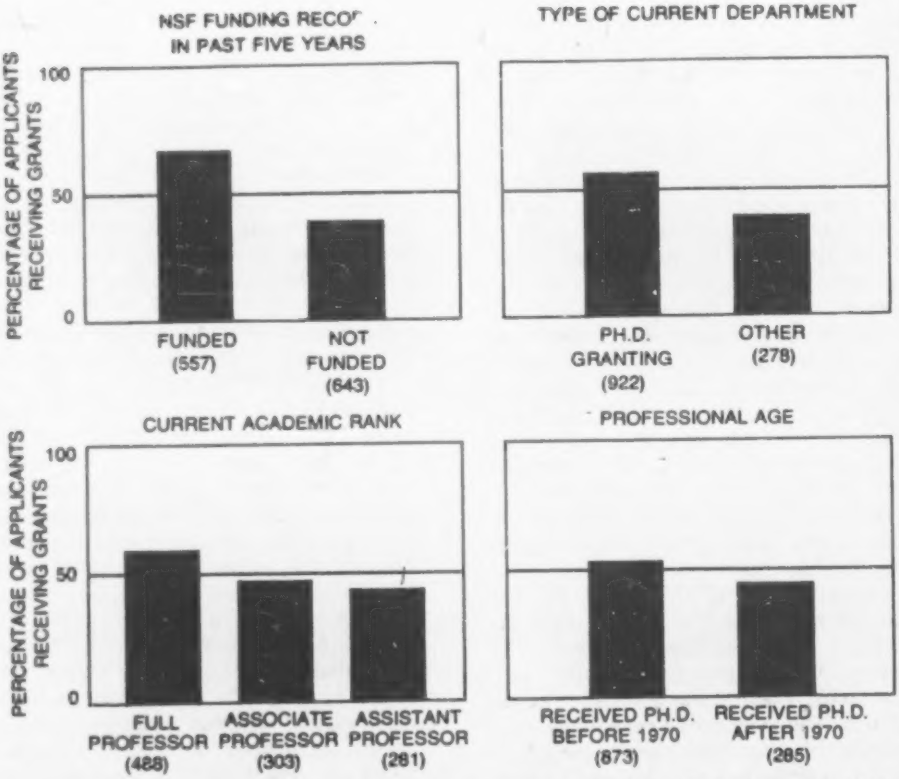
CHARACTERISTICS of successful applicants for NSF grants in 1975 are summarized in these bar charts. Among the characteristics represented here are rank of Ph.D.-granting department (*top*), rank of current department (*second from top*), number of scientific papers published between 1965 and 1974 (*middle*), number of citations to work published between 1965 and 1974 (*second from bottom*) and to work published before 1965 (*bottom*).

program were reviewers at high-ranked departments detectably more lenient toward the proposals of their colleagues at similarly high-ranked departments.

Another statistical analysis of variance tested the reviewers' bias in terms of geographic location and of the relative eminence of the reviewer and the applicant. It showed no significant tendency for scientists in one part of the country to favor proposals from colleagues in their own region or for eminent scientists to favor the proposals of eminent scientists over the proposals of less eminent scientists. Thus even if it were true that the program directors at the NSF were attempting to manipulate the outcome of the peer-review process by their selection of reviewers (and our qualitative findings indicate that it is unlikely), the quantitative data suggest that they have not been successful.

One reason it is difficult to test the validity of the old-boy hypothesis is the absence of conceptual clarity in the charge. What is referred to by the old-boy label? There are at least three possibilities. The term could refer to investigators with a common view of their field who will only appraise favorably work that is done by people with similar views. It could refer to networks of friendships: scientists who know one another, who "grew up" together or attended the same schools and who tend to fraternize and also to favor one another's proposals. It could refer to social position: scientists at a given level of eminence might tend to favor the proposals of others who are similarly situated in the hierarchy of science, even if they have no personal contact with them. Critics of the peer-review system never specify clearly which form of old-boyism is undermining the peer-review system. The data reported here allow us to examine the assertion that persons of similar rank, similar intellectual background and similar repute favor one another's proposals, but we do not have in hand data for examining forms of old-boyism that may be connected with friendship patterns.

How do the characteristics of the applicants affect the peer-review ratings they receive? Critics of the peer-review system say that regardless of the quality of proposals eminent scientists enjoy an advantage over those who are



OTHER CHARACTERISTICS of successful applicants for grants in 1975 are represented in these charts. Characteristics include past five years' funding record (*top left*), type of academic department (*top right*), academic rank (*bottom left*) and professional age (*bottom right*).

not eminent. In the final analysis, these critics contend, the peer-review system results primarily in eminent scientists at high-ranked departments having an unfair advantage in grant approval over less eminent scientists at lower-ranked departments. To test this "rich get richer" hypothesis we combined the applicants from all 10 programs into one large standardized sample. The 1,200 applicants in the sample were characterized by nine variables that established their status in the social system of science. Each of these characteristics was then tested separately to see if it provided evidence in support of the rich-get-richer hypothesis.

For example, we characterized the applicants according to the graduate departments from which they obtained their doctoral degree to see if scientists that come from prestigious Ph.D.-granting departments tend to get higher ratings than those who come from less prestigious departments. The applicants were also classified according to their current academic departments in order to test the assertion that applicants in high-ranked departments have an undeserved advantage over applicants in low-ranked departments. We classified the applicants according to their current academic rank in order to see if assistant professors are any less likely to receive grants than associate professors or full professors. In addition we classified all the applicants according to their professional age, their published scientific works, the number of citations of their published works and whether or not they had received NSF funds in the past.

The rich-get-richer hypothesis would suggest the existence of strong correlations between all of these variables and the ratings the applicants received on their proposals. There are, indeed, reasons other than old-boyism for this expectation. For one thing scientists who in the past had done research that other scientists had valued highly could reasonably be expected to write proposals that would be more likely to be rated highly. Moreover, since the NSF explicitly instructs reviewers to regard past performance as one of the major criteria in determining a rating, reviewers could be expected to give higher ratings to scientists with a superior "track record."

The data, however, provide little support for the rich-get-richer hypothesis. Our results show only weak or moderate correlations between each of the nine "social stratification" variables and the ratings received on proposals. The most highly correlated variable was the number of citations in the 1975 *Science Citation Index* of work published between 1965 and 1974. Even this rough measure of the significance of recently published work is not correlated very strongly with the ratings, explaining only 6 percent of the variance in the ratings. The correlations between the other variables and the ratings are all surprisingly low, explaining only an additional 5 percent of the variance in the ratings. In the end 89 percent of the observed variance in the ratings is left unexplained by the nine variables.

These results ran so counter to our expectations that at first we suspected they might have been caused by some methodological error. A thorough review of our correlation and regression procedures, however, left the results intact. In fact, the validity of our findings has been corroborated by a recent study conducted by members of the NSF's own chemistry section. Their independent analysis yielded results that were virtually identical with our own. It is difficult to avoid the conclusion that there is no substantial correlation between peer-review ratings received by applicants for NSF grants and statistical indicators of their professional status or past scientific performance. Scientists whose published work is frequently cited were only slightly more likely to receive favorable ratings than scientists with only a few citations or none.

It still appeared possible, however, that the weak correlations we observed could have resulted from a lack of agreement among the reviewers. For example, if an applicant with a large number of citations of his work received very favorable ratings from some reviewers and very unfavorable ones from others, that could account for a weak or nonexistent correlation between citations and ratings. How much agreement was there among the various reviewers of a given research proposal?

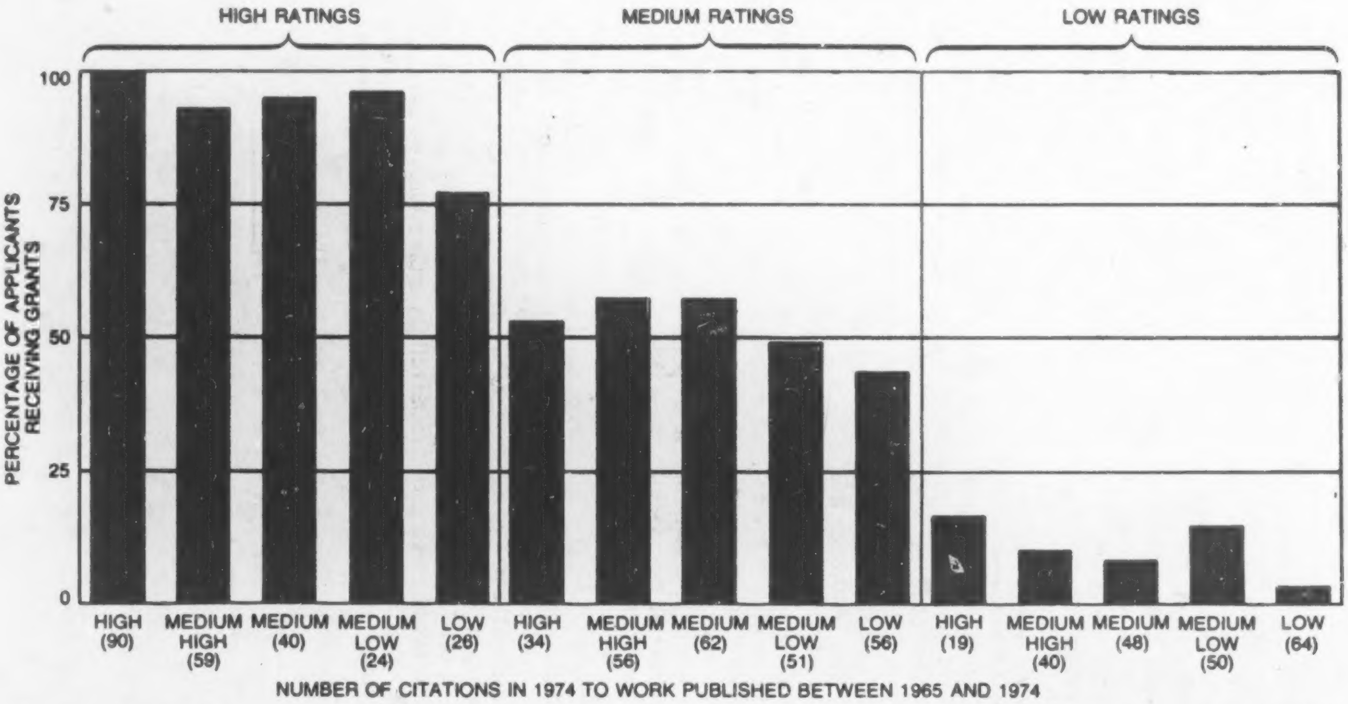
To answer this question we first determined the mean standard deviation of the reviewers' ratings, a quantity that can be taken as an approximation of the degree of agreement in a given field. This number varied from a low level of .31 in algebra to a high level of .69 in ecology and meteorology. (A low mean standard deviation corresponds to a high degree of consensus, and vice versa.) This approach could itself be flawed, however, if one were to fail to take into account the mean rating of the reviewers in each field. Clearly if there is a general tendency in a field to restrict the range of evaluations to either high or low scores, there would be less chance for variations in the ratings. We therefore relied on a statistic called the coefficient of variation, which is simply the mean peer-review rating divided by the mean standard deviation. In general we found that there was a good deal of agreement among the mail reviewers in all 10 fields and little systematic variation among the fields. The coefficient of variation ranged from a low of .13 in economics to a high of .30 in ecology.

To test further the notion that the weak correlations we observed resulted from a lack of agreement among the reviewers, we examined the correlations between the mean rating received by a proposal and several characteristics of the applicant. If the weak correlations had resulted from a lack of agreement among the reviewers, the associations between mean ratings and individual characteristics would be substantially higher, since mean scores are almost invariably more strongly correlated with any given variable than are individual scores. When the mean rating was used as the dependent variable in a statistical regression analysis, we obtained results similar to those obtained in our original analysis. The highest correlation was found between citations of recent work and the mean rating, followed by the correlation between past funding history and the mean rating. Although this method of analysis had the effect of increasing the amount of variance explained by the characteristics of the applicants from 11 percent to 16 percent, the great bulk of the observed variance in the ratings remained unexplained. The new analysis supported the conclusion that the weak correlations observed were not a result of a lack of agreement among reviewers.

In short, these data suggest that the mail reviewers are not strongly influenced by the professional status of an applicant in evaluating a proposal. On the contrary, they appear to be much more likely to be influenced by their perception of the quality of the research proposed. One crucial question remained: How is the program director's funding decision related to the reviewers' ratings on the one hand and to the characteristics of the applicants on the other?

Critics of the peer-review system contend in effect that the decisions of the NSF program directors depend more on who you are than on what you propose to do. So far our data have tended to refute this version of the old-boy hypothesis. Before this refutation can be established conclusively, however, we must establish that the peer-review ratings are the single most important determinant of the program director's funding decision and that the characteristics of the applicants have little independent effect on the outcome.

The NSF states clearly that the reviews by either the mail reviewers or the panel members are advisory and the program director has the final responsibility for deciding whether or not a proposal is to be funded. Our data show that the program directors in fact rely very heavily on the evaluations of the peer reviewers. For example, among those applicants who received comparatively high mean ratings from the mail reviewers 92 percent were awarded grants, whereas among those receiving low mean ratings only 10 percent got grants. Among the group who received mean ratings in the middle ranges about half were awarded grants. Similarly, among those applicants who received



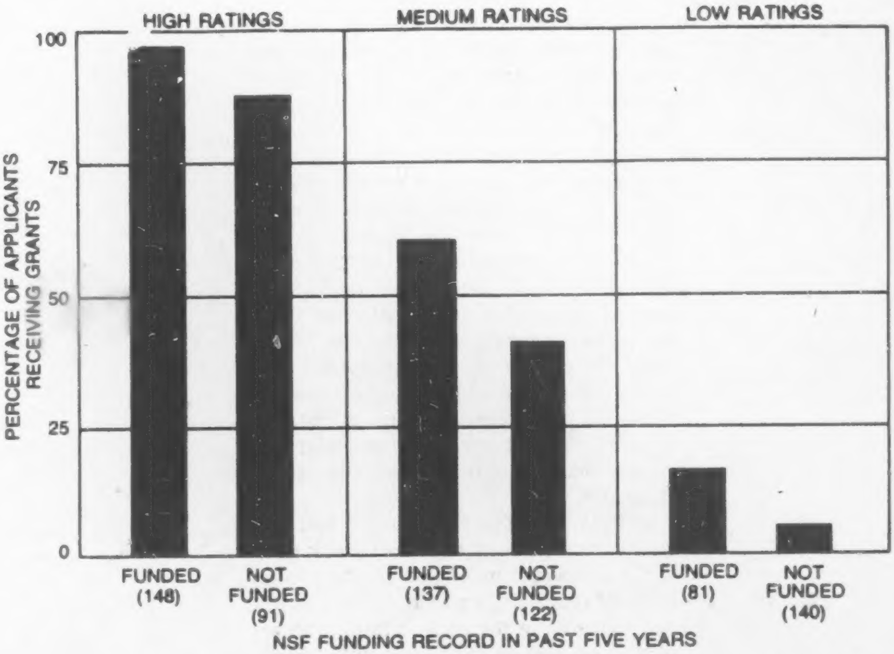
INDEPENDENT EFFECTS of a scientist's past achievements on the probability of receiving an NSF grant are represented in this chart and the one on the next page. The applicants were divided into three groups: those who received comparatively high mean ratings from mail reviewers, those who received medium mean ratings and those who received low mean ratings. Within each category the probability that particular scientists—in this case those with different numbers of citations of their recent work—would receive grants was then calculated. The results show that scientists whose work is frequently cited have a slight competitive advantage in the competition for funds.

comparatively high panel ratings 84 percent were funded, and among those who received low panel ratings only 12 percent were funded [*see illustration on page 36*].

What types of scientists were successful in receiving grants from the NSF in 1975? Of those applicants who obtained their degrees from the highest-ranked graduate departments 62 percent were awarded grants, compared to 38 percent of those who were graduated from the lowest-ranked departments. Similarly, 74 percent of the applicants currently employed in the highest-ranked departments were funded, compared with 38 percent currently in either unranked departments or nonacademic institutions.

Recent NSF funding history and frequency of citations of recent work both had a moderate influence on the probability of receiving a grant. Among applicants receiving the most citations to recently published work roughly three-quarters were awarded NSF grants; among those receiving the least citations of recent work less than a third received grants. The number of papers published and the number of citations of work published before 1965 were less strongly associated with the receipt of a grant. Other attributes of the applicants, such as their professional age or their academic rank, had a minor effect on the probability of receiving a grant.

The effect of professional age on the probability of receiving an NSF grant is



SIMILAR ACCUMULATIVE ADVANTAGE is indicated, among those scientists whose proposals received medium or low peer-review ratings, for applicants who had been funded by the NSF in the past five years. Again, a good record appears to produce a slight advantage.

particularly noteworthy. When we began our study many scientists indicated that they believed it was more difficult for younger scientists to obtain NSF funds. Our interviews with program directors, on the other hand, revealed that they perceived just the opposite. Because there is a commitment on the part of the NSF to help young, talented scientists get started, several program directors said that in the case of roughly equal peer reviews they would prefer to fund younger applicants. As it happens, the perceptions of both the applicants and the program directors are mistaken. The data we have gathered indicate that professional age has almost no effect on either the peer-review ratings or the final funding decision.

The overall pattern of our data suggests that scientists with an established track record, many scientific publications, a high frequency of citations, a record of having received grants from the NSF and ties to prestigious academic departments have a higher probability of receiving NSF grants than other applicants do. Nevertheless, the granting process is actually quite open, and there is nothing approximating a scientific caste system. Even among the most frequently cited scientists who apply for support an appreciable number do not receive grants, and among the group with the fewest citations to their work a significant number do receive grants. There is no evidence that scientists who have received grants in the past are guaranteed continued support, or that those without a past funding record have no chance of obtaining current NSF funding. Indeed, given the heavy emphasis the NSF places on past performance as one of the two most important criteria in evaluating research proposals, it is somewhat surprising that measures of past scientific performance do not show a stronger influence on the probability of receiving a grant.

It should incidentally be noted that the data presented here allow us to answer two distinct questions. The first is: How well do the social characteristics of scientists and their previous record predict peer-review ratings and the probability of funding in general, that is, when we examine the entire sample of applicants? The second is: Are there substantially different probabilities of receiving high ratings or a favorable decision for the most eminent applicants compared with the least eminent applicants, that is, when we compare relatively small subsets of the sample? The answers can be different depending on which of these two questions we ask.

For the sample as a whole status differences are not good predictors of ratings. Consider a concrete example of what we mean by focusing again on the relation between the rank of an applicant's current department and the final funding decision. First recall that 55 percent of all 1,200 applicants received NSF grants; if one had to predict whether an individual applicant had received a grant, to predict in every case that he had received one would make one right on 55 percent of the applicants and wrong on 45 percent. The question is: How does knowledge of the rank of an applicant's department increase the ability to predict whether he received a grant? To estimate this we examine each of the five classifications of departmental rank. In the two bottom categories, where a majority did not obtain support, we would guess that all applicants did not receive grants; in the other three categories, where a majority received support, we would do better to guess that all received support. That would result in correct predictions in 63 percent of the cases. When we subtract from this total the proportion (55 percent) that we would have guessed correctly without any information about the individual's departmental affiliation, we get an estimate of the increase in predictability that results from knowledge of rank of department: in this case an increase of 8 percent, which is not an extraordinary increase in predictability.

Suppose, on the other hand, we want to know whether scientists in the highest-ranked departments have a better chance of receiving NSF support than those in unranked departments or in a nonacademic setting. If we compare the percentage difference between these extreme subgroups, we find a substantial 36-point difference. In other words, some percentage differences do appear large in the extremes, but that does not mean the characteristic is a good predictor of a decision for the entire sample. Of the variance that can be accounted for in funding decisions, the peer-review rating is by far the best predictor.

The well-documented social process referred to by sociologists of science as "accumulative advantage" would lead one to expect that eminent scientists have a better-than-average chance in the competition for NSF funds. Accumulative advantage in this context means that a scientist who has been rewarded at one stage in his career has an enhanced probability of being rewarded at a later stage, regardless of the quality of his scientific work in the interim. The concept explains in part the increasing inequality in rewards that is observed as an age cohort of scientists moves through time.

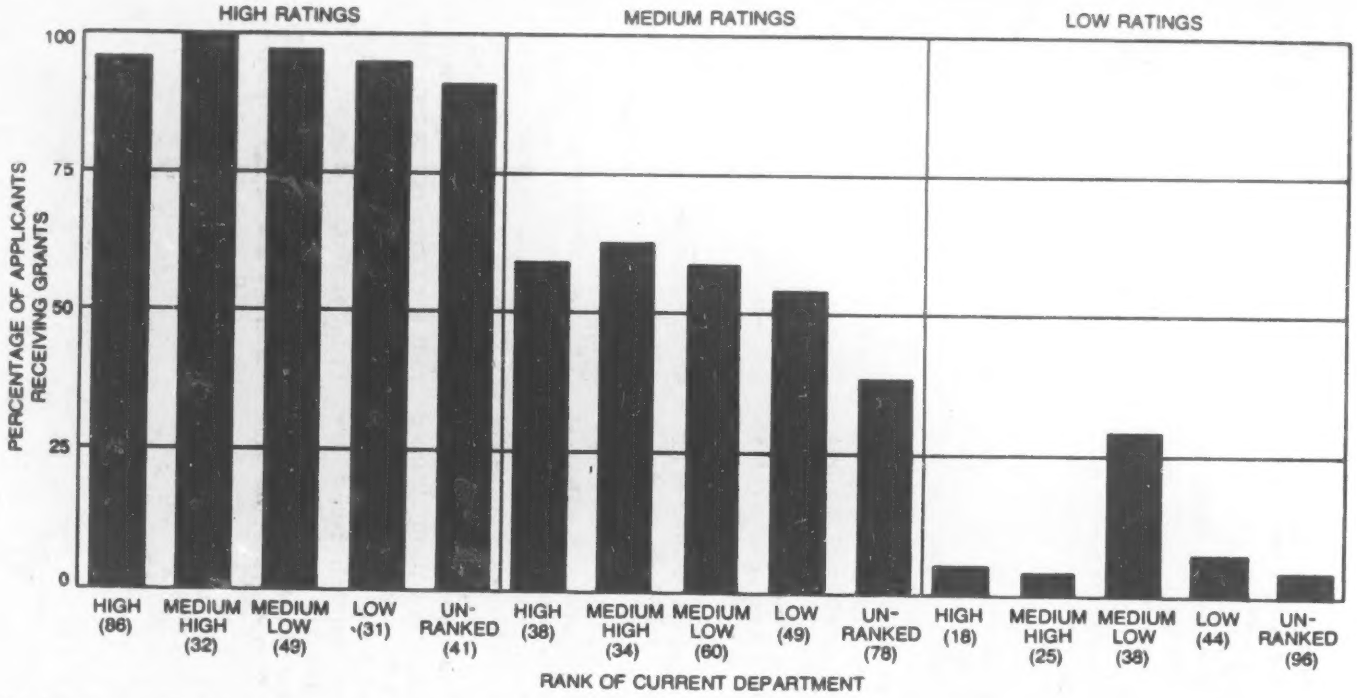
According to the concept of accumulative advantage, the initial social status of a scientist influences the probability of his obtaining a variety of forms of recognition, including the esteem of his colleagues, an association with centers of excellence in the academic world and the resources and facilities necessary for productive scientific work. For example, young scientists who are trained in the best university science departments, and particularly those who have been apprenticed to leading scientists, have a better chance than less well-placed students of equal ability to secure first jobs at prestigious institutions. Once established in these positions they have a better chance than their peers to obtain support for their research. With greater support they have an enhanced opportunity for making significant scientific discoveries and publishing the results. And once the results are published they have still greater chances for future success. To the extent that this process works to the advantage of scientists who are initially well placed in the social system of science it also works to the disadvantage of their peers who are not so fortunate.

By taking the mean peer-review rating received by an NSF research proposal as a rough measure of the quality of the proposal we attempted to determine the independent effect of a scientist's past achievements on his receiving a grant. We first divided the applicants into three groups: those who received comparatively high mean ratings, those who received medium mean ratings and those who received low mean ratings. Within each category we calculated the probability that scientists who had had different numbers of citations of their recent work would receive grants. We then considered only the group of proposals that received the highest peer-review ratings. Of this group 100 percent of the quintile with the highest number of citations were awarded NSF grants. In the lowest quintile 77 percent received grants. This finding leads to two conclusions: (1) the mean peer-review rating is a far more important determinant of whether a scientist receives a grant than is the number of citations of his recent work, and (2) within each category of mean ratings the number of citations of recent work has only a slight influence on the probability of approval.

We next considered the cases of those scientists whose proposals received low ratings. A substantial majority of all the proposals in this category were declined, but the number of citations made little difference. Within the group of proposals that received low ratings 16 percent of the scientists with the most citations received grants, compared with 3 percent of those who received the fewest citations.

The foregoing data offer some limited support for the concept of accumulative advantage. Scientists whose recent work has been frequently cited have a measurable advantage in the competition for current funds; this advantage is, however, very slight. The process of accumulative advantage is somewhat more evident among those scientists whose research proposals received medium peer-review ratings but who had been funded frequently by the NSF in the past five years. Among scientists whose proposals received medium ratings, for example, 61 percent of those who had been funded within the past five years were awarded a current grant, whereas only 41 percent of those who had not received funds from the NSF in the past five years were awarded a current grant. Clearly a good funding record gives rise to a slight competitive advantage.

We also examined the independent effect of an applicant's current academic department on the probability of his being awarded an NSF grant. Here the story was somewhat different. The rank of a scientist's current department apparently has almost no effect on the probability of his receiving a grant independent of the peer-review ratings received by the applicant's proposal. Of the scientists in the highest-ranked departments whose proposals received comparatively low ratings 6 percent were



NO INDEPENDENT EFFECT was detectable in this similar statistical analysis, which measured the influence of an applicant's current academic department on the probability of being awarded an NSF basic-research grant. Apparently current academic affiliation does not give an applicant any competitive advantage independent of the peer-review ratings that were received by his research proposal.

awarded grants, a figure no different from that found among applicants in lower-ranked departments. In the competition for current funds, therefore, a scientist's past performance as measured by citations of his work and his recent NSF funding record does lead to a very slight accumulative advantage, but his academic affiliation does not appear to give him any advantage.

The results of our study of the operation of the peer-review system in the basic-research programs of the NSF are consistent thus far with other recent findings in the sociology of science, which suggest that the scientific enterprise is an exceedingly equitable, although highly stratified, social institution in which the individuals who produce the work that is most favorably evaluated by their colleagues receive the lion's share of the rewards. Further study of the equity of research-fund distribution will address two basic problems not yet considered. In the first phase of our study we relied on the peer-review ratings elicited by the NSF program directors as an indicator of quality and found those ratings were strongly related to the actual funding decision; now we are submitting proposals to independent review panels in order to obtain independent appraisals of their quality. Finally, having learned that peer-review ratings are strong predictors of funding decisions, we are interested in whether or not they also are good predictors of future scientific performance, and so we are studying how the ratings and recent research performance compare as predictors of future research performance.

The Controversy over Peer Review

Recent studies of the peer review system show that its critics have yet to make their case.

Thane Gustafson

One of the most striking features of federal support of fundamental science is heavy reliance on scientists outside the government and on the decentralized decision-making processes of the scientific community. Much of the overall direction of federal support for fundamental science depends on the individual decisions of working scientists about which fields to enter and which lines of investigation to pursue. These decisions are communicated to government agencies through requests for funds for project support and through the opinions of nongovernment scientists who advise the government at many different levels. This advice is principally of two sorts: evaluations of requests for project support for their scientific merit and relevance to agency goals; and expressions of opinion as to which fields and lines of investigation are especially worthy of federal support.

The outstanding example of such decentralized mechanisms in federal science policy is "peer review," used by the National Institutes of Health and the National Science Foundation. In its purest form, peer review relies on the flow of unsolicited grant applications from individual researchers ("proposal pressure") (*1*) as the funding agency's principal guide for the planning of research support, and uses committees of nongovernment scientists as the main instrument of proposal evaluation and policy assessment (*2*).

In practice, peer review procedures and patterns of proposal vary widely from one government body to another and even within a single agency. In some agencies peer review is elaborate, proposals are largely unsolicited, and the influence of advisory committees is great; in others peer review is cursory, the influence of agency staff outweighs that of outside advisers, and research proposals originate in response to requests from the agency.

The most influential advisory committees and the most highly developed system of peer review are to be found in the NIH, which funds 75 percent of the biomedical research performed in American medical schools as well as more than 40 percent of all university research. The NIH peer review mechanism has two parts. Initial review of proposals for research grants is delegated to approximately 50 study sections composed entirely of nongovernment scientists. They are organized by scientific discipline and rank grant applications ac-

---

The author is an assistant professor of government at Harvard University, Cambridge, Massachusetts 02138, and a member of the seminar on Science, Technology, and Public Policy of the John F. Kennedy School of Government, Harvard University.

cording to their scientific merit. The recommendations of the study sections are then forwarded to the pertinent NIH institute. By law, each institute must maintain an advisory council of nongovernment scientists and informed laymen. The advisory council, by working its way down the project ranking as established by the study sections, decides how far the institute's available funds can extend (thus eliminating many projects approved by the study sections). In addition, the councils may raise the priority of projects that are considered to be especially relevant to the institute's objectives. Without the prior approval of their council, institute officials cannot make research awards.

The result is a unique type of dual review which ranks projects at the first stage according to their scientific merit, and at the second stage according to their relevance to the institute's objectives as well as the nation's (the latter is reflected in the size of each institute's appropriations).

Thus, peer review, even in its purest form, is far from a device for giving a blank check to nongovernment researchers. Its interest as an administrative mechanism lies in the fact that it combines the intrinsic criteria of the scientists with the extrinsic criteria of government agencies, yet in such a way that the two types of criteria remain distinct.

In addition to two-stage review, which is unique to NIH, two further types of peer review that are widespread in the federal government are single-stage review and mail review. Single-stage review is used by certain divisions of the NSF (*3*). The initial evaluation of proposals is done by NSF program directors, who make a preliminary ranking. Although advisory panels of outside experts exist for each NSF program, their role is confined mainly to a review of cases considered borderline by the program director. The outside experts do not assign priority scores, and the final assignment of priorities is left to the program director. Outside scientists are not without influence, however; in some NSF programs the influence of the panels is greater than that of the program director (*4*).

Mail review, which is also used in NSF as well as in other federal agencies, is the form of peer review that gives the greatest role to the program director. Project proposals are sent by mail to outside reviewers around the country, but the opinions of the reviews are not binding on the program director. Even in this form of peer review, however, the influence of the program directors is limited by the fact that all large projects are subject to the approval of the National Science Board (*5*).

Peer review relies heavily on the discretion, integrity, and good judgment of working scientists and agency staff. The opinions of reviewers are confidential (*6*). Accountability is maintained (at least in principle) through the review of funding decisions by agency superiors and through periodic oversight by congressional committees.

Peer review has its critics, however. The system has undergone recurrent attacks from several sources and is currently under fire once again. A few conservative members of the House of Representatives have recently attacked the confidentiality of peer review in NSF and have questioned the integrity of its program officers. For example, Representative R. E. Bauman of Maryland denounced the peer review system in bitter terms on the floor of the House (*7*):

> I suggest that there is a need for revision of the basic system by which . . . research grants are made. They are handed out in an unregulated and secretive manner known as the "peer review system." This system allows cronies to get together and finance their pet projects, where grant application writing has become an art and where many people are not devoting themselves

to basic research needs but rather to feathering their own nests.

Another congressman, Representative John Conlan of Arizona, has charged that in some instances the peer review system has been deliberately tampered with by program officers of NSF. Specifically, Conlan stated that in two instances the comments of outside reviewers had been misrepresented by NSF program directors. Representative Conlan further objected to the secrecy which has traditionally shrouded the identity of outside reviewers and criticized the refusal of NSF officials to make their reviews public (*8*).

However, these congressmen are not the only figures in government who are skeptical about peer review. The Office of Management and Budget in 1973 issued a sharply worded critique of the peer review system in NIH (*9*). Criticism also comes from outside the government. Representatives of small liberal arts colleges have complained that the peer review system is biased in favor of the major graduate universities (*10*). Feminist groups have charged that peer review and advisory groups are heavily biased against women scientists (*11*). And, there are protests that peer review groups, in choosing new members, discriminate against younger scientists (*12*).

Despite the dark suspicions of congressional critics, we should begin by noting that integrity is not the main issue; it is most unlikely that the integrity of external advisers and agency staff will be found wanting. In NIH study sections, for example, great care is taken to avoid conflicts of interest. Grant applications of study section members are never assessed by the section to which the applicant belongs. Study section members who belong to the same institution as the applicant whose proposal is under consideration are required to absent themselves during the debate and final vote (*13*). Recent statistical studies have shown that, in NSF, applications from top-ranked universities do not receive preferential treatment in the assignment of reviewers; no correlation exists between the rank of an applicant's department and that of the reviewer's (*14*).

What is at issue depends on the position of the critic. One can discern three principal preoccupations.

The first concerns the management of the peer review system, especially its integrity and its accountability to political authority. The second is directed at the principle which governs the choice of research proposals and of advisors—whether it should be that of merit, equity, or relevance to national goals. The third preoccupation focuses on the functions and limits of peer review. Under this category come questions as the extent to which new techniques of programming and evaluation, such as benefit-cost analysis, should be substituted for peer review and proposal pressure as policy-making instruments, and whether peer review should serve chiefly as a one-way signaling mechanism from the scientific community to the funding agencies, or as a two-way channel that includes a feedback link for the communication of agency priorities to the scientific community.

These three concerns—over management, principles, and functions—are present simultaneously in the three most prominent criticisms of the peer review mechanism. They are: (i) that there is favoritism and discrimination in the selection of advisory committee members and in their behavior; (ii) that agency staffs play excessive or improper roles in the selection of research projects; and (iii) that the peer review system is not the most reliable way to coordinate federal funding of basic research with the political goals of the government. My principal purpose is to review the evidence on these issues and to determine, where possible, their impact on the progress of fundamental research.

Bias in Structure and Behavior of Science Advisory Committees

According to critics, members of external advisory committees are predominantly white, male, and more than 35 years of age, they are drawn disproportionately from prestige graduate universities, they discriminate against younger researchers and untried approaches, and they perpetuate these biases by their long terms of service and by their tendency to maintain an entrenched "old boy network" by nominating their personal acquaintances to succeed them.

Most of the critics share the premise that the selection of committee members and of research projects should be based primarily on professional achievement and scientific merit according to universalistic standards. Their principal concern is that biases in representation will lead to biases in evaluation of the "state of science," that is, of the intellectual opportunities facing the scientific community. Some critics, however, feel that to be representative in their advice, panel members must be representative in their socioeconomic characteristics. This is a popular viewpoint among reformers (*15*). However, it assumes that social characteristics or institutional affiliation influence the panel members' scientific judgment; and, as we shall see, this assumption is not easy to substantiate in the case of science advisory bodies for fundamental science.

Other critics are less concerned about scientific merit than about equity, defined as equality of funding among regions or as adequate representation for minority groups or less prestigious institutions. This is a more fundamental challenge, for it calls into question the concept of universalistic professional standards on which the peer review system rests.

The Evidence on Representativeness

Membership on scientific advisory groups in the federal government is publicly available in most cases; the names, occupations, and addresses of nearly all the 20,000-odd individuals who advise the government in formal committees are published annually, as required by the Federal Advisory Committee Act of 1973 (*16*). In addition, the composition of NIH and NSF committees has long been made available in annual reports. Recently a major gap has been filled as a result of the decision of the National Science Board to make public the names of the approximately 35,000 reviewers throughout the country who advise NSF on project selection (*17*).

Unfortunately, this abundance of information has not yet been used for systematic studies of the characteristics of scientists serving on peer review committees. Published studies of other types of scientific advisers, if used with caution, can be used to gain an approximate idea of the characteristics of peer review groups. Since peer review groups tend to be more prestigious than most advisory committees but somewhat less so than policy-making advisory bodies, it seems reasonable to speculate that the characteristics of their members may fall somewhere between those of the two types of bodies.

The most thorough analysis now available of the social and professional characteristics of scientific advisers is contained in a study of the National Research Council (NRC) (*12*). The study reports that NRC advisers are an average of 10 years older than the population of scientific doctorate holders in the United States. While 50 percent of the advisers studied were more than 50 years old, only 3 percent were less than 35 (*12*, p. 53). The situation was similar in another agency investigated in the study; for the five senior advisory bodies of the Department of Defense the median age was 50 years (*12*, p. 54). The median age of NSF advisers in 1970 was also in the same range (47 years) (*18*).

National Research Council advisers were predominantly male. Whereas 7 percent of the holders of scientific doctorates in the United States in 1969 were women, 1 percent of the NRC advisers were female. Subsequent studies have shown that similar ratios prevail in NIH; in 1972 women accounted for 2 percent of the advisory body membership (*11*). This pattern is not universal, however: female scientists in NSF make up nearly 8 percent of the advisory group panels (*19*).

Certain regions were found to be better represented in the NRC than others, although the pattern varied with the importance of the advisory committee involved. For the NRC system as a whole, the South Atlantic region was the best represented (*12*). But on 35 policy-related committees the New England, Pacific, and Mid-Atlantic regions were dominant, while the South Atlantic trailed behind. The latter pattern is also characteristic of NSF panels; the New England, Pacific, and Mid-Atlantic states together, while employing about 50 percent of the nation's doctoral scientists and engineers (*20*), account for nearly two-thirds of NSF advisers (*19*). As for NSF reviewers, data for fiscal year 1974 show that California, Massachusetts, Maryland, and Washington, D.C., are represented considerably more than proportionately in relation to their populations of scientists (*14*).

Another charge is that advisory panels are dominated by representatives from prestige universities. Statistics for NSF and NIH do confirm that the top graduate institutions are heavily represented. An American Chemical Society (ACS) study of 11 NIH study sections in biochemistry showed that of 113 institutions represented, 32 contributed more than two-thirds of the advisers, and the eight institutions most heavily represented supplied nearly one-quarter of the advisers (*21*). In NSF the picture is similar: the 18 top institutions, which employed 9 percent of the nation's academic doctorate-holding scientists in 1971, contributed more than a third of the advisers (*22*).

In contrast, there is little evidence that the science advisory apparatus consists of a small number of individuals who spend long years on one committee or rotate from one committee to another. The ACS study of NIH biochemistry study sections found that only seven out of 366 advisers from 1964 to 1973 served twice. This appears to be in part the result of deliberate policy, for NIH's practice is to make no immediate reappointments to its study sections. Similarly, a study of science advisers to the U.S. Public Health Service (which included NIH study sections at the time of the study) showed that over the period from 1957 to 1964 fewer than 15 percent of the advisers served two full terms and fewer than 20 percent served more than one full term. The median seniority of Public Health Service advisers was a little more than 1 year during the period covered by the study (*23*).

If periods of service are so short, then recruitment must be very large. The study of the Public Health Service concluded that the intake of its panels would eventually include one out of every two active biomedical researchers in the United States. The ACS study of NIH biochemistry panels, which used a different approach, came to a similar conclusion.

In contrast to the rank and file of science advisers, there is evidence that the consistently influential advisers that serve the most important committees come from a smaller pool of individuals and rise in the science advisory hierarchy over a period of several years (*23*). Nevertheless, the dominant picture is one of great mobility among science advisers, which is consistent both with the extraordinary fluidity of the active research population in the American scientific community and with the high turnover rates in the professional staffs of science-funding agencies.

To sum up the evidence I have reviewed so far, the available studies confirm that the membership of science advisory committees does not strictly reflect the composition of the doctorate-holding scientific community, at least in some overall social and institutional features. The next question is what to make of this point.

First, there may be more appropriate criteria by which to judge representativeness. For example, although the prestige universities may appear to be overrepresented in comparison to the number of Ph.D.'s they employ, they seem underrepresented when one looks at the number of Ph.D.'s they train since the top 20 doctorate-granting institutions account for more than two-thirds of the output of doctorate-holding scientists.

Defenders of the peer review system assert that the selection of peer reviewers mirrors scholarly achievement. There is some evidence for this; according to a recent statement by Deputy Director of NSF Richard Atkinson, the distribution of NSF reviewers by state approximates the geographical distribution of scholarly publications. By that measure, the only overrepresented states are New York and the District of Columbia, and in the latter case the explanation is that many NSF proposals are reviewed by officials of other federal agencies, so as to ensure coordination (*14*, p. 11, figure 3).

A better indicator of merit would be the impact of research publications on the scientific community. Sociologist Hagstrom of the University of Wisconsin, by counting the frequency with which the publications of scientists from prestige institutions are cited, has concluded that the impact of those institutions is roughly in line with their representation on science advisory committees in Washington (*24*).

Most important, the representativeness of committee membership is significant only if we accept the premise of the advocates of representative reviewship that skewed membership leads to a skewed pattern of awards. I now turn to the evidence on distribution of research awards among institutions and among individuals in order to determine whether this is true.

Patterns of Awards

Raw data on patterns of funding indicate a strong advantage for prestige institutions. In NIH, according to a recent memorandum by the Office of Management and Budget, applicants from ten institutions accounted for 46 percent of all grant funds in 1971, and their share then was greater than it had been 5 years before (*9*, p. 9). Similar patterns prevail in NSF. In 1974, research grant awards to the top 20 institutions represented one-third of total NSF obligations for all programs (*24*, p. 15). In the field of chemistry alone, according to an in-house study, the top 20 departments, employing 18 percent of the country's graduate chemistry faculty, received 49 percent of the NSF chemistry awards in 1973, an increase of 5 percent over 1972 figures (*25*). Moreover, the success rate of the top 20 departments was 53 percent, compared to 30 percent for all other departments (*26*).

However, the issue of concentration in funding is more complicated than it appears at first glance. For example, concentration does not always work to the advantage of the prestige institutions. A study of the allocation of NSF project grants in metallurgy and materials research shows that they are clustered among the middle-ranked institutions rather than the top-ranked ones (*27, 28*). In addition, the more significant pattern of concentration may not be among institutions, but within them. Several studies have found that when one takes into account the size of each institution, and particularly the number of faculty members capable of performing or supervising graduate-level research, the funding distribution among institutions becomes more nearly uniform. Thus, on a per capita basis, most of the inequality is among faculty within a single department. Whether the department is rated high or low, approximately 70 percent of the outside funding is received by the more productive half of the faculty, the half which receives more than 90 percent of the citations in the scientific literature (*29*).

A related issue is the extent to which concentration increases in times of budgetary crisis. Critics complain that in hard times the representatives of prestige institutions act to protect their own. Data from NIH provide some evidence on this point: from 1967 to 1970, when the purchasing power of NIH funding declined sharply, the number of institutions funded declined from 330 to 277 (*30*). However, during a similar period (1966 to 1972) the total pool of NIH grant recipients declined much less—from 10,250 to 8,150 (*31*). The implication is that the concentration of NIH funding in the prestige institutions, even when reckoned on a per capita basis, increased during this period, a point which receives confirmation from Office of Management and Budget data (*9*). Since the early 1970's the number of institutions funded by NIH has expanded again, although much of the increase is due to the new prosperity of the National Cancer Institute (NCI). In 1974 alone the NCI allocated its funds among 367 institutions (*32*).

In times of reduced funding, how new proposals and younger researchers fare in comparison with scientists applying for renewals is of interest. At first glance, hard times seem harder for the researcher seeking his first grant. From 1967 to 1970 the share of NIH research grants awarded to new projects declined from 23 percent to 16 percent (*33*). Since that time the downward trend in the percentage of NIH grants awarded to new applicants has continued for all NIH institutes except the NCI and the National Heart and Lung Institute (*34*). Particularly in the NCI the increased flow of money since 1971 has had a dramatically different effect. Despite the enormous influx of new-project applications (proposals to NCI now account for roughly half of all applications to NIH), the success rate of new principal investigators (that is, the ratio of awards to applications) has risen from 30 percent in 1970 to 54 percent in 1974 (*34*, p. 479).

Even in bad times, however, younger researchers among the new applicants have not been neglected. For NIH as a whole in 1971, applications of young scientists (age 36 and less) for traditional project grants were approved (that is, recommended by the study sections) at a higher rate than those of older scientists (*34*, p. 48). Funding data for 1972 and 1973 for NCI give the same picture (*31*, pp. 240 and 241).

> Of those investigators 35 years of age and under applying for research support, 52% were funded . . . in fiscal year 1972, and 35% were funded . . . in fiscal year 1973. Of those applicants older than 35, only 32% were funded . . . in 1972 and 25% . . . in 1973.

A similar pattern has been observed in internal studies of NSF funding of younger investigators (*35*).

Once again, we must question the meaning of these data and then ask whether the observed pattern of awards is unfair or inefficient. For those who value equity over scientific merit, the existing pattern is discriminatory. However, for those whose principal concern is scientific merit, the key question is whether the existing distribution of awards is out of line with the distribution of scientific quality in the country's research institutions. In order to find the answer some reasonable indicator of the quality of research is required (*36*).

One possibility is the response of the scientific community itself to the present pattern of awards, for presumably researchers are good judges of whether NSF and NIH are funding the best science. Until recently most researchers gave the appearance of being satisfied. But this was primarily because, until the late 1960's, most worthy proposals could find support from one federal agency or another. Unsuccessful applicants usually recast their proposals in response to reviewers' comments and tried again. In NSF, for example, the number of rejections was never so great as to warrant a separate review and appeals procedure. In recent years, however, funding for fundamental research has dropped and the number of funding sources has declined. More and more front-rank scientists find themselves the unhappy recipients of "declination" notices from NIH and NSF. The two agencies are now faced with mounting protests from these disgruntled applicants. Consequently, in order to justify the pattern of funding produced by the peer review system, more objective indicators are necessary.

One measure that is attracting increasing attention is the rate at which scientific publications stemming from NSF or NIH grants are cited in the scholarly journals. Although subject to certain distortions, the citation rate does provide a rough indicator of the impact of a scientific work within the scientific community. This makes it possible to cross-check the judgment of peer review groups by comparing it to the reaction of the scientific community as a whole. A study by the Rand Corporation is enlightening (*37*). By surveying the 15-percentile fraction of NIH grants that produced the most heavily cited publications, the study showed that their greater merit was recognized from the beginning by NIH study sections, which awarded that fraction a significantly better priority score than they did to the remaining group. Figures on applications for renewal were also revealing: the most frequently cited researchers received a much higher priority score in applications for renewal than they had in their initial applications. These findings appear to indicate that peer review groups have a good capacity for recognizing potential in a research proposal, and for recognizing outstanding achievement when they see it (*38*).

The Office of Management and Budget's charge that NIH study sections deliberately approve more applications than can be funded in order to make a case for higher appropriations for the agency (*9*) can also be illuminated by these points. Their claim appears to be weakened by the fact that the average priority score awarded to new grant proposals by NIH study sections has remained much the same since 1967 (*37*). This suggests that the study sections are doing much as they have always done; what has changed is the amount of funding available to cover the proposals they approve.

The link between citation rates and peer review scores also provides a means for judging the claim that peer review groups award too much money to prestige institutions. For example, an internal study of NSF awards in chemistry shows a striking correlation between the distribution of citation rates and the allocation of NSF funds. Of NSF awards in chemistry, 80 to 85 percent go to departments whose faculty averaged more than 60 citations per author during the 5-year period ending in 1972; the top four or five departments, which received the lion's share of NSF grants, averaged around 400 citations per author (*39*). If one accepts the premise that citation rates reflect the quality and impact of research results rather than simply the visibility of their authors, then the charges of bias leveled at peer review groups lose much of their force.

On balance, however, these conclusions are still tentative. There has been too little research into the impact of the peer review system on funding patterns and on the quality of the resulting research. At this writing, several major studies are under way. It seems certain that, in view of the growing popularity of citation analysis and the controversy that surrounds the peer review system, the questions addressed in this article will soon be much better understood (*24*).

Up to this point we have concentrated exclusively on the use of outside scientists in the peer review system. However, in many programs and agencies professional staff officers play an important or even dominant role. Let us now examine their influence in peer review.

Agency Staffs as a Force in Peer Review

Program officers of funding agencies such as NIH and NSF play an important role in the evaluation of proposals. In several important respects the program officers are themselves the peers of the applicants for research funds: they are professional scientists with a background in research; they are often not career employees of their agencies; and their length of service is usually short (*40*).

However, program officers in many cases (the executive secretaries of NIH are an exception) are expected to further the program objectives of their agencies, and these do not necessarily coincide with those of the scientific community. Consequently, the program director may feel torn by competing criteria and he may find himself at odds with the external advisory committee attached to his program.

However, the potential for conflict varies, because the role of the program manager differs from agency to agency and from program to program. In comparison with that in NIH, the peer review process in most other agencies is less formal and sharp, and the influence of agency staff is generally greater.

The question of how much influence agency staff members have in the process of peer review is also relevant. Even where the influence of outside scientists is greater, as in NIH, agency staffers such as executive secretaries of study sections play an important, if discreet, role. In NIH, the staff of the Division of Research Grants has the power to rearrange study sections (*2*, p. 26). The executive secretary appoints the study section chairman and together they recommend replacements for panel members (*41*). When applications first reach the NIH Division of Research Grants, it is an executive secretary, temporarily acting as referral officer, who decides to which institute to assign the application (*42*)—a crucial decision since institutes differ widely in the number of approved projects they can fund. Executive secretaries in NIH decide which study section members shall have primary responsibility for reviewing a given proposal; in NSF the program director selects the reviewers for each proposal. During the processing of applications, the program officers of both agencies are the principal contacts between reviewers and applicants (*43*). In NIH the executive secretary acts as the link between the study section and the next stage of review by interpreting the decisions of the study sections to the advisory council of the institute. In NIH advisory councils it is the professional program manager who has the responsibility for recommending departures from the priority ranking established by the study sections whenever he deems a proposal especially relevant to the institute's mission (*2*, p. 28). Finally, the program directors of both NIH and NSF have final authority in funding decisions, although they will only very exceptionally make awards to proposals that have been disapproved by reviewers or panels.

Thus, even in the programs in which external peer review panels have the greatest sway, there appears to be the opportunity for the agency staff to influence the process of proposal evaluation by shaping the agenda, channeling the flow of information to and from the outside advisers, or actually altering or overriding their decisions. In NIH this influence is discreet and informal while in NSF it is usually more important than that of the external advisers. In both agencies the importance of the professional staff appears to be growing.

Of late, program officers find themselves increasingly involved in controversy. There are several reasons for this: funds are short and researchers are anxious; agency leaders are under pressure to pursue a wide array of social objectives; reformers call for openness in administrative procedures and for direct accountability to the public; and in mistrustful times the good faith of all administrators is under suspicion. As the chief points of contact between scientists and agency leaders, the program officers are caught in the middle.

Recent congressional attacks on program officers have focused mainly on their integrity. For example, Representative Conlan has charged (*8*, p. 4):

> It is common knowledge in the scientific community that NSF program managers can get whatever answer they want out of the peer review system to justify their decision to reject or fund a particular proposal. Since program managers soon learn, like college students, which professor is good for an easy "A" and which can be counted on for an almost certain "C" or "D," it's no trick to rig the system.

Such charges are extreme. The real issue, just as in the case of the external advisors, is not the program officers' integrity, but rather their accountability. According to the congressional critics, NSF section heads and division chiefs do not have time to give more than a cursory look at program officers' recommendation (*8*, p. 7), and reviewers' reports are not made public; therefore, in this view there is no effective mechanism to check up on the decisions of program officers. The issue of accountability arises also in NIH, since several institutes have shifted their emphasis from grants (which require the traditional two-stage peer review) to contracts, in which the major decisions are made by staff officers (*44*).

Critics in Congress propose to remedy the problem of accountability both by making reviewers' names and comments public and by making information available to Congress about individual research projects in advance of final approval (*45*).

Such remedies would make a major departure from the accepted administrative doctrine, which has evolved out of long experience in dealing with technical programs in government. Three points are central to the doctrine: (i) in the assessment of scientific merit, a certain degree of confidentiality is required in order to encourage candor on the part of reviewers and to guard against the danger of plagiarism; (ii) congressional oversight is best conducted after the fact, not before, and should be devoted to assessment of overall policy rather than detailed review of individual projects; and (iii) accountability is best achieved by focusing responsibility on visible and answerable superiors, not on their subordinates. The measures recently proposed in Congress would discourage candor or even participation by outside reviewers; they would involve congressmen directly in "legislative service" in support of scientists from their districts; and they would place the political spotlight directly on the program officers rather than on their policy-making superiors.

These dangers were clearly present in the minds of several members of the House Subcommittee on Science, Research, and Technology at congressional hearings on peer review this summer.

If the Conlan-Bauman proposals are not adopted—and it seems unlikely that they will be—the question is raised as to what reforms of the peer review system are needed. To answer this question it is necessary to look carefully at the purposes of the peer review system and its inherent limitations.

Purposes and Limitations of Peer Review

Peer review and proposal pressure are signaling mechanisms for providing funding agencies with information about the possibilities of science. The decentralized systems of rewards and communications of the scientific community supply external advisory panels with the necessary knowledge and cues for appraising proposals and recommending new policies. Most important, the peer review system is based on the assumption that the flow of proposals to funding agencies will provide a reasonably accurate sampling of the universe of research opportunities arising in all fields of science. The objective of any reform should be to safeguard the accuracy of this signaling device.

Recent developments in the support of fundamental science may make this objective difficult to attain. The bulk of funding for basic research is now concentrated in four federal agencies, all of which are under growing pressure to promote "targeted" research programs. Such concentration creates new networks of communications and rewards alongside the traditional ones, altering the "market" mechanisms of the scientific community. There is the danger that proposal pressure, filtered through peer review, will no longer provide a reliable guide to the best science. An illustration of this problem is the remarkable flood of proposals to the NCI since 1971, or the recent surge of energy-related applications to NSF. In both cases researchers appear to be responding to the news of increased funding. The question is whether their response really matches the actual distribution of intellectual opportunities in science.

Distortions in the signal generated by the proposal pressure–peer review system can arise from four sources:

1) The structure and mechanism of the peer review system itself can distort the signals reaching the funding agency from the scientific community, thus producing a false impression of the intellectual opportunities available in each field (selective channeling).

2) The application of extrinsic criteria and funding constraints by agency staff, by encouraging some directions of research and neglecting others, may sharply affect the distribution and content of proposals submitted in the next time period (indirect feedback).

3) Agency staff may influence the generation of proposals through direct publicity, requests for proposals, seminars, and other. External advisors may carry back to their departments information on new funding priorities in Washington or informal lore on which proposals will be best received (direct feedback).

4) The practice of funding by project rather than by institution may cause a pattern of over response by researchers to short-term swings in agency priorities, because of the necessity of applying for new funding at frequent intervals.

Some observers of the peer review system, particularly in government, would say that these features of the peer review system are virtues, not defects. Items 2 through 4 above stimulate a fast response by the scientific community to national needs as they are perceived in Washington. Nevertheless, the signaling function of proposal pressure and peer review is vital. Without an accurate picture of the state of science, funding agencies run the risk of pouring public money into fields whose titles are politically appealing but which are not scientifically ripe for major advances, and of neglecting others in which gains might be made, but which appear esoteric or unintelligible to the layman.

Ways of limiting distortions in the signaling function must be found. One method is to move away from the traditional project grant and make greater use of funding by institution, thus leaving project selection to local institutional management as is currently done in national laboratories and coherent area programs. Funding takes the form of subvention for the institution as a whole, or a particular research center or broad field within an institution, and leaves allocation of resources as a responsibility of the local management, to be justified only after the fact. High scientific merit is maintained through the use of outside visiting committees, overseeing corporations, or various kinds of internal peer review. Institutional funding is already widely used in programs that require an interdisciplinary, well-coordinated approach.

An institutional approach helps to reduce several of the sources of distortion discussed above. It reduces the risks bearing on the individual researcher. When project grants are the sole source of research funds, the funding of a particular proposal nearly becomes a life and death decision for the career of a scientist or the survival of a department. This exacerbates the sensitivity of researchers to news of changing priorities in Washington (*46*). In addition, funding by institution clearly separates decisions on the political relevance of a field of research from those on the scientific merit of individual proposals.

However, many of the institutional funding mechanisms currently in use have been criticized by the scientific community. Many scientists argue that in national centers and area programs scientific merit is not as carefully scrutinized as in universities. An example is the National Center for Atmospheric Research (NCAR), which is funded as a unit by NSF. Until 1973 NCAR scientists were given wide latitude in their selection and execution of research proposals. Peer review took the form of general supervision by a University Consortium. According to a recent evaluation of NCAR, however, the results of this approach were disappointing (*47*). Scientists at the NCAR had few publications to their credit in proportion to the support provided to them; their research was not interdisciplinary as had been advertised in justifications of the NCAR approach; and the work and the quality of its personnel were not as highly regarded among atmospheric scientists as should have been expected, given the amount of funding and of logistic support. According to critics, one of the reasons for the undistinguished performance of the NCAR was a lack of close supervision by the managing consortium.

Another example of the mixed results of funding by institution is the center-grant program of NIH. Research funds in such centers are allocated at the local level. However, critics charge that this system produces "peer reviews at home rather than peer review at a distance," which carries the potential for conflicts of interest and personality (*13*). Similar problems plagued the NCI in the management of its Cancer Chemotherapy National Service Center in the early 1960's (*42*, p. 206).

Thus, while institutional funding may be the answer for certain large and closely coordinated programs, it will not necessarily enable funding agencies to obtain a reliable sampling of the best research opportunities. For most types of fundamental research the traditional project grant, selected by peer review, with overall priority among fields and subfields determined at least in part by proposal pressure, appears to provide the best available guarantee of scientific merit and accurate information. It is important, however, to extend existing safeguards: to choose advisors and agency staff who are representative of the best science; to limit their terms of service; to separate as much as possible the evaluation of scientific merit from that of funding so as to reduce the dependence of researchers on the priorities or biases of any one agency or congressional committee; and finally, to subject the entire system to periodic review and criticism. Given the present—admittedly tentative—state of our knowledge about the impact of the traditional peer review system, there is not a convincing case that the system's defects warrant the risk of sacrificing its virtues.

References and Notes

1. Most project proposals come from an individual "principal investigator," who represents a small group of junior colleagues and collaborators, including graduate students. The advantage of this system is that it focuses scientific accountability: the "principal investigator" serves as the channel of responsibility between the project team and the sponsor.
2. J. G. Wirt, A. J. Lieberman, R. E. Levien, *R & D Management: Methods Used by Federal Agencies* (Rand Corporation R-1156-HEW, January 1974).
3. For background information on the various types of proposal review, see Wirt *et al* (*2*). Specific details on the roles of program directors, advisory panels, and outside reviewers in each NSF program may be found in *Peer Review and Proposal Evaluation: Staff Study* (National Science Foundation, Washington, D.C., June 1975).
4. An interesting question for future investigation is the mechanism of the distribution of power between the program director and his advisory panel. In some cases, it may be a matter of tradition; in other cases, power is wrested from the advisory panels by the section head. For example, before 1966 the advisory panel for chemistry is said to have played a very influential part in NSF chemistry awards. With the arrival of a new section head, however, the panel's powers were sharply reduced. It is interesting to speculate, incidentally, on the connection this fact may have with the remarkable subsequent stability of the chemistry section staff. Turnover among program directors could conceivably be inversely related to the power they enjoy.
5. Projects involving more than $500,000 during the first year or more than $1,000,000 over the lifetime of the project must be approved by the National Science Board (NSB). However, the NSB rarely rejects a basic research project after it has received a favorable recommendation from the NSF staff; such rejections, when they occur, are based on considerations of policy, not of scientific merit. By contrast, the NSB has never succeeded in reinstating any project rejected by the top level staff of NSF, even though the project may have been energetically recommended by the program officers at the disciplinary level.
6. Paraphrases of the opinions of reviewers may be made available to applicants upon request. The NSB is considering whether to release the names of reviewers under some circumstances.
7. *Congressional Record*, 24 June 1975, p. H6015.
8. Statement by Representative John B. Conlan (R-Ariz.) before the Subcommittee on Science, Research, and Technology of the U.S. House Committee on Science and Technology, 22 July 1975.
9. Office of Management and Budget, *Drug Research Reports*, 16 (No. 22), 4 (1973).
10. See, for example, the statement by J. L. Powell, Provost of Oberlin College, before the Subcommittee on Science, Research, and Technology of the House Committee on Science and Technology, 24 July 1975.
11. D. Shapley, *Science* 175, 1090 (1972).
12. Charges such as these led to the formation by the National Academy of Sciences of a Committee on the Utilization of Young Scientists and Engineers in Advisory Services to Government. See the report of the Committee, *The Science Committee* (National Academy of Sciences, Washington, D.C., 1972).
13. N. Wade, *Science* 179, 158 (1973).
14. Statement by R. C. Atkinson, Deputy Director, NSF, before the Subcommittee on Science, Research, and Technology of the House Committee on Science and Technology, 23 July 1975.
15. For a review of the issue of representative bureaucracy, see C. Mosher, *Democracy and the Public Service* (Oxford Univ. Press, New York, 1968); K. J. Meir, *Am. Political Sci. Rev.* 69, 526 (1975).
16. Public Law 92-463. A crucial exception to the coverage of the act is the many advisory committees of the National Academy of Sciences-National Research Council, which account for nearly half of the total number of advisory committees in the federal government.
17. *Science & Government Report* 5 (No. 13), 8 (1975).
18. The median age of NSF advisers has undergone some interesting changes over the last 20 years. From 51 years in 1954, it dropped to 44 in 1966, and rose again to 47 years in 1970. (These data are drawn from a forthcoming study by N. Mullins *et al.* at Indiana University. I thank Professor Mullins for making some of his findings available to me.)
19. National Science Foundation, *NSF Management Statistics* (prepared by the NSF Administration Directorate, July 1975), p. 19. It should be observed that the apparent sex bias is no doubt a reflection of the age bias. That the percentage of Ph.D.'s awarded to women has been increasing in scientists would reflect the sex discrimination of graduate education 20 or 30 years ago.
20. NSF Administration Directorate, *An Analysis of the Geographical Distribution of NSF Awards as Compared with Other Selected Indicators* (National Science Foundation, Washington, D.C., 1975), p. 1.
21. Findings of the Public Policy Committee, Division of Biological Chemistry, American Chemical Society. I thank E. W. Westhead, professor of biochemistry at the University of Massachusetts (Amherst), for a summary of the committee's data.
22. Preliminary results of a study of the NSF advisory system by N. C. Mullins, *Social Stud. Sci.* (1 July 1975).
23. N. C. Mullins, *Sci. Stud.* 2, 7 (1972).
24. Hagstrom's work is described in N. Wade, *Science* 188, 429 (1975). It is conceivable, however, that this finding would not hold for peer review panels; since the latter are highly prestigious, they may draw more than proportionately from prestige institutions.
25. Data presented by NSF to the Committee on Science and Public Policy of the National Academy of Sciences, cited with permission.
26. Atkinson (*14*, pp. 13, 23). The success rate is defined as the ratio of successful applications to total applications.
27. D. Kuhlmann-Wilsdorf, University of Virginia, before the Subcommittee on Science, Research, and Technology of the House Committee on Science and Technology, Washington, D.C., 29 July 1975.
28. Kuhlmann-Wilsdorf's statement has aroused considerable controversy, which is discussed in D. Shapley, *Science* 189, 622 (1975). The explanation may be that metallurgy-materials research in the prestigious institutions is generously supported through the Materials Research Laboratory program, which is block-funded.

29. Kuhlmann-Wilsdorf (*27*, p. 607). Similar conclusions were reached by the Westheimer Panel in the mid-1960's. See National Academy of Sciences–National Research Council, Committee for the Survey of Chemistry, *Chemistry: Opportunities and Needs* (National Academy of Sciences, Washington, D.C., 1965).
30. T. J. Kennedy, Jr., J. F. Sherman, R. W. Lamont-Havers.
31. C. D. Douglass and J. C. James, *Science* **181**, 241 (1973).
32. F. J. Rauscher, Jr., *ibid.* **189**, 115 (1975).
33. Kennedy *et al.* (*30*, p. 604). In NSF the percentage of funds awarded to new proposals has averaged about 10 percent in recent years. NSF memorandum, *Selection of Physics Research Proposals for NSF Support*.
34. Data drawn from J. T. Kalberer, Jr., *Cancer Res.* **35**, 473 (1975).
35. The NSF studies were performed under C. Falk at the behest of the NSB.
36. In practice, the various categories are not as neat as I have painted them here. The NSF, for example, applies several criteria such as scientific merit primarily and geographic and disciplinary distribution secondarily. As Deputy Atkinson explained in a recent statement, NSF program officers have no set formula for weighing the merit and equity criteria; rather, when choosing between two proposals of roughly equal merit, they decide to a large extent on the basis of geography. But as Atkinson stressed, it is done largely "by intuition" (*14*).
37. G. M. Carter, *Peer-Review, Citations, and Biomedical Research Policy: NIH Grants to Medical School Faculty* (Rand Corporation, R-1583-HEW, December 1974).
38. Evidence in the same direction comes from D. Kuhlmann-Wilsdorf's study. For NSF as a whole, she found a close relation between the size of the NSF grant and the rate at which the applicants' overall work was cited (*27*, p. 7).
39. NSF report to National Academy of Sciences, Committee on Science and Public Policy, 1972. Cited with permission of K. Wilson.
40. To document this point it is instructive to compare the rosters of program officers listed in NSF's Annual Report for 1969 and 1974. Although reorganizations and expansions of disciplinary categories occasionally make comparison difficult, the overall picture that emerges is one of remarkable movement. Sections such as physics, atmospheric sciences, and oceanography underwent almost complete turnover. In fact, the only major research section for which the program officers in 1974 were the same as in 1969 was chemistry.
41. Wirt *et al.* (*2*, p. 60). On the other hand, in NSF all external advisors are appointed by the director.
42. U.S. Office of Science and Technology, *Biomedical Science and its Administration: A Study of the National Institutes of Health* (Government Printing Office, Washington, D.C., 1965), p. 193.
43. G. N. Eaves, *Fed. Proc.* **31** (No. 1), 3 (1972).
44. The NCI, which was the leader in this trend, is beginning to reverse itself. Some programs that were funded by contracts will be funded via special cancer research emphasis grants.
45. Representative Conlan has introduced legislation (HR 9892) that would establish a "peer review office" in NSF, which would maintain a detailed record of applications, reviews, and final actions. See J. Walsh, *Science* **190**, 253 (1975).
46. For interesting illustrations, see G. N. Eaves, *Fed. Proc.* **32** (5), 1541 (1973).
47. N. Wade, *Science* **182**, 36 (1973).
48. This article is based on a survey of American policy on fundamental science conducted for the Joint Working Group on Science Policy under the U.S.-U.S.S.R. Agreement on Cooperation in Science and Technology. A similar survey of Soviet science policy is being conducted simultaneously by the U.S.S.R. Academy of Sciences. I thank H. Brooks, D. K. Price, and W. W. Lowrance for advice and comments.

● *ABSTRACT*

The controversy over peer review is viewed as a dialectic. The arguments espoused by advocates and critics of the system wherein research proposals are evaluated by advisors to funding agencies are reviewed, particularly the findings of two recent studies of peer review at the National Science Foundation. These findings seem to establish merit as the primary factor in the recommendations of peer reviewers to fund proposals. The findings also beg several questions as to 'acceptable' definitions of meritoriousness and innovativeness, the links among belief, perception, and evaluation, and the sanctioned operation of particularistic factors in the review process. Future studies, it is suggested, must include psychological variables — especially measurement of applicants' and reviewers' 'cognitive styles' — if data are to narrow gaps in knowledge and inform the debate itself. Finally, three models which undergird views of peer review are discussed and related to key social issues in the debate.

Peer Review at the NSF:
A Dialectical Policy Analysis

Ian I. Mitroff and
Daryl E. Chubin

In recent years, scientific controversies have, with growing regularity, attracted public scrutiny and debate. The controversy over the nature and functioning of the peer review system is an outstanding case in point. That this controversy strikes to the heart of science's most sacred and cherished values — institutional and political autonomy vis-a-vis the external society — may account for the intensity of the debate.[1] That the debate did not reverberate through the American scientific community at large until 1975, however, suggests two complementary realities that may have forestalled the definition of peer review as a pressing and researchable problem: (1) the sanguinity of scientists during the halcyon years of growth in federal funding for R&D and graduate training;[2] and (2) the tenacity of certain values which undergird the

---

Social Studies of Science (SAGE, London and Beverly Hills), Vol. 9 (1979), 199-232

very institution of science, precluding systematic investigation of mechanisms by which the institution's autonomy, self-governance, and 'uneasy partnership' with government is maintained.[3]

As sanguinity and tenacity have flagged, a more defensive posture has emerged, resulting in empirical investigation of contemporary peer review. The results of such investigation occasion this paper, the purpose of which is fourfold:

1. to outline the nature of the debate: that is, to present systematically the position of the contending parties;
2. to review critically some of the evidence, particularly that emanating from two studies of peer review at the National Science Foundation, which bears upon the debate;
3. to raise issues not addressed in recent studies, but which bear fundamentally on the debate; and
4. to propose a strategy for future studies that will clarify old and new issues and hasten collection of appropriate data for informing, if not resolving, the debate — a debate which centres on peer review as an evaluative mechanism in the execution of science policy.

Before proceeding, two caveats must be sounded, lest we be misconstrued. Firstly, notwithstanding our commentary, we are neither anti-science not anti-peer review. Indeed, we regard peer review in principle as the best available system; this does not mean that the system in practice cannot be improved. Likewise, and one would think with greater ease, studies of peer review *can* be improved, not merely in terms of measurement and modes of analysis, but in approach. If scientists refuse to be reflexive, sceptical, and probing of their own institution — its organization and management — then can they really decry congressional 'incursions' into their policies and practices? Part of the responsibility of autonomy and self-governance is self-scrutiny. This is a reasonable expectation, yet too few dissenting voices within the scientific community tend to be heard.[4]

The second caveat is closely related to our first, but pertains to the scope of this paper. We regard peer review as a kind of science advice involving select members of the community, all of whom act — to a greater or lesser degree — as gatekeepers. These gatekeepers help to regulate the flows of information and fiscal resources through the community by directing, impeding, and expediting flows based upon judgments of quality and merit, allegiances and biases and, probably, on sheer caprice as well.[5] The point is that science advising entails the disposition of scholarly work (such as grant proposals and manuscripts) by referees representing, but not representative of, the scientific community.[6] It is the linking of advice with ultimate dispositions (that is, decisions) which endows peer review with a distinctive content. Clearly, we seek to generalize in this paper about content, recognizing that the form of peer review varies. For example, whereas the National Institutes of Health use a system of study sections, NSF uses review panels.[7] Whereas some referees act as ad hoc mail reviewers, others attend periodic meetings as panel members. But what counts is that multiple judgments are solicited and weighed differentially (depending on the source) to reach a decision: to fund or not to fund. What we shall argue is that defensible decisions are not inevitably guaranteed by the peer review mechanism. Indeed, the process can be used to justify any decision. The power vested in the mechanism or process is derived, in large part, from the power (for instance, reputation) of the referee-advisor-gatekeeper, and is rationalized by the system.[8] To reiterate, it is to this content we shall generalize, though our data are of a more limited form. The philosophy underlying the mechanism of peer review (at least in the US) warrants such substantive generalization.

The Debate:
A Dialectical Statement of the Issues

The mode of presenting the peer review debate can help to elucidate the substance of the debate itself. Insofar as a debate features arguments sampled from a continuum of opinion, those arguments can be presented in the form of a dialectic.[9] However, since this way of posing policy issues may not be as familiar as other forms, it might be helpful to offer at least a brief exposition of dialectical analysis.

In recent years scholars in those diverse fields now subsumed under the 'social studies of science' rubric — particularly in history, philosophy, sociology, and management science — have advanced a series of potent theoretical arguments for a dialectical treatment of policy issues: indeed, they have argued that the conduct of science is dialectical in its basic structure.[10] The essence of the argument is as follows:

(1) most social issues, and for that matter, topics on the leading edge of the sciences (natural as well as social) are conflictual in nature: that is to say, it is difficult, if not impossible, to secure widespread agreement (at least initially) as to their basic definition, let alone their solution;

(2) the failure to secure agreement is not because such issues inherently defy treatment or analysis, but because various parties, due to their respective social, intellectual, and/or value positions, will perceive the same issue in very different ways: in a word, parties at interest bring fundamentally different background assumptions to the same issue; as a result, they tend to develop various interpretations of the same set of data (observations or 'facts');

(3) by themselves, data or facts may not be sufficient to resolve the dispute between contending parties, but may actually serve to intensify it;[11] therefore, rather than presume and depend upon initial agreement between parties, what is required is a method for identifying the disparate assumptions that parties bring to an issue and its debate.

Table 1 is a dialectical representation[12] of the views of the proponents (pro) and the critics (con) toward the peer review system, as currently used by NSF. A careful reading of the report by the US House of Representatives on National Science Foundation Peer Review,[13] plus related documents by the proponents and critics of the present system,[14] clearly reveals the operation of two distinct sets of assumptions about peer review. This means that in Table 1 *for every assumption or contention we have identified as characteristic of the position of one side, we have identified a counter-assumption which is characteristic of the other side. Not only are the assumptions on each side strongly held by their proponents, but they are maximally opposed as well. For each assumption which is characteristic of the one side, there is an equally strong assumption on the other such that the two assumptions are the diametric (or nearly diametric) opposite of one another.*[15]

This characteristic procedure is largely responsible for making the dialectic a distinctive means of conducting policy analysis.[16] By aligning the positions side by side, the method explicitly contrasts and draws out the implications of each. It not only shows what each position affirms (that is, what it alone entails) but it also shows explicitly that to which it is maximally opposed. Clearly, no position, no matter how internally consistent and comprehensive it is, is ever completely self-contained. As a result, no position can be

TABLE 1
A Dialectical Representation of the Current System of NSF Peer Review

Basic Assumptions and/or Contentions	
PRO the Current System	CON the Current System
1. The current system is open; it is free from substantial bias.	1. The current system is closed; it contains substantial bias ('an incestuous buddy system')
2. The system leans over backwards in favour of the maverick.	2. There is a natural bias against revolutionary and innovative ideas.
3. It is possible for programme managers to manipulate the system to get the review they want but this is not being done.	3. Programme managers do manipulate the system to get the reviews they want.
4. Proposals should not be 'blind reviewed' since it is not only difficult to conceal completely the identity of a proposer but it is 'a significant factor in determining the likelihood of success of a project'.	4. Proposals should be 'blind reviewed' so that 'the reviewer cannot play favourites or be biased by his knowledge or ignorance of the proposer'.
5. Reviewers should not be selected at random because the most knowledgeable persons would thus be eliminated.	5. Reviewers should be selected at because this would 'eliminate the possibility of the programme manager purposefully biasing the review through selecting reviewers whose opinions he can predict'.
6. '. . . the system should be designed on the presumption that programme managers and reviewers are, on the whole, honest and ethical, but that vigilance should be maintained over the system in such a way as to insure that unscrupulous acts are rare.'	6. It is 'best to design decision-making systems defensively, i.e., on the presumption that the proportion of dishonest or unscrupulous people among (NSF) programme managers and reviewers is high enough to cause severe problems if those people have a significant opportunity to turn the system to their advantage.'

TABLE 1 (continued)

Basic Assumptions and/or Contentions	
PRO the Current System	CON the Current System
7. 'Applicants should receive verbatim reviewer comments or requests but should not know the identity of reviewers;' 'reviewers will be more candid on all aspects of the proposal . . . if their [identities] . . . are kept confidential.'	7. 'Applicants should receive signed verbatim peer reviews or requests;' 'openness would result in more responsible and objective reviews.'
8. There should not be formal appeal procedures for rejected applicants; 'formal appeal procedures will introduce adversary relationships into the scientific community that have heretofore fortunately been missing.'	8. There should be formal appeal procedures for rejected applicants; 'a procedure is needed to check peer review and ensure that important innovations are supported.'
9. NSF should fund less research at colleges and less prestigious universities.	9. NSF should fund more research at colleges and less prestigious universities.

Source: Op. cit. note 13; see also note 15.

fully explicated and understood in terms of itself alone; we need to understand, at a minimum, how a position pertains to an extreme counter-position. One main purpose of a dialectical policy analysis is to make as explicit as possible the points of opposition between different views of an issue. Because it is vitally important to understand on which points parties disagree, Table 1 frames the debate and allows us hereafter to take the term 'policy analysis' as synonymous with a dialectical treatment of peer review.

Faced with a profound disparity of views, one may be tempted to trivialize or demean the position of one side or the other. Thus, for example, Gustafson states:

> A few *conservative* members of the House of Representatives have recently attacked the confidentiality of peer review in NSF and have questioned the integrity of its program officers. For example, Representative R. E. Bauman of Maryland denounced the peer review system in *bitter* terms on the floor of the House.[17]

Gustafson then cites an excerpt from Representative Bauman's remarks which may be, by any standard, 'bitter' indeed. However, whether they are bitter or not is tangential to the issue. That Representative Bauman is a 'conservative' is likewise tangential. Central to the issue is a deep and serious division between points of view that cannot be dismissed by attaching labels of liberal/conservative, bitter/favourably predisposed, and the like.

Other exchanges between proponents and critics demonstrate just how serious the division is, and underscore that it is far from unequivocally a case of one side being right, and the other being wrong; rather, the situation involves two distinctly differing points of view, each bolstered by cogent arguments. From its own perspective, each side is 'reasonable':

> *Mr Pressler.* Now, concerning the quality of the meetings being a bit higher if they are held in confidence, I am not sure why that is true in governmental meetings. I wonder if you could give us an example of why that would be true in your (scientific peer review) meetings?
>
> *Dr Sherman.* It has to do with one particular aspect of human nature. Even though the system is based on project grants, it is nonetheless necessary in the assessment of the project proposals to assess carefully the quality of the individual scientist named on the application. Sometimes, from my own experience with the system, the discussions about the individual's qualifications can be extremely heated. Now, it would seem in terms of the right of privacy of the individual, just because he is applying for funds from the Federal Government, that he should not have to lay out or make open to the public all of the considerations about his particular qualifications. The system can operate without jeopardizing the right of the individual.
>
> *Mr Pressler.* If there was something being used against that individual that was not entirely true, he should have a chance of rebuttal or to correct any misinformation. Then people would have a way of knowing. *That is the other side of the coin.*[18]
>
> *Mr Schever.* Recently we [the Congress] have discovered what a cleansing effect openness has. It seems to cure a lot of problems. There may be problems with openness in the scientific decision making process, but we have not anticipated that they would be very serious. Recently, the Congress has moved from secrecy to openness. It used to be that after we heard from people like you [Dr Sherman] in our hearings we would go into executive session and do our markups. When it was suggested that the markups be made in public, with people listening to us talking or negotiating, many feared that the system would break down. Many

> thoughtful members felt that going to public markup sessions was an exercise in sheer idiocy. They feared that the majority and minority would not be able to compromise, and that we could never settle anything or report a bill out of committee. We changed this procedure and I think that everyone agreed that the system has been vastly improved.
>
> You have described the [peer] review system as one based on mores. Mores can change. They can yield to the pressures of changing time and conditions . . . In the past, the lifestyle based on these mores [that is, secrecy] promoted a degree of integrity, decency, and internal fair dealing . . . An awful lot of problems would be solved if we changed our system from one based on confidentiality to one based on openness and fair treatment. I don't think that we are now aware of all the problems connected with an open system, but I am willing to bet that many of these problems would not materialize.[19]

Although these quotations reveal the intensity of the differences between the two sides in the debate, they underestimate the significant propositions on which both sides agree. We have detected at least five points of consensus, namely:

(1) 'No method superior to peer review has been found for judging the scientific competence of proposers. Scientific peers are better able than others to judge the design of proposed work, the importance of proposed work to the scientific field, and the past performance of the proposer. Appropriate peer review procedures generally lead to the support of proposals in a high quality range. Using peer review procedures [NSF] has successfully fostered significant advances in basic science over the past 25 years.'[20]

(2) 'Witnesses agreed overwhelmingly that some form of peer review should continue to be used to assist in the allocation of Federal funds for scientific research. Not a single witness suggested that peer review be abandoned, although several witnesses proposed changes in the decision-making processes of [NSF] — some minor and some major in their potential effects.'[21]

(3) 'While many witnesses avowed that peer review results in the support of high-quality research, some of which is truly innovative, there was not much confidence expressed that peer review consistently leads to the support of innovative research if it challenges the mainstream of scientific thought or if it seems unlikely to succeed. Arguments and the weight of opinion to the contrary were rather persuasive.'[22]

(4) 'The Subcommittee had ample opportunity during the hearings to explore whether Congressional review of individual [NSF] grants should be required in addition to Foundation approval before Foundation's action becomes final . . . Opinion was over-

whelmingly against Congressional review.'[23]

(5) No one ascribed to the extreme position that 'applicants should know nothing about who reviewers were or what they said.'[24]

Such substantial agreement on these propositions makes them no less true or false, however, than the propositions over which there is explicit disagreement. In either case, beliefs may be so deeply held by their proponents that, at least for them, they may be immutable and irrefutable — utterly impervious to evidence. For those who are not so rigidly committed to either side as to dismiss either position a priori, the question is: What evidence, if any, exists or could be produced that bears on the various propositions and could alter the beliefs of proponents and critics alike?

The Evidence

In reviewing the evidence marshalled in support of each side of the debate characterized above, we shall restrict our attention to two studies: one conducted by Deborah Hensler,[25] the other by Stephen Cole and his colleagues.[26] The justification for this restriction is that, based on a review of materials on the NSF peer review system,[27] these two contain the most comprehensive bodies of empirical evidence which speak directly to some of the contentious issues in peer review. The review of each study will consist of describing its focus, data, and principal findings, followed by our assessment of its merits and shortcomings. Finally, we shall discuss the inferences about gaps in knowledge which can be drawn from the current literature on peer review — theoretical and operational gaps which invite further imaginative study to effect closure on key issues in the debate.

The Hensler Study

In 1975-76, Deborah Hensler sent a questionnaire to a 5 percent random sample ($n = 1552$) of approximately 31,000 persons who had served as reviewers of research proposals submitted to NSF during fiscal year 1974. The identical questionnaire was also sent to a random sample of 3256 applicants for NSF grants during the period (a 16 percent sample framed by a population of 20,000 grant applications). For both the reviewer and the applicant samples, the response rate exceeded 80 percent.

Both the original questionnaire items and the resultant analyses of responses were designed to attempt to address several points of contention in the debate. An initial question concerns the similarity of backgrounds of reviewers and applicants. Are applicants being evaluated by those similar to themselves or by those who are significantly different? In terms of age, geographical location, institutional affiliation, and so on, the major difference Hensler finds between the two populations is that 'applicants are somewhat more likely [at a probability level less than 0.05] than reviewers to belong to a more recent academic generation and to be currently located at a non-PhD granting institution.'[28] In general, applicants *are* being evaluated by their peers, if by 'peer' one means one who is similar in professional and demographic background.

On appraising the peer review process, those participating as ad hoc mail reviewers (45 percent of the total respondents across all divisions [directorates] of NSF) saw the system as 'sound'; half saw it as an 'an acceptable peer review mechanism with some weaknesses', and only 4 percent saw it as 'a questionable peer review mechanism with many weaknesses'.[29] In comparison, of those participating as panel review members, 60 percent saw the system as 'sound'; 34 percent as 'acceptable with some weakness'; and 5 percent as 'questionable with many weaknesses'. Analysis of the scaled and open-ended responses generally supports the contention that 'reviewers' assessments of the peer review process based on their experience during the past two years are largely positive.'[30]

The issue of whether particularistic factors may intervene in the evaluation process and influence the reviewer's recommendation to fund was broached by three variants on a single question in the Hensler survey: Given two equally good proposals except for one marked difference, which proposal did the respondents think had a better chance of receiving peer review recommendation to fund? In the first case, one of the proposals was from a well-known institution; the other from a lesser-known institution. In the second case, one of the proposals was submitted by a young, as-yet not established principal investigator (PI); the other, by an older, well-established PI. In the third case, one of the proposals featured approaches which were consistent 'with the mainstream of thought' in the discipline or research area; the other, a project which challenged the mainstream of thought. This last case, of course, concerns one of the pivotal issues in the peer review debate: whether or not the system is biased against innovative ideas.[31]

The responses to these three questions were unequivocal: 52 percent of the reviewers and 61 percent of the applicants felt that the proposal from the well-known institution had a better chance of being funded; 29 percent of the reviewers and 16 percent of the applicants felt both had an equal chance; less than 3 percent of each group felt the proposal from the lesser-known institution had a better chance. The responses to the other two questions were similar: the older PI and the 'mainstream' proposal, respectively, were favoured.

It is particularly instructive to compare the responses of applicants from (a) institutions that are among the top 20 in securing federal research funds[32] with (b) those located at other PhD granting institutions. Whereas 28 percent of the applicants from the top 20 believed that proposals from both the lesser and the well-known institution have an equal chance of being funded, only 14 percent of the applicants from the other institutions believe this to be the case. Even more revealing is that 39 percent of the 'top 20' applicants believe the proposal from the well-known institution has a better chance, whereas 63 percent of the applicants from the other institutions believe that the well-known institution fares better in the competition.[33]

Finally, how do the respondents regard the confidentiality or openness of the peer review process? Hensler summarizes her respondents' views as follows:

> A substantial majority of reviewers and applicants approve of NSF's new policy of providing verbatim review comments to applicants. About two-thirds of the applicants surveyed indicate they personally would have found verbatim review comments useful the last time they submited a proposal to NSF. Respondents who favor a policy of providing verbatim reviews to applicants say the reviews help applicants to understand the reasons for reviewers' reactions, permit applicants to judge reviewers' competence and provide a possible basis for rebutting reviews. A minority of reviewers — nineteen percent — would approve of a policy of identifying reviewers to applicants and thirty-five percent say they would refuse to continue as reviewers if such a policy were adopted. But close to one-third of the applicants would approve of such a policy. Among the applicants who have *not* also served as reviewers recently more than forty percent would approve of identifying reviewers. Applicants from more recent academic generations and those affiliated with academic institutions which are *not* among the top twenty in federal research funding are more likely to approve of identifying reviewers to applicants, than others. Applicants with recent or previously

> unsuccessful experience obtaining NSF funds are most likely to approve of identifying reviewers. Those who disapprove of identifying reviewers feel that this would lead to lower quality reviews, more difficulty securing participation of reviewers and cause bad feelings among colleagues in the scientific community, among other results.[34]

Reactions to the Hensler Study

While the Hensler survey was the first to document so extensively the perceptions of scientists who had participated in the NSF peer review process, the study was not without its limitations (as Hensler openly acknowledges). For example, the study was initiated by a NSF committee the composition of which is not specified in the report.[35] Originally, the committee was interested in the views of three groups: NSF peer reviewers, recent applicants for NSF funds, and researchers who had served NSF neither as reviewers nor as recent applicants. It is reported that

> after some consideration, the Committee decided that it was not feasible to survey the latter group. But by drawing from the Foundation's files of reviews and proposal actions, it was possible to select two independent samples of recent NSF reviewers and applicants.[36]

We have no way of ascertaining on what grounds it was decided 'not feasible' to survey the opinions of researchers who have neither served as reviewers for NSF proposals nor applied recently for NSF funds. Insofar as the Hensler study demonstrates dramatically the link between one's experiences with and perceptions of the NSF system, it would seem desirable to pursue this feasibility question. We shall argue later that it would seem not only highly desirable but imperative to survey the 'null' group: those who, for whatever reasons, have chosen not to interact with NSF or whose interaction has not been sought by NSF. What are the demographic characteristics of those scientists not interacting with NSF? What are the reasons they give for not doing so? What reasons would NSF personnel give? A study of the attitudes of this null group would seem necessary before general conclusions about the equity of NSF's peer review system can be drawn.[37]

Because the Hensler survey does not 'tell us all that we would like to know about NSF reviewers' and applicants' experiences with the

NSF review process and their attitudes toward this process',[38] future surveys (or other studies, for that matter) must augment our knowledge claims. To wit, we would like to know more about their experiences with NSF, but we should also know more about them qua working scientists, and in relation to their views of science. *The Hensler study precludes inferences about NSF's peer review process not only by restricting the range of questions asked of the respondents, but also by restricting what we know about the respondents.* Even if one retained the current set of questions asked about the process, other questions about the respondents should be asked.

For example, much is made in Hensler's report of the finding that a substantial proportion of both reviewers and applicants feel that a proposal which is consistent with the mainstream of thought in an area stands a better chance of being funded; indeed, 53 percent of the reviewers and 60 percent of the applicants who have been recently declined for a NSF grant and who, in addition, have been previously unsuccessful in securing a grant, concur with this proposition. Given the data and the contention of Congressmen, among others, that the peer review system is generally unresponsive to new or innovative ideas, it would seem especially desirable to seek out and secure the views and experiences of those who can be identified as 'innovative'.

If the measurement (if not the definition) of 'innovativeness' is fraught with difficulties, then calling for the study of those judged to be particularly innovative might merely seem to exchange one can of worms for another. Nevertheless, there now exists a growing literature in the social psychology of science[39] which makes it possible to identify and to differentiate empirically 'more innovatively' minded from 'less innovatively' minded scientists. It would be germane to the debate to know the views of those scientists who may be classified as innovators. Are they as a group more sensitive to the perception of bias (or its absence) in the system? Are they even more sensitive to the lack of receptivity afforded innovative ideas? Are they less likely to apply for a NSF grant because of their perceptions, rightly or wrongly, of NSF? Or is it rather because of their particular innovativeness that they are able to play grantsmanship — that is, to clothe novel ideas in mundane or conventional terms?[40]

Overall, the most serious deficiency of the Hensler study is its fundamental concern with attitudes — that is to say, with what reviewers and applicants *believe* to be the operation of the NSF peer review system — and not with more direct evidence of how it actually operates (which the study never purported to establish). Lest our intent be misunderstood, we are not thereby disparaging the value of the study. What scientists believe about an institution that vitally affects them is key information. The fact that so many scientists would oppose the disclosure of names of reviewers is important information in its own right. It can inform those in power that strong opposition awaits if a policy of disclosure were instituted. This finding does not indicate, however, to what extent and what forms such opposition might take, or whether the benefits of a new policy might so outweigh the disadvantages that the change would be worthwhile.

It is not that the Hensler study is merely a survey of beliefs, but that it fails to penetrate to the heart of the debate, and consequently, does not aid in its resolution. To facilitate movement towards resolution, at the very least, other crucial beliefs would have to be exposed; at best, there would have to exist some other method(s) for assessing the actual state of the system. As they stand, *the Hensler data do not prove that the NSF peer review system is either biased or unbiased, but that there are sizeable numbers of scientists whose experiences (that is, reviewers versus successful applicants versus unsuccessful applicants) predispose them to support one side or the other of the debate.*

The Cole, Rubin and Cole Study

The Cole, Rubin and Cole study represents the most ambitious project to date to determine the actual operation of the NSF peer review system, at least in its basic research programmes.[41] Commissioned by NSF on behalf of the National Academy of Sciences, the study by Cole and his colleagues (hereafter referred to as Cole) seeks to provide evidence that is independent of scientists' beliefs or attitudes about the presence or absence of biases in the NSF peer review system. The kind of evidence sought by Cole thus augments that yielded by the Hensler survey. Specifically, Cole conducted

> seventy in-depth interviews with scientists involved at all levels of the peer review system, including program directors, former program directors, mail reviewers, review-panel members and supervisory-level NSF officials. We also scrutinized

> more than 250 specific research proposals, read all of the peer review comments on those proposals and examined all of the correspondence between the applicant and the program director . . . In addition, we conducted a quantitative analysis of 1200 applicants to the NSF in the fiscal year 1975. (Roughly half of the applicants were ultimately awarded grants.) The purpose of the quantitative study was to identify those characteristics that were correlated with the receipt of a grant from the NSF.[42]

The characteristics chosen for analysis (basically a series of multiple regressions) consist of nine 'social stratification' variables, including rank of PhD-granting department, current academic rank, and three measures of publication and citation. These variables were then correlated with the ultimate disposition of a grant proposal measured in the aggregate as 'percentage of applicants receiving grants' and 'ratings received on proposals' (trichotomized as high, medium, and low).[43]

Taken together, analyses of these variables are intended to test the validity of two hypotheses which dominate the peer review debate. The first (the 'old-boy' hypothesis) lacks 'conceptual clarity', according to Cole. Does old-boyism refer to 'investigators with a common view of their field', 'networks of friendships', or to 'social position' ('level of eminence')?[44] The second (the 'rich get richer' hypothesis) stipulates that particularistic factors (that is, those unrelated to the merit of a proposal) result in an unfair advantage (for example, for the more eminent and/or those located in high ranked departments) in gaining grant approval.[45]

Based on their quantitative analyses, Cole interprets the evidence as a refutation of both hypotheses:

> The overall pattern of our data suggests that scientists with an established track record, many scientific publications, a high frequency of citations, a record of having received grants from the NSF and ties to prestigious academic departments have a higher probability of receiving NSF grants than other applicants do. *Nevertheless, the granting process is actually quite open and there is nothing approximating a scientific caste system.*[46]

> Of the variance that can be accounted for in funding decisions, the peer review rating (among the social stratification variables) is by far the best predictor.[47]

> . . . a scientist's past performance as measured by citations of his work and his recent NSF funding record does lead to a very slight accumulative advantage, but his academic affiliation does not appear to give him any advantage.[48]

Not surprisingly, in summarizing the results of their study thus far, Cole suggests that

> the scientific enterprise is an exceedingly equitable, although highly stratified, social institution in which the individuals who produce the work that is most favourably evaluated by their colleagues receive the lion's share of the rewards.[49]

Reactions to the Cole Study

We think the Cole study begs several questions that are vital to the peer review debate. To compound this error, the authors make some definitive-sounding extrapolations that seem unfounded by their data.

First, to conclude that 'the peer review rating is by far the best predictor' of the probability of receiving a grant by no means suggests that this rating is a good predictor. Indeed, '89 percent of the observed ratings is left unexplained by the nine variables.'[50] This would indicate that factors other than 'social stratification' variables are at work. Yet no such factors are either employed in the analyses or conjectured in discussion of those analyses. Thus the finding that 'individuals who produce the work that is most favourably evaluated by their colleagues receive the lion's share of the rewards' circumvents the questions of why the work is favourably evaluated. No measure of its significance or innovativeness is presented; we are simply asked to believe that voluminous citation of articles denotes their high quality.[51] (After all, proposals which seek to extend such widely-recognized work must be of sufficient merit to justify NSF's decision to fund.) If these results accurately describe peer review in the basic research programmes of NSF, then Cole must further show why this system is 'extremely equitable, although highly stratified'. They have not done this, their rhetoric notwithstanding.

Second, what Cole recognizes in the data, but overlooks in the interpretation, is that evaluation of a producer is hopelessly intertwined with the evaluation of his or her product in science. If producer and product cannot be separated analytically, then we must ask: Where in the distribution of peer review ratings does particularism tend to prevail? If high consensus is achieved in both tails of the rating distribution (as the data attest),[52] then the critical region of peer review is in the middle. This is the grey area where particularistic factors (such as the applicant's present affiliation or institution of PhD) colour the perceived quality of the proposed research. Since performance or 'track record' (that is, reputation) is supposed to be an explicit factor in reviewing for NSF, particularism has been institutionalized as a (partial) rationale for making both favourable and unfavourable decisions to fund. The contradiction is legitimate; tension between universalism and particularism is built into the peer review process. Why deny this fact, as Cole seems to do? And why present no data (for example, from interviews) which might contain clues about the tension — namely, the extent to which perceptions of quality are coloured by particularistic considerations?[53]

Third, if we ponder — as we did in our reaction to the Hensler study — the kind of data needed to advance the peer review debate beyond its present impasse, our thoughts return to the characteristics of reviewer and/or applicant which might influence the final decision to fund or not to fund a proposal.

Recent studies of the cognitive-styles of inquiry of scientists reveals that one of the key dimensions distinguishing various styles and scientists from one another is the ability to make, as well as to appreciate, fine differentiations between people, objects, or institutions.[54] Persons who excel at this ability are called 'high differentiators'. They, in short, have a high tolerance for ambiguity. As Gordon and Morse put it:

> High differentiators perceive their environment as a series of discrete parts while low differentiators see their environment as highly homogeneous . . . The ability to differentiate manifests itself in two related ways depending on the nature of the stimulus, human or inanimate. In interacting with people the high differentiator perceives and reacts to each as a unique individual possessing a combination of capabilities and inabilities. The low differentiator perceives people as being more or less alike and thus tends to suppress or ignore individual capabilities.[55]

The point is that low differentiators would tend to see the personal characteristics of an investigator as irrelevant to a proposed investigation because they would see all investigators in a similar light. High differentiators, on the other hand, tend to see personal characteristics as very relevant. Specifically, then, does the sample studied by Cole contain an overabundance of low differentiators? Does a large sample tend to mask or damp out the effect of high differentiators? Does the institutional or social process of rating proposals induce even a high differentiator to act like a low differentiator? That is, does the social process of rating proposals foster a 'do-unto-others-what-might-be-done-unto-you' approach? In short, if we had a sample of clearly identified high differentiators and another of clearly identified low differentiators and we gave each the same set of proposals to rate, would their ratings be the same? Before one can reject the hypothesis that the characteristics of the rater/reviewer and those of the individual being rated are irrelevant to the ultimate disposition of a proposal, one must at least attempt to construct a kind of experiment to test the hypothesis.

Until this is done, *the Cole data and analysis cannot be used to ignore or deny the relevance of personal or cognitive attributes in the operation and understanding of peer review.*

Finally, we feel compelled to remark that despite Cole's claim of 'complete autonomy from NSF in conducting' the research,[56] the research they have reported betrays a commitment to show that even where peer review is not equitable within NSF, the inequity is for the good of science;[57] put another way, inequity is functional for the maintenance of the system — and peer review is the tool of this handiwork. Though they began with good intentions, Cole and his associates may have done more to defend the status quo than to inform the debate on peer review: their evidence has yet to sustain the weight of their conclusions. We eagerly await their complete results.

Further Reflections on the Debate

Suppose that there existed a method of establishing 'conclusively' whether the peer review system was either biased or unbiased.[58] If we take the Hensler findings at face value, then we must acknowledge that a significant number of scientists believe that the NSF peer review system is biased, while another significant number believe that it is *not* biased. One could then construct, in ideal-typical fashion, the contingencies represented in Table 2, where the rows represent the beliefs or judgments of scientists as to whether the NSF peer review system is perceived as biased or unbiased.

TABLE 2
Beliefs Versus System States
In Peer Review

		STATE OF SYSTEM	
		Unbiased	Biased
BELIEF OF SCIENTIST	Unbiased	I Correct	II Problematic
	Biased	IV Problematic	III Correct

The columns represent the admittedly oversimplified case where the actual state of the system is either biased or unbiased.[59]

Cases I and III represent the supposedly 'true' or 'correct' situations, where the system is either unbiased or biased and the perceptions or beliefs of scientists match the correct state of the system. Cases II and IV represent the more interesting and 'problematic cases':[60] we can make this judgment, and claim that these two belief conditions demand special examination, even if we cannot determine the absolute state of the system. Suppose for a moment that the NSF peer review system is biased. What, if anything, would it take to convince the sizable number of scientists who believe that it is unbiased to think otherwise? A body of social psychological evidence and arguments suggests that on the whole scientists are conservative in their judgments,[61] and that those who select a career in science partially do so because they have an overly developed need to believe in the orderliness of the world, if not in its ultimate rationality.[62] Consider, too, the oversocialization argument: the vast majority of scientists are trained for normal, workaday science and not for great or extraordinary science.[63] They are neither trained or interested in challenging old theories,[64] let alone prepared to invent novel or 'revolutionary' theories. At the same time, since strong evidence and arguments exist that the system of science is strongly élitist in its structure and orientation,[65] Case II cannot be dismissed or ignored.

To state the matter somewhat differently, Cases II and IV represent situations of denial or projection. Case II represents the situation of denying there is a problem when there is; Case IV represents the situation of asserting there is a problem when there is not. Case II entails the classic phenomenon of identifying with the aggressor, where in order to ease the painful admission of being the underdog, the underdog or victim overly identifies with the values of the aggressor. The question is: How many of those scientists saying that the NSF peer review system is unbiased are identifying, consciously or unconsciously, with the values of élite scientists? For analytical purposes, scientists are constantly being grouped into 'élite' versus 'non-élite'. Given the endless jostling for position that goes on in academic life, plus the constant ratings of departments and institutions to which scientists are subjected, we can plausibly assume that scientists themselves are aware of their relative standing.[66] What does it do to the self-esteem of scientists to know they are located in an élite or non-élite department or institution? Can we expect this to have no effect on the operation of the system — or, at the very least, on their beliefs about the system?

Indeed, the question that now emerges is whether scientists differ systematically by discipline, institution, or research area in their beliefs of the presence or absence of bias in the system. This question fuels speculation on the relation between individual and social (systemic) innovativeness hypothesized earlier: Are individuals whose cognitive-style betrays a high propensity to innovate viewed as such by their colleagues? Does this research in fact reflect their innovativeness? Furthermore, do these innovators communicate more frequently with other innovators, and in this sense nurture one another? Finally, and most importantly, are innovators located in greater or lesser abundance at prestigious institutions?

The evidence that élite scientists tend to associate and communicate with other élite scientists more frequently than they do with non-élites, and that élite scientists tend to be affiliated with élite institutions,[67] would suggest that an examination of the interplay of psychological, intellectual and social factors operating in the peer review process is in order. Above all, if sustained innovativeness and éliteness go hand in hand, the concentration of innovative ideas and high quality research proposals submitted by those in a select pool of institutions would need no remedy. However, the distribution of high innovators in the scientific community (as well as in the subpopulation of applicants for NSF funds) is unknown.

This missing link in the debate — a control variable, if you will — signals a need to measure the cognitive styles and background beliefs of participants in the peer review process before one can interpret either (1) the meaning of responses to a survey such as Hensler's, or (2) a quantitative analysis (such as Cole's) which sacrifices qualitative insights into individual differences for statistical significance. Finding this missing link is all the more necessary given Mitroff's findings from his Apollo moon study.[68] Nearly all of his scientists were extremely sceptical of the conventional portrait of the scientist as a neutral, unbiased, objective observer of nature. Moreover, the overwhelming majority of those interviewed gave revealing reasons for why they thought scientists in their role as scientists should not be entirely unbiased. The majority view was that it was necessary for scientists to act as partisan advocates for their hypotheses and theories lest those theories suffer a premature death. This means not that these scientists neglected or refused to test critically their theories, but that their actual conduct of science is more complicated than that portrayed in conventional accounts. The relevance of these findings to the present discussion is this: Can we expect scientists' views on the general operation of science as an intellectual and social system not to influence their views regarding peer review? Were the moon scientists' views exceptional (they too, constitute an élite sample) or more the rule than previously thought? These are fittings topics for future studies, and in our opinion, vital for further assessment of the peer review system.

What we now know better about the peer review debate from the Hensler and Cole studies is summarized in Table 3. *In our judgment, these data may be necessary, but are insufficient, to settle the principal issues. Although they enhance our understanding in dialectical terms, the data underscore the serious gaps that exist in our knowledge — gaps which must narrow if the debate is ever to approach closure.*

Conclusions and Recommendations

The principal conclusion of this review is that the current data are inconclusive to resolve the debate represented in Table 1; nevertheless, some data do exist to support contentions on each side of the debate.

Because issues such as those inherent in peer review expose tensions in the workings of science as a social system, they call forth deep divisions of value. Such issues, therefore, may not be amenable to treatment (and hence, to resolution) via conventional methods. The debate instead calls for treatment of the issues from more than one theoretical point of view. Philosophers of science have long recognized that scientific data can neither be collected in the first place, nor analyzed in the second, apart from some prior theoretical point of view.[69] That is, one does not collect data without having presupposed some hypothesis, theory, or model, no matter how implicit, unconscious, or informal it may be.

We would assert that at least three models undergird the peer debate; (1) the Accumulative Advantage Model; (2) the Political Model; and (3) the Merit Model. The Accumulative Advantage Model derives from the 'Matthew effect', as explicated by Merton:[70] one who has developed a good reputation based on past

TABLE 3

Relation of the Hensler and the Cole Studies to the NSF Peer Review Dialectic

Evidence in Support or Denial of Assumptions	Basic Assumptions and/or Contentions (see Table 1)	
	PRO the Current System	CON the Current System
Assumption	1. Current system is open, free from bias	1. Current system is closed, contains bias
Evidence:	Supporting: similarity of characteristics of reviewers and applicants; vast majority of respondents see system as sound; believe they were treated fairly.	Denying: extremely few respondents see current systems as possessing major flaws or believe they were treated unfairly.
Status of evidence	Weak on both sides of argument; insufficiency of beliefs per se to determine actual operation of system.	
Assumption	2. System encourages innovative ideas	2. System blocks innovative ideas
Evidence:	Denial: less than 7 percent of respondents believe a proposal which challenges 'mainstream' has a better chance of being funded; roughly 20 percent believe both have an equal chance.	Support: nearly half of respondents believe a 'mainstream' proposal has a better chance of being funded.
Status	Weak; insufficiency of data to establish operation of system.	

TABLE 3 (continued)

	PRO the Current System	CON the Current System
Assumption	3. Programme managers do not manipulate reviews	3. Programme managers do manipulate reviews
Status	Not addressed explicitly in Hensler study; data collected, not reported by Cole	
Assumption	4. Proposal should not be blind reviewed	4. Proposals should be blind reviewed
Status	Not addressed explicitly by Hensler; institutionalization of particularistic factors into review process recognized by Cole.	
Assumption	5. Reviewers should not be selected at random	5. Reviewers should be selected at random
Evidence:	Support: only 15 percent of respondents at most believe in randomization; nearly 65 percent believe in some form of judgment sampling in conjunction with NSF staff	Support: roughly 31 percent believe in some form of randomization and judgment sampling
Status	Weak; insufficiency of beliefs to warrant procedural change.	
Assumption	6. System should be designed on presumption of honesty	6. System should be designed defensively
Status	Not tested for explicitly; charge of 'old boy-ism' refuted, according to Cole.	

TABLE 3 (continued)

	PRO the Current System	CON the Current System
Assumption	7. Applicants should not know reviewers' identity	7. Applicants should know reviewers' identify
Evidence:	Support: roughly 40-50 percent of respondents feel knowing name would make no difference; 12-20 percent feel comments would be less useful	Support: 30-40 percent feel comments would be more useful if one knew name
Status	Weak; again, insufficiency of beliefs to warrant procedural change.	
Assumption	8. There should not be formal appeal procedures	8. There should be formal appeal procedures
Evidence:	Support: feeling that a formal process will further bureaucratize system	Support: about 73 percent of applicants favour a formal appeals system as a remedy for mistakes
Status	Endorsement of idea by Hensler; no consideration of procedures.	
Assumption	9. NSF should fund less research at colleges and less prestigious universities	9. NSF should fund more research at colleges and less prestigious universities
Status	'Rich get richer' hypothesis supported in part by Cole data: researchers at more prestigious institutions and with track record slightly favoured in the review process.	

work accrues more advantages (that is, disproportionately) than those lacking such a good reputation. This cumulates over time so that the 'rich get richer'. The Political Model stipulates that certain élite scientists at élite institutions have disproportionately more access (a) to other élite scientists and scarce scientific resources (for example, information and research funding) and (b) to governmental agencies such as NSF, where they exert influence on science policy and its implementation in their roles as gatekeeper, advisor, and peer reviewer.[71] Finally, the Merit Model states that the work of a scientist is judged primarily on its merit, that research monies are awarded competitively according to universalistic criteria which favour, above all, the applicant's current ability to perform.[72]

We hasten to add that merit is a component in each of these models, but is differentially weighted. Again, in terms of the universalism-particularism continuum, we predict that particularistic factors (that is, attributes of the scientist) tend to predispose reviewers to favourable evaluation of the scientist's work. This emphasis is typically an outgrowth of prior, and oft-repeated, evaluation of that scientist's other work as meritorious. Unlike the Merit Model, both the Accumulative Advantage and Political Models recognize this 'contamination' of evaluations. Alternatively put, these models treat discrete research products (for example, a new proposal or book) as continuous in time or imbued with the quality (fixed at a certain threshold, it would seem) of its producer. In brief, evaluation of research is highly contingent on its source. Adherents of these two models would insist that supporting such researchers — 'the best' — is functional for the system; therefore, favourable peer review of the research in question should follow suit. Those operating on the Political Model would rely more on particularistic-factors than on merit of the specific proposal in recommending disposition. Reviewers enamoured of Accumulative Advantage would attend somewhat less to the credentials of the researcher and more to the substance of the proposed research. Finally, those utilizing merit as the chief criterion of funding support would resort to proposal details per se, far more than to characteristics of its author. These models, then, capture the tension inherent in the reviewer role — a tension which encompasses both the discharge of the particular reviewing task and the overarching mentality one brings to the task.[73]

How, then, have these models been applied to studies of the peer review process? In Hensler's study, none of the three models appears to be presupposed. That is, in the design, conduct and analysis of her survey, Hensler was essentially atheoretical. In contrast, the Cole study embraces the Merit Model. Cole exhibits a marked preference for (if not an a priori belief in) the Merit Model. We have no quarrel with this, as Cole makes the preference explicit, claiming later, as we have seen, that the data tend to support the Accumulative Advantage Model. However, this preference is troublesome if one suspects that something so complex as peer review requires simultaneous and explicit examination from a number of diverse and competing theoretical perspectives. Even stronger, the same set of data ought to be examined from the perspective of each model. Because each, in all likelihood, is partially correct, future studies must establish, for example, under what circumstances each model obtains. What we are advocating is a testing of the alternatives — new data collection and analysis — to expand the empirical base that impinges upon and must eventually mediate the debate.

There are other aspects, however, to the peer review process and to the debate which we have not considered. Foremost among these is the role of the public in shaping the institutions which purportedly operate on its behalf. What does the public want from NSF management? Does this differ from what the scientific community wants? Do NSF practices produce the best science, and are they conducive to the optimal long-term development of knowledge?[74] Is it not the responsibility of the scientific community and federal agencies such as NSF to invite interested lay parties to enter the dialogue among experts,[75] especially when some of the most important persons for whom the studies are being conducted are not scientists?

If the crux of the peer review debate is the analysis of negotiations between science and its environing communities and not solely negotiations within the scientific community,[76] then science must promote research that illuminates both negotiating processes. Like our predecessors, we have emphasized the latter in this paper. The former, however, is an equally, if not more, vexing research problem that will not conveniently fade away.

The study of scientific autonomy and self-governance is really the study of the science-government partnership. What we have recommended is that this study begin by linking the social psychology of the protagonists to their respective roles in the conduct of scientific inquiry. Only then will the debate over peer review fulfill the promise of a dialectical policy analysis; only then will the debate prescribe changes of policy into practice.

● *NOTES*

A presentation based on an early version of this paper was made at the Second Annual Meeting of the Society for Social Studies of Science, held in Boston, Massachusetts, and Harvard University, 14-16 October 1977. The incisive comments of P. Thomas Carroll on that version were most helpful in rethinking and rewriting.

1. See T. Gustafson, 'The Controversy Over Peer Review', *Science*, Vol. 190 (2 December 1975), 1060-66; D. Shapley, 'House Votes Veto Power on All NSF Research Grants', ibid., Vol. 188 (25 April 1975), 338-41; Shapley, 'NSF Violations of Personnel Code Alleged', ibid. (31 May 1975), 915; J. Walsh, 'NSF and Its Critics in Congress: New Pressures on Peer Review', ibid. (6 June 1975), 999-1001; Walsh, 'NSF House Appropriations Panel Gives Warning Tug on Purse Strings', ibid., Vol. 189 (4 July 1975), 26-28; Walsh, 'NSF Peer Review Hearings: House Panel Starts with Critics', ibid. (8 August 1975), 435-37; Walsh, 'Peer Review: NSF Faces Changes, the Question is How Extensive', ibid., Vol. 190 (17 October 1975), 253-56.
2. S. MacLane, 'Peer Review and the Structure of Science,' *Science*, Vol. 190 (14 November 1975), 617.
3. G. M. Lyons, *The Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century* (New York: Russell Sage, 1969). See also J. Haberer, *Politics and the Community of Science* (New York: Van Nostrand Reinhold, 1969).
4. Our mention of this caveat stems from two sets of experiences: firstly, in response to the first public draft of this paper, where colleagues rhetorically asked, 'You're not against peer review, are you?'; and secondly, our inability to convince NSF programme managers that despite the recent studies of their system, the 'crucial experiment' had yet to be performed. We claimed that the present system had been legitimated, but not accurately appraised because (1) many of the right questions had not been asked, or if they had, the answers were not carefully weighed (for example, could *not* be quantified), or (2) the crucial data were not in the public domain, but with special consent could be made available for analysis. Consequently, the study whose findings could be used to alter policy and strengthen the operation of the system never materialized. Our interpretation may smack of sour grapes, but we think the opportunity for internal self-scrutiny was lost (although congressional critics were pacified — for the moment). We do *not* think our argument fell on deaf ears; rather, no one was willing to handle a hot potato which had temporarily cooled.
5. For a review, see H. Zuckerman and R. K. Merton, 'Age, Aging, and Age Structure in Science', in M. W. Riley, M. Johnson and A. Foner (eds), *A Theory of Age Stratification*, Vol. 3, *Aging and Society* (New York: Russell Sage, 1972), 292-356.

6. In general, referees and advisors are older, more eminent and published, and located at more prestigious institutions than the 'average' member of the scientific community. See N. C. Mullins, 'The Structure of an Elite: The Advisory Structure of the Public Health Service', *Science Studies*, Vol. 2 (1972), 3-29; L. Groeneveld, N. Koller and N. Mullins, 'The Advisers of the US National Science Foundation', *Social Studies of Science*, Vol. 5 (August 1975), 343-54; M. J. Mulkay, 'The Mediating Role of the Scientific Elite', *Social Studies of Science*, Vol. 6 (1976), 445-70.

7. Studies of NIH peer review include G. M. Carter, 'Peer Review, Citations, and Biomedical Research Policy: NIH Grants to Medical School Faculty' (Santa Monica, Calif.: Rand Corporation Report R-1583-HEW, 1974); C. Henley, 'Peer Review of Research Grant Applications at the National Institutes of Health', *Federation Proceedings*, Vol. 36 (July 1977), 2066-68, 2186-90, 2335-38; NIH Grants Peer Review Study Team, 'Grants Peer Review: Report to the Director, NIH, Phase I, Vols. I-III' (Bethesda, Md.: National Institutes of Health, December 1976).

8. A provocative formation of this relationship can be found in J.-J. Salomon, 'The Mating of Knowledge and Power', *Impact of Science on Society*, Vol. 22 (January-June 1972), 123-32.

9. By 'dialectic' we mean more than a mere polar opposition or conflict between viewpoints. We mean that viewpoints are intensely opposed to one another, though the meaning of one is dependent on the other; that is, either viewpoint is completely self-contained but is defined, if only in part, through the other. Be this as it may, we are more interested at present in the *operational* use of the dialectic as a unique methodology for analyzing issues, rather than in quibbling about the various historical meanings of the term.

10. C. West Churchman, *The Design of Inquiring Systems* (New York: Basic Books, 1971); P. Feyerabend, *Against Method* (London: New Left Books, 1976); G. Holton, *Thematic Origins of Scientific Thought* (Cambridge, Mass.: Harvard University Press, 1973); A. Kantrowitz et al., 'The Science Court Experiment, an Interim Report', *Science*, Vol. 193 (20 August 1976), 653-56; M. Levine, 'Scientific Method and the Adversary Model: Some Preliminary Thoughts', *American Psychologist*, Vol. 29 (September 1974), 661-77; R. O. Mason, 'A Dialectical Approach to Strategic Planning', *Management Science*, Vol. 15 (1969), B-403-14; R. K. Merton, *Sociological Ambivalence* (New York: The Free Press, 1976); D. Nelkin, 'Thoughts on the Proposed Science Court', *Newsletter on Science, Technology, and Human Values*, No. 18 (1977), 20-31.

11. Churchman, Feyerabend and Mason, ops. cit. note 10.

12. Mason, op. cit. note 10.

13. 'National Science Foundation Peer Review, Volume I', A Report of the Subcommittee on Science, Research and Technology of the Committee on Science and Technology, US House of Representatives, Ninety-Fourth Congress, Second Session (January 1976).

14. For example, Gustafson, op. cit. note 1.

15. Verbatim quotes have been purposefully included in Table 1 to indicate the depth and sincerity with which the respective parties hold their views. In all cases, the assumptions are direct or abridged quotes excerpted from the document cited in note 13.

16. Churchman and Mason, ops. cit. note 10.

17. Gustafson, op. cit. note 1, 1060 (our italics).
18. Subcommittee, op. cit. note 13, 571 (our italics).
19. Ibid., 579-80.
20. Ibid., 2.
21. Ibid., 25.
22. Ibid., 27.
23. Ibid., 41.
24. Ibid., 43.
25. D. Hensler, 'Perceptions of the National Science Foundation Peer Review Process: A Report on a Survey of NSF Reviewers and Applicants', Prepared for the Committee on Peer Review, National Science Board, and the Committee on Science and Technology, US House of Representatives (Washington, DC: NSF 77-33, December 1976); R. Abel, 'Applicants' and Reviewers' Assessments of the NSF Peer Review Process', International NSF draft paper (Washington, DC: NSF, November 1976).
26. S. Cole, L. Rubin and J. R. Cole, 'Peer Review and the Support of Science', *Scientific American*, Vol. 237 (October 1977), 34-41. This article is an interim report on the project, which is still in progress (see note 41, below).
27. One of us (I.I.M.) was asked to undertake such a review for the Office of Planning and Policy Analysis, National Science Foundation,.under Contract No. OM, Order No. 77-SP-0370.
28. Hensler, op. cit. note 25, 15-18, quote on 17.
29. Ibid., 23.
30. Ibid., iv. Furthermore, as Hensler explains (considering both successful *and* unsuccessful applicants), principal investigators'

> evaluations of the appropriateness of the review procedures used are related to disposition of the proposal. But even among those whose proposals were declined, half feel the procedures were appropriate. A majority of *unsuccessful* applicants feel that the decision to decline was unfair but a substantial proportion — forty-three percent — feel that [even this] decision was fair. About 84 percent of declinees who thought the decision was unfair, say they would have appealed the decision if a formal appeals process had existed. Assessments of appropriateness of procedures and fairness of the funding decision do not appear to be related to academic generation, institutional affiliation or region. However, those who have served as NSF reviewers or who have received NSF grants in the past are more likely to evaluate their most recent experience positively — even if they were turned down — than those with less successful experience dealing with NSF . . . About 73 percent of the PIs, including both grantees and declinees, would favor NSF adopting a formal appeals system. The reason for supporting such a system which is volunteered most frequently is that it would provide a remedy for mistakes and misjudgments; the leading reason for opposing it is that it will further bureaucratize and burden the review process. (ibid., v-vi).

31. As many writers have indicated, what is perceived as an innovative idea is relative to time and place in any research community. The line separating innovation from charlatanism or, in the lexicon of the exemplary 'science studies' literature, the difference between transgressions of cognitive norms and true anomalies, is fine indeed. Our view is that 'excessively' innovative ideas will so challenge the paradigmatic foundations of a research area that the innovators and their ideas

will neither gain ready access to the literature nor approval of proposals to pursue their research programme. Mainstream thought, in short, can sustain only moderate innovation. The issue, in the context of peer review, is whether the agencies which administer the system and its resources are the guardians of the mainstream or a refuge for innovators. Surely they are a little of both; hence, the issue of bias and evidence are elusive at best. For related discussion, see below, plus T. S. Kuhn, 'Second Thoughts on Paradigms', in F. Suppe (ed.), *The Structure of Scientific Theories* (Urbana, Ill.: University of Illinois Press, 1974), 459-82; M. J. Mulkay, *The Social Process of Innovation* (London: Macmillan, 1972); H. M. Collins, 'The TEA Set: Tacit Knowledge and Scientific Networks', *Science Studies*, Vol. 4 (1974), 165-86; D. E. Chubin, 'The Conceptualization of Scientific Specialties', *Sociological Quarterly*, Vol. 17 (Autumn 1976), 448-76, esp. 459-70.

32. While Hensler does not specify these 20 institutions, the following ten US universities have been identified as receiving more than a third of all federal expenditures in universities, producing about a third of all the doctorates, and providing 37 percent of the members of federal review panels in the 1960s: California, Caltech, Chicago, Columbia, Cornell, Harvard, Illinois, MIT, Michigan and Minnesota. See W. Hirsch, *Scientists in American Society* (New York: Random House, 1968), 106.

33. These findings accord with previous perceptions, as Hensler (op. cit. note 25, 50) observes that 'reviewers in general, and applicants who have also served as reviewers, are significantly less likely to perceive bias in the process than other applicants . . . . Applicants who have not been successful in obtaining NSF grants recently or in the past are most likely to think that process is biased.'

34. Hensler, op. cit. note 25, 84.

35. Ibid., 1.

36. Ibid.

37. It could well be the case that those who have either *experienced* the most 'bias' (no matter how it is defined) or who *attribute* bias to the system may be those who have either (a) 'dropped out' of the system *prior to* the sampling period or (b) never 'dropped in' in the first place. Without sampling this group, such conjectures simply cannot be evaluated; yet they cannot be dismissed out of hand. In this respect, Hensler can be criticized for a uniform lack of conjecture; she apparently feels no compulsion to explain *why* her survey generated the responses it did.

38. This quotation appears in an earlier version of Hensler's report (op. cit. note 25), dated September 1976, on p. 80.

39. G. Gordon and E. V. Morse, 'Creative Potential and Organizational Structure', *Academy of Management Journal*, Vol. 12 (1969), 37-49; I. I. Mitroff, *The Subjective Side of Science, A Philosophical Inquiry into the Psychology of the Apollo Moon Scientists* (New York: Elsevier, 1974); E. V. Morse and G. Gordon, 'Cognitive Skills: A Determinant of Scientists' Local-Cosmopolitan Orientation', *Academy of Management Journal*, Vol. 17 (1974), 709-23.

40. A prior question (underlying all of these) concerns the distribution of innovativeness in the community at large, and whether the purposive sampling of scientists for the role of peer reviewer proportionately captures this characteristic. Given the profile of reviewers developed in the works cited in note 6, one would think so. Indeed, one would think that innovators are *over*represented among reviewers; likewise, one would *hope* that innovators are overrepresented among the recipients of research funds. (As far as we can tell, there are no questions in the

Hensler study dealing directly with the topic of 'grantsmanship', although it is implied by a few of her questions.)

41. Cole et al., op. cit. note 26. The longer version of this report had yet to appear when we were preparing the final revision of this paper. It was published while the paper was in press: see S. Cole, L. Rubin and J. R. Cole, *Peer Review in the National Science Foundation: Phase One of a Study* (Washington, DC: National Academy of Sciences, 1978). We have been unable to incorporate and respond to details of the published report. An earlier draft of our paper benefitted from an unpublished preliminary version of the Cole report forwarded to I.I.M. as part of his review (see note 27). However, since it was the only public version available at the time, we have taken special pains to confine our comments here to the published interim *Scientific American* report.

42. Ibid., 36.

43. Ibid., 37-39.

44. Ibid., 37.

45. Ibid., 38.

46. Ibid., 40 (our italics).

47. Ibid.

48. Ibid., 41.

49. Ibid.

50. Ibid., 38.

51. The use of citations as a measure or indicator of the perceived importance or standing of a scientist within his or her domain of research seems non-problematic to Cole, despite the reservations expressed by some investigators regarding the validity of the measure. See D. E. Chubin and S. D. Moitra, 'Content Analysis of References: Adjunct or Alternative to Citation Counting?' *Social Studies of Science*, Vol. 5 (1975), 432-41; N. Kaplan, 'The Norms of Citation Behavior: Prologomena to the Footnote', *American Documentation*, Vol. 16 (1965), 179-84. Do numerous citations to the work of a scientist truly reflect the long-term importance and significance of the work, or merely its short-term popularity? Does one cite a work to support one's own, or for more critical and rhetorical reasons? Even if it is presumed that the reviewer has knowledge of citation performance, without taking into account the reasons why scientists cite others, and why variations in citation behaviour exist across disciplines and research areas, can one confidently use citation counts as a 'common denominator' predictor variable? On this score, Cole finds — interestingly enough — that past citations contribute little to the explained variance of the funding decision, suggesting to us that citation may be an irrelevant criterion of performance because most reviewers are *ignorant* of an applicant's citation 'performance'.

52. Cole et al., op. cit. note 26, 39.

53. This is really an expression of our dismay over Cole's decision to report only the results of quantitative analyses. Such analyses can mask individual differences manifested in anecdotal accounts, such as interviews. For example, it would be instructive, if not indispensable, to know how NSF personnel view the system of science, and the rationality of the enterprise in which they are engaged. In the study of eminent scientists who investigated the moon rocks returned by the Apollo missions (see note 39), it was found that nearly the entire sample of forty-two scoffed at, and in the most derisive of terms, the stereotypical view of the scientist and science itself as the 'open, free, unbiased exchange of pure ideas' that is so

commonly portrayed in college texts and in popular accounts of science. Surely it is important to know whether a respondent holds a conventional or a radical view of the workings of science before one can properly evaluate the respondent's attitudes towards the peer review system. Surely programme directors differ in discharging their duties. Are those harbouring a less conventional view of science more critical and sceptical of the operation of NSF peer review?

54. Gordon and Morse, op. cit. note 39.

55. Ibid., 42. In the typical situation used by Gordon to measure differentiation, a person is asked to rate ten of his most immediate colleagues, friends, associates, and so on, on a ten-point scale with respect to (a) their productivity, (b) their creativity, and (c) how easy it is to get along with the individual being rated. Low differentiators tend to rate all ten persons identically; in other words, low differentiators make use of only a small portion of the total ten-point scale, whereas high differentiators tend to make significantly more use of the whole scale. High differentiators tend to view people as different and unique; low differentiators view them as the same.

56. Cole et al., op. cit. note 26, 34.

57. To this end, we are surprised that the Cole study was not couched in terms of the Ortega hypothesis which the Coles (J. R. Cole and S. Cole, 'The Ortega Hypothesis', *Science*, Vol. 178 [27 October 1972], 368-75) investigated a few years ago. The Coles' rejection of this hypothesis (that all scientists contribute through their modest research efforts to the incremental progress of science) raises questions as to the concentration of funding support among a small portion of the research community. S. J. Turner and D. E. Chubin, in 'Another Appraisal of Ortega, the Coles and Science Policy: The Ecclesiastes Hypothesis', *Social Science Information*, Vol. 15 (1976), 657-62, argue that to equate the distributions of scientific talent, productivity, and reward is little justification for a science policy that deliberately concentrates resources among the élite that populates one tail of those distributions. Rather, they question the efficiency of a policy that would waste the talents of trained personnel without modifying the organizations that train and employ them (though we realize this is far easier said than done). To sustain the research of more scientists could calculably enhance their contributions. Yet no experiments in the democratization of research allocation have been carried out. Thus the proposition remains untested, and for us at least, the Ortega hypothesis, like the 'old boy' and 'rich get richer' hypotheses to which it is intimately related, has been gratuitously laid to rest by the Coles.

58. An 'unbiased' system would obviously not be one that randomly funds proposals; rather, it would fund primarily according to *merit*, which might be defined as innovative, feasible, relevant, or some combination thereof. In the discussion that follows we assume that a lack of bias is both desirable and attainable.

59. We purposefully use the term 'admittedly oversimplified' because the actual situation may be too complex to admit of the two exclusive categories, 'biased' or 'unbiased'. The actual state of the system may be neither biased nor unbiased, or it may be a condition of both — that is, a complicated mixture of partially 'biased' and 'unbiased' elements. Nevertheless, for the purpose of this analysis, it suffices to consider the 'idealized' cases in Table 2.

60. Notice that we do not say that Cases II and IV necessarily represent 'incorrect cases', since the difficulty in knowing the 'true' state of the actual system also

makes it difficult to know or assess 'error'; the term 'problematic' is more appropriate than such decisive terms as 'truth' or 'error', since complex social systems may not admit of such rigid or precise determinations.

61. P. Feyerabend, op. cit. note 10; A. H. Maslow, *The Psychology of Science* (New York: Harper and Row, 1966); D. C. McClelland, 'On the Dynamics of Creative Physical Scientists', in L. Hudson (ed.), *The Ecology of Human Intelligence* (Harmondsworth, Middx.: Penguin Books, 1970); Mitroff, op. cit. note 39.

62. McClelland, op. cit. note 61.

63. On this issue, Kuhn and Lakatos appear to agree. See S. S. Blume, *Toward a Political Sociology of Science* (New York: Free Press, 1974); T. S. Kuhn, 'The Essential Tension: Tradition and Innovation in Scientific Research', in C. W. Taylor and F. Barron (eds), *Scientific Creativity, Its Recognition and Development* (New York: Wiley, 1963), 341-54.

64. Mitroff, op. cit. note 39.

65. Mulkay, op. cit. note 6.

66. Scientists are not only aware of their relative position (for example, department or institution rank), but they tend to aggrandize their position relative to their perception of other departments and institutions. See T. Caplow and R. J. McGee, *The Academic Marketplace* (New York: Basic Books, 1958).

67. For a review, see Mulkay, op. cit. note 6; M. J. Mulkay, 'The Sociology of the Scientific Research Community', in I. Spiegel-Rösing and D. de S. Price (eds), *Science, Technology and Society: A Cross-Disciplinary Perspective* (Beverly Hills: Sage, 1977), 93-148; and P. Boffey, *The Brain Bank of America* (New York: McGraw-Hill, 1975). For a discussion of the institutional 'halo effect' which blurs the empirical distinction between prestige of institution and scientist's reputation (as a proxy for performance, quality of research, and so on), see H. Zuckerman, 'Stratification in American Science', in E. O. Laumann (ed.), *Social Stratification: Theory and Research for the 1970s* (Indianapolis: Bobbs-Merrill, 1970), 235-57.

68. Mitroff, op. cit. note 39.

69. See, for instance, Churchman and Feyerabend, ops. cit. note 10.

70. R. K. Merton, 'The Matthew Effect in Science', *Science*, Vol. 159 (5 January 1968), 56-63.

71. Mulkay, op. cit. note 67; Boffey, op. cit. note 67; Salomon, op. cit. note 8. See also D. K. Price, *The Scientific Estate* (Cambridge, Mass.: The Belknap Press, 1965; and D. S. Greenberg, *The Politics of Pure Science* (Washington, DC: New American Library, 1967).

72. J. R. Cole and S. Cole, *Social Stratification in Science* (Chicago: The University of Chicago Press, 1973); J. Gaston, *Originality and Competition in Science* (Chicago: The University of Chicago Press, 1973); Gaston, *The Reward System in British and American Science* (New York: Wiley-Interscience, 1978).

73. To quote an anonymous referee (for an earlier version of this paper) on decision-making behaviour:

> My hunch is that, as the uncertainty of peer evaluation increases, more and more of the elements of the dialectic are brought to bear so that in some cases, after 'objective' criteria have been used and are found not to distinguish between pairs of proposals, other more subjective and politically controversial premises are used . . . and I think for quite defensible reasons.

74. We thank an anonymous referee for reminding us to call attention to these questions.

75. This is the spirit of the proposed Science Court — the involvement of various publics in scientific decisions which are 'too important to be left to the scientists'. But another anonymous referee observed:

> Basic to the Science Court concept is the idea of scientific judgment. Its purpose is not simply 'presentation and review' of the issues, but indeed, a verdict that reflects the assessment of a 'scientific judge' . . . I have argued this out with Kantrowitz suggesting that he maintain the 'presentation and review' procedure and minimize the importance of the verdict, but he claims that would change the intention in a fundamental way.

See D. Nelkin, 'The Political Impact of Technical Expertise', *Social Studies of Science*, Vol. 5 (February 1975), 35-54, and op. cit. note 10.

76. As Dorothy Nelkin, reflecting on the recombinant-DNA debate, has recently argued: see her 'Threats and Promises: Negotiating the Control of Research', *Daedalus*, Vol. 107, No. 2 (Spring 1978), 191-209.

Ian Mitroff's latest book is *Methodological Approaches to Social Science* (Jossey-Bass, 1978) coauthored with Ralph H. Kilmann, Graduate School of Business, University of Pittsburgh. *Daryl Chubin* has just completed a book manuscript entitled *Viruses and Cancer: A Social Study of Growth and Specialization in Biomedical Research,* coauthored by Kenneth E. Studer, Department of Sociology and Anthropology, Virginia Commonwealth University. Their most recent publication is 'Knowledge and Structures of Scientific Growth: Measurement of a Cancer Problem Domain', *Scientometrics,* Vol. 1 (January 1979), 171-93.

Authors' addresses (respectively): Interdisciplinary Department of Information Science, University of Pittsburgh, Pittsburgh, Pennsylvania 15260, USA; Department of Social Sciences, Georgia Institute of Technology, Atlanta, Georgia 30332, USA.

NSF accused of misusing peer review system

In its recommendation for funding an educational project. NSF staff may have misrepresented some critical review comments

Judging by the turn of events on—and since the Jan. 28 opening day of hearings on the National Science Foundation's fiscal year 1977 budget and program by the House Science & Technology Committee and its Subcommittee on Science, Research & Technology, this could well be a long and not altogether happy year for NSF's top management.

First off, a General Accounting Office study of the accuracy and completeness of the treatment of peer reviewers' comments in a 1972 NSF staff memorandum recommending support for what is now the "Individualized Science Instructional System" (ISIS) project concludes that there has been some misrepresentation of reviewers' comments to the National Science Board—NSF's policy-making advisory group that must approve such grants—and to NSF management hierarchy. Further, an internal NSF study of the ISIS project (along with other NSF-supported curriculum development efforts) under the supervision of NSF assistant director Robert Hughes apparently failed either to detect or attach much significance to the discrepancies presented in the GAO report. That situation is more than a little disturbing to Rep. James W. Symington (D.-Mo.), who is chairman of the SR&T subcommittee and who requested the GAO study.

Rep. John B. Conlan (R.-Ariz.), perhaps the fiercest critic in the House of NSF peer review and of support of developing social science curriculum programs, says that the GAO findings indicate that a "major scandal" has developed within NSF. (He also finds the Ford Administration's proposed 20% boost in basic research funds at NSF as "unreasonable in a tight budget year.") And the SR&T subcommittee, in addition to examining NSF's budget request in detail, plans to "take a look at the role of science in society along with some of the fundamental assumptions and promises that underlie federal support for science."

SR&T chairman Symington, in opening the hearings, observed that, "There are other serious matters to be considered. Increased demands for 'short-term payoffs' from government investment in science are increasing the emphasis on applied research to attack unfolding problems related to the environment, energy, transportation, food, health care, and economic development possibly creating imbalances in research activities. The development and implementation of science curricula, particularly social science projects, by federal agencies have called for a serious re-examination of such projects in relationship to our philosophy of public education."

Symington cites controversy surrounding NSF's funding development of "Man: A Course of Study" (MACOS), an anthropology course for fifth graders. And of more immediate concern, he says, the GAO report on the $3.35 million ISIS project "revealed a number of management irregularities" which have come under extensive review.

In essence, GAO concludes that the 1972 NSF staff memorandum accurately represented summary comments of 11 peer reviewers on five general areas and gave a more detailed account of another comment. "However . . . thirty-three excerpts expressing only favorable comments from peer reviewers were quoted in the memorandum. . . . Twenty-eight of the excerpts appeared to accurately represent the reviewers' thoughts, but the other five could be considered to not accurately reflect the entire thought of the passages from which they were taken. In addition, the memorandum stated that all reviewers recommended funding. We believe that the rationale for this statement was not fully justified with respect to three of the 11 reviewers."

NSF critic Conlan published selected conclusions of the GAO report and his interpretation of them in the Jan. 28 *Congressional Record*. And in a letter to NSF director H. Guyford Stever requesting an end to ISIS funding, and chastizing Stever for allowing such management practices to occur, Conlan levels a blast at the internal NSF review conducted under Dr. Hughes.

"It is unbelievable to me that your National Science Foundation internal curriculum review team headed by Dr. Robert Hughes did not discover this misrepresentation of peer reviews on the ISIS project, and acknowledge such findings fully and forthrightly. . . . Neither you nor Dr. Hughes acknowledged the great seriousness of this situation . . . . Instead, you attempted to cast aspersions on my findings now verified by GAO. Moreover, Dr. Hughes made further false statements before the subcommittee about the ISIS proposal having been 'seriously revised' to accommodate peer reviewer criticisms and concerns before it was sent to the National Science Board for funding—which your staff knew was not the case."

In a letter dated Jan. 15 to NSF director Stever, SR&T chairman Symington says that "based on my examination of the GAO report and preliminary subcommittee staff analysis, I am forced to conclude that representations given to the subcommittee by foundation staff concerning the evaluation of ISIS and the report of the Science Curriculum Review Team (May 1975) are not entirely accurate . . . . I am greatly concerned about the findings of the GAO report and will want to discuss with you steps to preclude the kinds of mistakes that apparently took place in the management of this project."

None of this is causing much cheer at NSF's Washington headquarters. In a letter responding to Symington, Stever says, "The conclusions in the report and your letter are indeed most disturbing, so much so that I have conferred in a special executive session with NSB and with other Presidential appointees at the foundation." Further, Stever says of GAO's findings, "I agree that, based on the written record, the statement which appeared in the Sept. 5, 1972, memorandum to the effect that all reviewers recommended funding is wrong and should never have been made. Moreover, I am disturbed that the statement is repeated in the Appendix which serves as backup of the report by the Science Curriculum Review Team. We regret these mistakes. While we believe that the judgment for funding probably would still have been positive, there is no excuse for the misstatement." Stever then lists steps he has taken to substantially reduce the possibility of such errors in the future.

And in testimony before Symington's SR&T subcommittee, Stever again voiced great concern at GAO's findings. He indicates that he has discovered some other problems with the NSF review team study, and says that he, together with NSF deputy director Richard C. Atkinson, personally would look into it. He says he will comment further after he and Atkinson complete their investigation and that SR&T chairman Symington will be provided complete information of their findings.

Fred H. Zerkel, C&EN Washington

NEWS AND COMMENT

National Science Foundation: Criticism from Conlan, GAO

The scene on 10 February could have been a flashback to last summer when Representative John B. Conlan (R-Ariz.) accused National Science Foundation (NSF) officials of misrepresenting the views of peer reviewers. The difference at the 10 February hearings before a House Committee on Science and Technology subcommittee was that in the interim a General Accounting Office (GAO) report had substantiated Conlan's main contention. Also, NSF director H. Guyford Stever had written subcommittee chairman James Symington (D-Mo.) that "we regret these mistakes" and had pledged more strenuous efforts to remedy them.

Conlan testified on 10 February at an authorization hearing devoted to a discussion of NSF's science education program. He charged that there was a "scandal of deceit and corruption of the NSF grant award process" in the foundation's curriculum improvement program. The essence of Conlan's charge was that NSF staff members had misrepresented peer reviewer's comments in a summary of reviews provided to the National Science Board. The NSB is responsible for giving formal approval to major NSF programs. Conlan also accused NSF officials of a "cover-up" and urged that Congress suspend funding of NSF curriculum development activities for a year.

Although Stever and other top NSF officials were on hand for the session, they were not grilled on the Conlan accusations. An investigation by committee staff of the issues raised by Conlan and by the GAO report is now in progress and Stever has promised Symington that he will conduct his own thorough examination of the situation, so an inquest has been at least delayed.

Conlan's ire was concentrated on an NSF-supported interdisciplinary high school science course called Individualized Science Instructional Systems (ISIS) currently being developed at Florida State University. Conlan emerged about a year ago as a resolute critic of NSF curriculum programs (*Science*, 2 May 1975), when he objected not only to content but to contracting and course-implementation practices associated with an elementary school behavioral science course titled Man: A Course of Study (MACOS). In the peer review hearings last summer Conlan shifted his main fire to ISIS and insisted that evaluations of outside reviewers had been misrepresented. At the time, he was denied access to the reviewers' original comments on the grounds of NSF policies protecting the confidentiality of verbatim peer review documents. (Under the system of mail peer review prevailing at NSF, reviewers have been asked individually for their opinions and seldom learned whether their suggestions influenced NSF decisions on programs, were brought to the attention of those involved in projects, or were incorporated into the projects.) Subsequently, the material Conlan had sought as background to a 5 September 1972 staff memorandum recommending support of the ISIS project became available as a result of the GAO study, requested by Symington in October.

The GAO report is not a wholesale condemnation of the ISIS transaction. The report notes, for instance, that the staff memorandum in question was only one part of the package of documentation on which the National Science Board based its decision on ISIS. But a major conclusion of the GAO investigators is that "In our opinion, however, the memorandum recommending support of the Florida State University proposal was not a totally complete and accurate representation of peer reviewers' comments."

The cover letter accompanying the report says that, to the extent that the comments of 11 reviewers were summarized in the memo, "they are accurately represented. However, about 45 comments by 9 of the 11 peer reviewers are not explicitly dealt with in the memorandum, nor was documentation on file to indicate their disposition."

The authors of the report itself concluded, "We do not agree with the Foundation's rationale in stating that all reviewers recommend funding for the proposal. We question the Foundation's approach which assumes that, once a reviewer's criticisms are considered to be disposed of by the program staff, the Foundation can unilaterally consider that the reviewer, in effect, recommends funding for the proposal."

By comparing the original peer reviews with the excerpts used in the summary the GAO report writers were able to find several examples of highly selective editing.

For example, one comment which appeared as ". . . I am sympathetic to many of the objectives that lie behind this proposal" produces a different impression when quoted in the context of the complete passage.

> While *I am sympathetic to many of the objectives that lie behind this proposal*, I find myself drawing back from certain essential aspects of it. My objections are partly philosophical and partly pragmatic. Because of these, I count it a serious weakness of the proposal that no discussion of some of these matters was included in the presentation. [Italics added.]

In the peer review hearings last summer, Conlan drew attention to the use by NSF of an excerpt from the comments in 1972 by one peer reviewer he identified as Philip Morrison of the Massachusetts Institute of Technology. Conlan argued that the excerpt misrepresented Morrison's views. Morrison appeared at the hearings, but the original comments in complete form were not available and the discussion was inconclusive. In his statement of 10 February, Conlan was able to reproduce both the excerpt used by NSF in 1974 in seeking renewal of the ISIS funding and the text of the original comment.

In the staff summary the Morrison quote was: "The general scheme proposed—the system foreseen—has natural and conspicuous appeal. The flexibility and freedom of use are clear advantages. . . . The personnel are excellent. . . . The idea is good. . . ."

In the original form the comment was as follows:

> *The general scheme proposed—the system foreseen—has natural and conspicuous appeal. The flexibility and freedom of use are clear advantages.* So much is clear. But when I read the detailed descriptions—which seem to me very preliminary indeed for so grand a scheme—I grew much less enthusiastic. . . . It seems to me this whole grand scheme goes too far ahead. *The personnel and advisors are excellent*, but they have done very little work to date. I would grant enough to assemble a team able to produce a sample of minicourse topics and a few prototypes. Fifty topic headings, over a wide range of disciplines and levels, and two prototype courses would seem to me a minimum basis for the grant beyond Phase I. Nor would I approve the remaining phases without a strong sample of six months work at least on the Phase I issues. They determine all: *the idea is good*, but its execution determines its utility. [Italics added.]

With respect to the expurgation of the original comments, Conlan was in a position to say, "I told you so." He went on to discuss what he regards as a cover-up on ISIS, stressing that a special NSF Curriculum Review Team formed last spring to look into charges arising from the MACOS furor completely missed the anomalies in the handling of the staff memorandum on ISIS.

At this point Conlan adopted the tone of a prosecuting attorney and, in these excerpts from his statement, made what must be regarded as his most serious charges to date:

> As the General Accounting Office report on ISIS shows, and my quotations of ISIS reviews here today, misreading the file was not the problem. The peer reviews were clearly manipulated, deceitfully edited, and misrepresented to the National Science Board.
>
> As of last Friday afternoon, February 6, Dr. Joel Snow, director of the Foundation's Office of Planning and Resources Management and the executive secretary of the NSF Science Curriculum Review Team headed by Dr. Robert Hughes, had requested "reassignment." Those close to NSF understand the meaning of that action.
>
> NSF has apparently found a scapegoat to take whatever blame is to come from falsifications and abuses uncovered by the GAO, my own continuing investigation of NSF activities, and any continued examination by this Committee.
>
> It is inconceivable that Dr. Snow is solely responsible for all the careful scheming, the evasive report writing, and the stonewalling that has gone on at NSF over the past 12 months.
>
> Surely Dr. Robert Hughes must share the blame. He told this Subcommittee last July 23 that the only problem he found in the handling of ISIS could be attributed to what he called "administrative slippages."
>
> But I have concrete documentary evidence that a member of Dr. Hughes' own NSF Science Curriculum Review Team discovered and accurately reported the deceitful misuse and manipulation of ISIS peer reviews to get National Science Board approval of the project's multi-million dollar funding.
>
> This NSF staff official's honest report was laundered by team leaders and the true facts covered up in a skillful job of evasive writing—obviously with Dr. Hughes' knowledge and approval.
>
> I have reason to believe that higher NSF officials are aware of this cover-up, but have not yet acknowledged the full gravity of this matter to Members of this Committee or the scientific community.

At the hearing, Conlan declined to identify his sources of information or to elaborate on his charges but said he would be glad to assist Symington and the committee staff with their investigation.

NSF officials say that Snow's reassignment came at his own request. Snow acted as executive secretary of the curriculum review study which is now under scrutiny from the Hill. In his post as director of the office of planning and resources management he was, in effect, playing a liaison role with Congress in its investigation of management practices, and this placed him in an awkward position. Snow and Stever came to the conclusion that it would be better if Snow moved from the planning job. An NSF front office spokesman insists that no examples of mismanagement have been found involving Snow or other NSF officials. According to the same sources, Snow is not leaving the foundation but has not yet been given a new permanent assignment.

Although much of Conlan's testimony dealt with ISIS, he also elaborated on broader themes he introduced last year in criticizing the NSF science education program. He said that funding of the science education program should be suspended "until we have been completely reassured next year that the total integrity of NSF's management and grant award process has been reestablished," but he is clearly hoping that NSF's role in education R & D will be reassessed and modified.

In his testimony Conlan quoted extensively from correspondence with commercial textbook publishers who criticized NSF-supported curriculum revision courses on a number of grounds. Included were charges that many are too expensive for school districts to afford in present economic circumstances and are based on innovations which are too extreme to be successfully incorporated in the structure of most educational systems.

In an exchange of views with subcommittee members, Conlan declared himself to be generally in sympathy with those who would sharply limit the federal role in curriculum matters. Conlan said he feels that NSF support in developing a course endows that course with a prestige which gives it a competitive edge when school districts consider various alternatives. He objected even more strenuously to NSF spending on "implementation" activities which provide information on new courses and train teachers to employ them. He is irked by what he calls "career curriculum innovators" who appear to have an inside track with NSF. And he is most disturbed that the program was "getting into value areas." In concluding his testimony Conlan said, in response to a question about his view of NSF's role in education research, "there may be an argument for developing innovative concepts," but the agency should steer clear of "the development of pilot courses" and implementation activities.

There was little show of sympathy for this narrow view of NSF's role among the three subcommittee members who attended the hearing—Symington; Representative Charles A. Mosher (R-Ohio), the ranking minority member of the full committee; and Representative Don Fuqua (D-Fla.). Symington had said earlier in a statement accompanying release of the GAO report that "the Subcommittee has long been a supporter of National Science Foundation programs to improve education in science for the Nation's children and young adults. The Subcommittee will continue its support." Fuqua took exception to Conlan's choice of language in using words such as "deceit" and "corruption" to describe actions which the GAO report characterizes much less vividly.

It appears that the subcommittee's first concern is to improve NSF program management. Even Conlan conceded that some constructive steps had been taken. These steps, according to a policy statement by Stever, include (i) establishment of award review boards in all grant awarding directorates of the NSF, including the education directorate (these boards are made up of foundation officials not directly involved in the programs they are reviewing); (ii) making available verbatim reviewers' comments at NSB programs committee meetings when proposals are up for recommendation to the full board (this should preclude a repetition of the ISIS controversy); (iii) as announced earlier (*Science*, 11 July 1975), making available verbatim peer reviews to a project's principal investigators on request, with reviewer's identities removed.

Stever also noted that "two of our most capable staff members" have been assigned to head the education directorate. These are Harvey Averch, acting assistant director for education, who appeared at the hearing to present the budget request of the science directorate in uneventful testimony, and Jack T. Sanderson, Averch's deputy until he returned to the planning office as acting director to replace the newly departed Snow.

Averch and his colleagues have been conducting an evaluation of 19 current projects in his directorate using outside experts in the exercise. The results of the study are to be communicated to the National Science Board, and action by the board may well indicate to what extent the board intends to rethink NSF policy on its education role.

There is little firm indication of whether Congress will be disposed to chastise NSF sternly for the ISIS incident. Except for the travail of the education directorate, NSF appears to be doing well in the budget authorization hearings. Conlan himself praises other parts of NSF's operations but concluded his criticism of "mismanagement" in the education program by asking, "How extensive is it? That's the question."

So far, Conlan's colleagues tend to congratulate him for his "provocative" contributions but indicate that they think he is overstating the problem. There is no question, however, that Conlan has shaken up NSF and that the tremors continue.—JOHN WALSH

James W. Symington
with Thomas R. Kramer

Does Peer Review Work?

Hearings held in July 1975 before a House Subcommittee found fundamental soundness in NSF peer review evaluation systems

Can peer review lead to the effective advance of science? From its examination of that procedure at the National Science Foundation (NSF), the House Subcommittee on Science, Research, and Technology is satisfied that applications are being evaluated in a basically sound manner; moreover, the NSF's peer review system has survived the test of time and appears to enjoy the support of the scientific community. The system should not, however, be regarded as an unchangeable structure: it can be improved immediately in several ways, and further study may indicate additional improvements (*1*).

Why peer review?

For six days of oversight hearings in July 1975, the Subcommittee, under my chairmanship, addressed itself to the question "Once monies have been appropriated to the NSF and the division of those monies among disciplines and purposes has been agreed upon, how does the NSF go about making individual awards?" Several factors led to the selection of peer review as the topic of the hearings, probably the strongest of which were the Bauman Amendment and the ridicule to which scientific grants have been subjected in Congress.

The Bauman Amendment to the NSF authorization bill for FY 1976 passed in the House but was later killed in the House and Senate conference agreement on the bill. The amendment would have required the NSF each month to send a list of proposed grants to Congress for a 30-day layover, during which time either the House or the Senate could stop any listed grant by a simple resolution disapproving of it. Making fun of grants by title has become almost a tradition in the House—one of its less laudable traditions, to be sure. Rep. H. R. Gross established it, and Rep. Robert Bauman of Maryland, author of the amendment, has cheerfully carried it on. In the Senate, scientific grants frequently receive Sen. Proxmire's Golden Fleece award—an award distinctly of negative significance.

Peer review as a hearings topic was also indicated by the trend of recent years toward openness in government. It was appropriate for the Subcommittee to determine how compatible openness might be with the effective evaluation of applications.

A final factor leading to the selection of peer review was the increase in competition for NSF funds. It was believed that an increase in the numbers of disappointed applicants might lead to an increase in complaints about the system. The NSF wanted Congress to understand its procedures before this happened.

Witnesses

The 23 witnesses I asked to testify at the hearings included two Congressmen who had requested to appear, four principal NSF witnesses, ten persons selected by the Subcommittee staff, and seven persons recommended by Rep. Conlan of Arizona (Table 1). Philip Handler, President of the National Academy of Sciences, submitted important written testimony.

The two Congressmen, Rep. Bauman and Rep. Conlan, both had been critical of the NSF before the hearings, and Rep. Conlan launched the hearings with the following characterization of the NSF's peer review system:

> It is an incestuous "buddy system" that frequently stifles new ideas and scientific breakthroughs, while carving up the multimillion dollar Federal research and education pie in a monopoly game of grantsmanship.

The four principal NSF witnesses, on the other hand, believed the NSF procedures to be fundamentally sound and devoted much of their testimony to describing how decisions are made and what the resulting distribution of funds and reviewers has been. The ten expert witnesses selected by the Subcommittee staff were also generally supportive of the NSF, but they went on to suggest improvements.

The Subcommittee staff and I wanted to be sure that responsible scientists with negative views on the NSF were fully heard, and Rep. Conlan's wit-

James W. Symington, Democrat, of Clayton, MO, received his B.A. from Yale in 1950 and his LL.B. from Columbia Law School in 1954. He has been an assistant city counselor in St. Louis, an associate member of law firms in St. Louis and Washington, deputy director of the Food for Peace program, administrative assistant to Attorney General Robert F. Kennedy, director of the President's Committee on Juvenile Delinquency, consultant to the President's Commission on Law Enforcement and Administration of Justice, and chief of protocol in the Department of State. Elected to the 91st Congress in 1968 and reelected to the 92nd, 93rd, and 94th Congress, Representative Symington has served as a member of several committees and subcommittees and was chairman of the Subcommittee on Science, Research, and Technology. He is the author of The Stately Game *(Macmillan, 1971). Address: 3019 44th Street NW, Washington, DC 20016.*

Table 1 **Witnesses**, representing different points of view, who appeared at the hearings on peer review, before the House Subcommittee on Science, Research, and Technology

Congressmen	John Conlan, *Arizona* Robert Bauman, *Maryland*
National Science Foundation	H. Guyford Stever, *Director, NSF* Norman Hackerman, *Chairman, National Science Board* Richard Atkinson, *Deputy Director, NSF* Donald Rice, *member, National Science Board, and President, RAND Corporation*
Selected by Subcommittee staff	
Federal agencies	John Sherman, *former acting director, National Institutes of Health* William Raney, *Chief Scientist, Office of Naval Research*
Academic institutions	Charles Kidd, *Executive Secretary, Association of American Universities* James Powell, *Provost, Oberlin College*
Scientific societies	William Carey, *Executive Director, American Association for the Advancement of Science*
Engineering	*Donald Marlowe, *Executive Director, American Society for Engineering Education* *Daniel Drucker, *dean, University of Illinois* *Vincent Haneman, *dean, Auburn University*
Working scientists, NSF reviewers	Raymond Bowers, *Director, Program on Science, Technology, and Society, Cornell University* Philip Morrison, *professor, Massachusetts Institute of Technology*
Recommended by Representative Conlan	
Materials research	Doris Wilsdorf, *professor, University of Virginia, and member, NSF Advisory Committee on Metallurgy and Materials* Heinz Wilsdorf, *department chairman, University of Virginia* Rustum Roy, *department chairman, Pennsylvania State University, and member, NSF Advisory Committee on Metallurgy and Materials*
Biomedicine innovation	Gilbert Ling, *department director, Pennsylvania Hospital* Carlton Hazlewood, *associate professor, Baylor College of Medicine* Freeman Cope, *biophysicist, U.S. Naval Air Development Center Biochemical Laboratory*
Curriculum development project monitoring	Elmer Seevers, *science curriculum coordinator, Biomedical Interdisciplinary Curriculum Project*

*Drs. Marlowe, Drucker, and Haneman asked to testify.

nesses filled this need well: all seven were well-qualified scientists—several were leaders in their respective fields—and all were highly critical of one aspect or another of the NSF's operation.

As a result of the charges made by Congressman Conlan and his witnesses, the hearings were conducted in an atmosphere of perceptibly high tension. This was not without its beneficial effects. Issues were explored in more depth than they might otherwise have been, and the attention of those present rarely wandered. The hearings, which had been scheduled originally for a total of 12 hours, extended to nearly twice that.

Issues and Subcommittee views

The label "peer review system" describes a broad spectrum of processes for awarding grants. In particular, the label does not indicate who makes the final decision whether or not to fund a given proposal: it may be the group of peers, or it may be the person or organization controlling the money. The NSF, in fact, employs a wide variety of evaluation procedures, tailored to fit differing types of programs. The procedures normally include reliance on staff judgment and evaluation by an individual peer reviewer and/or an assembled panel, but they may also include site visits, consultation with other funding agencies, or action by the National Science Board (the NSF's policy-making body).

Four interrelated questions encompass the important issues of peer review at the NSF. Does the NSF support high-quality research? Are all the people involved in the decision-making processes performing the proper functions, and are they performing them well? How open and fair is the system of granting awards? Does a desirable distribution of monies result from the decision-making processes?

The full Subcommittee *Peer Review Report* (*1*) was released on March 8, 1976. It presents 22 findings and recommendations grouped according to those questions. The two principal findings and recommendations of the Report and a discussion of how open and fair (third of the four questions) follow in this article. Neither the Report nor this article deals in depth with two areas of controversy that arose at the hearings, namely, the Individualized Science Instructional System (ISIS) and materials research funding. These have been well publicized elsewhere. The Subcommittee's two principal findings and recommendations were:

1. *Finding:* Procedures relying on peer review for evaluating applications for scientific research awards can lead to the effective advance of scientific knowledge. The NSF's peer review evaluation systems appear basically sound. *Recommendation:* The NSF should continue to use some form of peer review. Congress should not require major changes at this time in the methods used by the Foundation for proposal evaluation.

2. *Finding:* Certain problems exist with the NSF's peer review evaluation processes and related operations. Substantial changes in the operation

of the NSF's peer review systems have been proposed which possibly could lead to improvements in the systems' effectiveness for consistently selecting the best proposals, and in the acceptability of the systems to potential applicants, the scientific community, and Congress. *Recommendation:* Some corrective actions should be taken immediately and certain proposals for substantial changes should be thoroughly investigated and instituted if judged worthy.

In the Subcommittee's judgment, the most important single consideration is whether the NSF's awards lead to the advancement of science in the most effective way possible. Using peer review procedures, the NSF has indeed fostered advances in basic science over the past 25 years. Beyond this empirical observation, the rationale for believing that peer review of proposed work tends to advance science is that experts in a scientific field are best able to judge several important factors, e.g. design and importance of proposed work, and past performance of the proposer.

It is widely agreed that the NSF staff could not perform as well without the benefit of peer review, and it appears that the potential for staff abuse is greater in systems lacking peer review. Additionally, peer review procedures are administrable, the concept of peer review is accepted by the scientific community, and the idea of evaluation by equals is considered fair.

The NSF evaluates over 20,000 proposals each year, and there are, undeniably, occasional failures and abuses of the system. The Subcommittee Report did not deal with isolated problems but did discuss broad areas in which improvement might be made (*1*). In the Report's summary, the Subcommittee recommended six areas for immediate NSF action and eight tasks for the National Science Board.

How open? How fair?

The most thoroughly discussed issue of the hearings was: How far can openness of the decision-making process coexist with the effective evaluation of award applications by the NSF? There was overwhelming agreement that openness per se is desirable and that applications ought to be evaluated effectively. The extent to which these two desiderata are in conflict and which should be favored in the conflict were hotly debated.

One set of arguments concerned the extent to which the comments and identity of reviewers should be known. No one supported the extreme position that applicants should not know anything about who the reviewers were and what they said, but until January 1, 1976, NSF policy was that applicants should not know the identity of reviewers and should receive, on request, only paraphrased reviewer comments. The National Science Board changed this policy in June 1975, and since January 1, 1976, NSF policy has been that applicants should still not know the identity of reviewers but should receive verbatim reviewer comments on request.

Witnesses who agreed with either the past or present NSF policy included Raymond Bowers, William Carey, Daniel Drucker, Vincent Haneman, and Donald Marlowe. Rep. John Conlan and Heinz Wilsdorf, however, believed that applicants should receive signed verbatim peer reviews on request, while Philip Morrison, James Powell, and Donald Rice held opinions intermediate between this position and present NSF policy. No one claimed that both applicants and the public at large should have access to signed verbatim peer reviews.

The National Science Board has also been deliberating the issue of confidentiality. The arguments it has considered encompass all the major arguments made at the peer review hearings and were elegantly summarized by Donald Rice (*3*).

With confidentiality, it is argued, reviewers will be more candid about all aspects of the proposal and about the qualifications of the proposer to accomplish the work. Without confidentiality, many potential reviewers would refuse to participate, and those that did participate would be reluctant to comment critically on proposals by others who could adversely affect their own careers. Personality clashes would arise, and program managers might have to take on the role of arbitrators between proposers and reviewers. Program managers would also find themselves spending more and more time defending their decisions, and the identification of reviewers would increase the opportunity for political pressures to intrude on the system.

Others argue that regardless of whether the system is open or closed, qualified reviewers will continue to participate in the interests of furthering the best quality science and can be relied upon to be candid and straightforward in their evaluation of proposals. They think openness would result in a more focused and effective evaluation; superficial or personality-based comments would diminish. Confidentiality makes the system unnecessarily difficult to defend from charges of internal bias, old-boy networks, favoritism, etc., and a new open system could better demonstrate its impartiality and effectiveness and therefore would better stand public scrutiny and attain a higher level of credibility.

Witnesses generally believed that the NSF's new policy will be an improvement over the old, but the kind of information presented in support of this belief or in support of applicants receiving verbatim peer reviews was largely anecdotal, personal opinion, or a small opinion survey. It is unfortunate that information concerning the important question of openness is so poorly developed. Better information might result from comprehensive surveys, experimentation—preferably using control groups—and analytical comparison of other, similar systems.

It is worth noting that in current practice the National Institutes of Health (NIH) study section system and the NSF ad hoc reviewing system share the principle that reviewers' names should be separated from their opinions in material provided to applicants. The NSF provides verbatim comments but withholds reviewers' names, whereas the NIH makes names available but provides only paraphrased comments unattached to any given name.

There is a degree of realism in current practice, which is the recognition that while it is a fairly simple matter to maintain the names of mail reviewers in confidence, it is impossible to keep secret the names of members of a standing committee who travel to Washington several times annually—

so to be equitable they should be published so that the entire community will be informed, not just a favored part of it.

The National Science Board has decided to make available the names of peer reviewers in an aggregated form. All witnesses who mentioned it felt that this was a step in the right direction, but several witnesses also felt that the reviewer list should be broken down to identify the reviewers in particular programs. The NSF has prepared a list of the reviewers drawn upon in 1974, and the list has been printed by the Subcommittee (2), but the preparation of future lists is under continuing debate. One undisputed contribution of the list, however, will be the public recognition of those scientists who have helped to advance science by reviewing applications submitted to the NSF.

Intertwined with the issue of confidentiality is the question of what other information should be provided to applicants. Excluding the matters of reviewers' names and verbatim comments, however, there was general agreement among witnesses who addressed the question that proposers should have the right to know the rationale behind the decisions taken on their applications and should be able to get it in writing. Moreover, proposers should be informed of their right to this feedback as a matter of course. It was not clear from the testimony whether the NSF in the past had adequately notified applicants of what information they could obtain on request.

Though one of the more important aspects of feedback, the question of what stage in the evaluation process applicants should be able to get verbatim reviewer comment has not yet been settled. Reviews might be provided upon request before a decision has been reached on an application, so that an applicant could provide rebuttal or alter the proposal. Alternatively, reviews might be withheld until a decision has been reached, so that grant evaluation does not become an adversary procedure. The Subcommittee had four findings and recommendations concerning openness and feedback:

1. *Finding:* Openness of NSF operations and the effective evaluation of proposals by the NSF are both desirable. *Recommendation:* The greatest degree of openness of the NSF's award decision-making process consonant with effective proposal evaluation and reasonable efficiency should be achieved.

2. *Finding:* In keeping with the policy of maximum feasible openness, the recent decision of the National Science Board to publish a list of reviewers used by the NSF is laudable. *Recommendation:* The National Science Board should study the effects of publication of the list and consider whether publication of the list in a less aggregated form might be desirable.

3. *Finding:* Information concerning the critical problem of effects on the NSF's peer review system of the level of confidentiality in which peer reviewers' names and verbatim comments are held is inadequate for making sound judgments on what that level of confidentiality should be. *Recommendation:* The National Science Board should collect further information concerning effects on the peer review system of the level of confidentiality in which peer reviewers' names and verbatim comments are held. The Board should report the information and any conclusions that may be drawn from it to Congress. Further changes in the level of confidentiality of the NSF's peer review system should be made slowly if at all.

4. *Finding:* Each applicant for an NSF award should have easy access to a description of the rationale for the decision taken on the application and a description of the decision-making process itself. *Recommendation:* The NSF should include in each letter announcing a funding decision to an applicant either a statement of the methods and rationale of the decision or a statement that such information will be provided on request.

Further surveys

The publication of the Report and of a full record of the hearings (3) has been followed up in several ways. With Norman Hackerman, Chairman of the National Science Board, I cosponsored a sample survey of NSF grant applicants and reviewers to gather facts and opinions about the peer review processes. I also asked the General Accounting Office—an arm of Congress—to perform two studies of NSF performance and to audit the peer review survey.

Preliminary data from the survey already make it clear that an applicant's satisfaction with the system depends strongly upon whether or not he or she has been successful. Reviewers and applicants—successful and unsuccessful alike—approved by a large margin the NSF's providing unsigned verbatim reviews to applicants but disapproved, also by a large margin, providing signed verbatim reviews.

Finally, I asked for a response from the NSF to the recommendations of the *Peer Review Report*. An ad hoc committee of the National Science Board was established to provide that response, and their report is expected by the time the Committee on Science and Technology acts on the NSF's authorization bill for fiscal year 1978.

References

1. U.S. House of Representatives Committee on Science and Technology. 1976. *National Science Foundation Peer Review*, vol. 1. U.S. Government Printing Office, stock no. 052-070-03234-1.
2. Ibid., vol. 2, stock no. 052-070-03235-9.
3. U.S. House of Representatives Committee on Science and Technology. 1975. *National Science Foundation Peer Review: Special Oversight Hearings*, no. 32. U.S. Government Printing Office.

NAS president lauds NSF peer review system

Academy committee's study of system for evaluating research grant applications refutes charges of bias favoring older, established scientists

The National Science Foundation's peer review system for evaluating research grant applications is more often the target of criticism than praise. But late last month it got rave reviews from Dr. Philip Handler, president of the National Academy of Sciences. According to Handler, NSF's peer review system "is eminently fair and equitable and shows no sign of systematic bias." And he has hard data to back up his statement.

NAS's Committee on Science & Public Policy recently completed an investigation, with NSF's cooperation, of about 1200 decisions concerning applications for research grant support made in fiscal 1975 to the managers of 10 of 80 disciplinary basic research grant programs at NSF. Of these 1200 decisions, 250 specific proposals were examined in depth, including perusal of all of the comments of peer reviewers and of all of the correspondence between the applicant and the program manager, as well as a study of the proposal itself. In addition, a series of 70 in-depth interviews were conducted with scientists involved in all levels of the peer review system.

Although final results of the study will not be out until this spring, Handler appeared before the House Subcommittee on Science Research & Technology, which is holding hearings on NSF's fiscal 1978 budget proposals, to outline preliminary results of the study. Handler's generally laudatory comments are in direct contrast to criticisms that have been leveled at the peer review system in the past two years by Sen. William A. Proxmire (D.-Wis.) and John T. Conlan, a former Texas Representative. According to these and other critics, the peer review system was actually an "old boys network" that looked kindly on research proposals from established scientists, working at the well-known and prestigious universities, while leaving younger researchers and those from less well-known institutions out in the cold. There also were charges that NSF program managers manipulated their choices of reviewers to get desired results and/or ignored advice received from peer reviewers, making decisions based on their own views.

Charges such as these prompted the NAS investigation, Handler says, "in an attempt to learn whether the system has been functioning in an even-handed, equitable fashion, whether all applicants get a genuinely fair shake, or whether there are inbuilt systematic biases that result in undesirable systematic discrimination."

As part of NAS's study, the interactions between a total of 3800 pairs of external reviewers and specific applicants were examined. The results, Handler told the subcommittee, show that overall, applicants from the academically higher-ranked institutions were reviewed slightly more favorably than those from medium-ranked institutions, and they more favorably than those from low-ranked institutions. But, he adds, "the spread among them is not nearly as great as one might have imagined." And the rankings given were independent of the home base of the reviewers. In fact, reviewers from low-ranked or nonacademic institutions tended to be somewhat more generous to applicants from high-ranked institutions and harsher to those from low-ranked institutions than were reviewers who were from high-ranked institutions.

In addition, NAS found that the academic ranking of the department in which the grant applicant received graduate training or is currently employed, type of institution in which he is employed, years since obtaining a Ph.D., and the academic rank of the applicant all have only trifling influence on the outcome of the review. All other things being equal, Handler says, assistant professors fare quite as well as full professors. And young investigators fare almost as well as older investigators, with 54% of the applicants who received Ph.D. degrees before 1970 and 46% of those who received Ph.D.'s after 1970 getting grants. In fact, he points out, only two characteristics of the applicants show significant positive correlation—the number of citations to an applicant's published work and the number of years since 1970 during which the applicant was funded by NSF. But even taken together these account for less than 10% of the variance in the ratings.

These findings are "very surprising," Handler says, adding, "We 'old boys' did believe that previous research productivity and place in the social structure of science must have a significant influence on the outcome of applications for research support, all other things being equal." But in fact what NAS found was that "it is the research proposal—and almost only that—which is appraised by external reviewers." And there is "simply no evidence to support the hypothesis that peer review systematically operates as a network of old boys."

As to whether NSF program officers really are guided by the results of peer review, Handler says they are quite considerably, but not totally. NAS found that among applicants who received comparatively high mean ratings from reviewers, 92% were awarded grants. Among applicants who received low mean ratings, only 10% received grants, and among those who received mean ratings in the middle of the range, about half received grants. NAS data do indicate, he adds, that each program manager makes a small number of decisions based on his own criteria.

Handler ended his testimony to the subcommittee by saying, "It all seems extraordinarily equitable. The system is, in fact, so equitable as to be slightly worrisome. The very low correlation between grant awards and past scientific performance simply doesn't fit the notions most of us hold concerning the development of science. [And] it seems ironic to think that in some small degree the ability to construct a good proposal may be more influential in affecting the fate of that proposal than may be the proven capability of a scientist, as demonstrated by past performance."

Nevertheless, Handler did have one suggestion for improving the peer review process at NSF. He recommends that the agency make much more use of periodically convened panels of reviewers, which he contends offer a number of unique advantages. For example, the personal interaction among a group of perhaps a dozen scientific experts who have all studied the proposals in advance would minimize the possibility of occasional bias by individual reviewers, since unacceptable influences are suppressed or neutralized in the setting of a panel meeting. It also would serve to bring to the attention of all the reviewers critical information that is perhaps known only to one of them. And perhaps most importantly, he says, by debating the merits of individual proposals at the meeting, the panel gets a better sense of their relative intrinsic merit, and this permits a relative ordering which is of use to the agency. ◻

NSF peer review system back on hot plate

GAO charges that NSF files lack documentation justifying choice of peer reviewers and how grant proposals are considered and resolved

Some scientists may have trouble remembering the controversy that raged over the National Science Foundation's peer review system a few years ago. At its height in 1975, the House even passed legislation making all NSF grant awards subject to Congressional approval.

Since then the heat of the controversy has apparently cooled down to a very low simmer. However, Congress' General Accounting Office has not forgotten. Since September 1976, it has been following up on a request from the House Committee on Science & Technology that it review a sampling of NSF's disposal of grant applications to determine if the new policies and procedures instituted over the preceding two years were being followed.

After a two-year investigation, GAO now has concluded that although things have improved, the situation could be better. GAO says much more documentation is needed in NSF's files to justify how and why peer reviewers are chosen for grant proposals and to show how negative, or in cases where grant proposals are turned down, even favorable comments are considered and resolved.

NSF, however, quickly comes back with the observation that the "report reflects a GAO view that scientific and managerial judgment should be reduced to a form that permits auditors to construct a traditional 'audit trail' of paper."

Background on NSF's organization and its peer review process sheds some light on the controversy. NSF has six organizational units, called directorates. Each is headed by an assistant director and is subdivided into divisions, sections, and programs representing science areas.

The key individual in each program is the program officer, who manages the program-evaluation process. The program officer selects peer reviewers for each research proposal and, after the reviews are received, analyzes them and recommends that grants be awarded either in whole or in part or that the proposal be declined.

If the program officer recommends an award, the recommendation is sent to higher-level management for further review and approval. In general, this review and approval involves the section head, if any, the division director, and the assistant director or deputy director of the directorate.

In addition, each directorate has an action review board, made up of NSF employees with scientific, legal, business, or policy background. Each board reviews the essential elements of proposed grant awards and assures that peer reviews and other parts of the proposal evaluation process follow NSF policy.

In October 1977 NSF established a separate Office of Audit & Oversight reporting directly to the NSF director. The office is responsible for sampling proposal actions after the decisions are made to evaluate documentation and adherence to grant award and administration procedures and to monitor the appeals procedures for proposals that are declined.

However, despite the effort NSF has put into reviewing and tightening its peer-review process, GAO is still not satisfied with the results. In its investigation, GAO reviewed 56 grant award and 18 declination files to determine whether there was sufficient documentation in the file to support the decision. In most cases, according to GAO, there was not.

For example, only 53 of the 74 actions had been reviewed by an action review board. Of these 53, 49 received peer review, but only eight of the 49 files contained any documentation relating the expertise of the reviewers to the proposal or otherwise indicating why reviewers were chosen.

Quotes out of context?

Following is a selection of the negative comments cited by GAO as not having been adequately disposed of by NSF in deciding to fund research proposals, along with NSF's comment on the comments.

GAO: "Although there is, as stated, a considerable overlap with our own research, I have learned no 'secrets' from the proposal."

NSF: The next sentences in the review discuss the difference in details that show the overlap is insufficient to warrant comment by the program officer.

GAO: "This proposal is systematically naive. The 'need' for a shotgun survey in an exceedingly diverse coral reef assemblage is not defended . . . . This proposal is seriously deficient in not saying what organisms will be studied and why."

NSF: These excerpts refer to a portion of the project which was deleted before funding, as noted in the program officer's summary.

GAO also was disturbed by what it saw as lack of documentation detailing what was done, if anything, to modify the proposal or resolve the comments concerning the disposition of adverse peer-review comment. Of the 43 award recommendations reviewed, only summaries prepared in two directorates (nine awards) provided such documentation, according to GAO. Program officers' summaries in the other four directorates (34 awards) did not show the disposition of a number of negative comments for 17 of the files. One file did not contain a summary.

To beef up documentation, GAO suggests that NSF require all its directorates to use a form now required in the Science Education Directorate. This document describes for each peer review selected for a proposal: the reviewer's (1) academic institution, (2) position and organization unit within the institution, (3) discipline, and (4) skills or other expertise.

GAO maintains that documenting the disposition of all reviewers' adverse comments also is necessary and says this could be done with minimum effort. According to GAO, adverse comments could be numbered consecutively and then grouped for comment on a summary review sheet. The numbers of those not warranting detailed response by the program officer could be listed and a general explanation given. Comments needing more detailed response should be explained.

However, NSF "strongly disagrees with GAO that the additional documentation recommended will better assure fairness and equity in our proposal evaluation process." Commenting on the GAO proposal, NSF director Richard C. Atkinson says, "The specific recommendations seem to result from assumptions about and perceptions of the foundation's peer review process that are quite different from our views."

Atkinson also points out that "the objective of our review process is to obtain adequate information on which to base a decision about a proposal. We believe that, in most cases, whether or not this has been done is clear from the reviews and the departmental background of the reviewers, who function more as 'technical referees' than as 'advisers.' In addition, as part of our external peer oversight review, members of our advisory committees periodically examine the operation of each program, including a sample of the files on proposals. Among the matters on which these groups comment are the selection of reviewers and the appropriateness and adequacy of the reviews. In our judgment, this examination . . . is the most effective way to review these aspects of our stewardship. Even if additional paperwork would be of significant value, anything more than a virtually meaningless pro forma exercise would require a considerable increase in staff." □

CRITERIA FOR SCIENTIFIC CHOICE

ALVIN M. WEINBERG

I

As science grows, its demands on our society's resources grow. It seems inevitable that science's demands will eventually be limited by what society can allocate to it. We shall then have to make choices. These choices are of two kinds. We shall have to choose among different, often incommensurable, fields of science—between, for example, high-energy physics and oceanography or between molecular biology and science of metals. We shall also have to choose among the different institutions that receive support for science from the government—among universities, governmental laboratories and industry. The first choice I call scientific choice; the second, institutional choice. My purpose is to suggest criteria for making scientific choices—to formulate a scale of values which might help establish priorities among scientific fields whose only common characteristic is that they all derive support from the government.

Choices of this sort are made at every level both in science and in government. The individual scientist must decide what science to do, what not to do: the totality of such judgments makes up his scientific taste. The research director must choose which projects to push, which to kill. The government administrator must decide not only which efforts to support; he must also decide whether to do a piece of work in a university, a national laboratory, or an industrial laboratory. The sum of such separate decisions determines our policy as a whole. I shall be concerned mainly with the broadest scientific choices: how should government decide between very large fields of science, particularly between different branches of basic science? The equally important question of how government should allocate its support for basic research among industry, governmental laboratories, and universities will not be discussed here.

II

Most of us like to be loved; we hate to make choices, since a real choice alienates the party that loses. If one is rich—more accurately, if one is growing richer—choices can be avoided. Every administrator knows that his job is obviously unpleasant only when his budget has been cut. Thus the urgency for making scientific or institutional choices has in the main been ignored both in the United States and elsewhere because the science budget has been expanding so rapidly: the United States government spent $1,600,000,000 on research and development in 1950, $9,000,000,000 in 1960, $14,000,000,000 (including space) in 1962.

Though almost all agree that choices will eventually have to be made, some well-informed observers insist that the time for making the choices is far in the future. Their arguments against making explicit choices have several main threads. Perhaps most central is the argument that since we do not make explicit choices about anything else, there is no reason why we should make them in science. Since we do not explicitly choose between support for farm prices and support for schools, or between highways and foreign aid, why should we single out science as the guinea pig for trying to make choices? The total public activity of our society has always resulted from countervailing pressures, exerted by various groups representing professional specialities, or local interests, or concern for the public interest. The combination that emerges as our Federal budget is not arrived at by the systematic application of a set of criteria: even the highest level of authority, in the United States, the President, who must weigh conflicting interests in the scale of the public interest, is limited in the degree to which he can impose an overall judgment by the sheer size of the budget if by nothing else. But because we have always arrived at an allocation by the free play of countervailing pressures this does not mean that such free interplay is the best or the only way to make choices. In any case, even if our choices remain largely implicit rather than explicit, they will be more reasonable if persons at every level, representing every pressure group, try to understand the larger issues and try to mitigate sectional self-interest with concern for broader issues. The idea of conflicting and biased claims being adjudicated at one fell swoop by an all-knowing supreme tribunal is a myth. It is much better that the choices be decentralised and that they reflect the concern for the larger interest. For this reason alone philosophic debate on the problems of scientific choice should lead to a more rational allocation of our resources.

A second thread in the argument of those who refuse to face the problem of scientific choice is that we waste so much on trivialities—on smoking, on advertising, on gambling—that it is silly to worry about expenditures of the same scale on what is obviously a more useful social objective—the increase of scientific knowledge. A variant of this argument is that with so much unused steel capacity or so many unemployed, we cannot rightly argue that we cannot afford a big cyclotron or a large manned-space venture.

Against these arguments we would present the following considerations on behalf of a rational scientific policy. At any given instant, only a certain fraction of our society's resources goes to science. To insist or imply that the *summum bonum* of our society is the pursuit of science and that therefore all other activities of the society are secondary to science—that unused capacity in the steel mills should go to ' Big Science ' rather than a large-scale housing programme—is a view that might appeal strongly to the scientific community. It is hardly likely to appeal so strongly to the much larger part of society that elects the members of the legislature, and to whom, in all probability, good houses are more important than good science. Thus, as a practical matter we cannot really evade the problem of scientific choice. If those actively engaged in science do not make choices, they will be made anyhow by the Congressional Appropriations Committees and by the Bureau of the Budget, or corresponding bodies in other governments. Moreover, and perhaps more immediately, even if we are not limited by money, we shall be limited by the availability of truly competent men. There is already evidence that our ratio of money to men in science is too high, and that in some parts of science we have gone further more quickly than the number of really competent men can justify.

III

Our scientific and governmental communities have evolved institutional and other devices for coping with broad issues of scientific choice. The most important institutional device in the United States is the President's Science Advisory Committee, with its panels and its staff in the Office of Science and Technology. This body and its panels help the Bureau of the Budget to decide what is to be supported and what is not to be supported. The panel system, however, suffers from a serious weakness. Panels usually consist of specialised experts who inevitably share the same enthusiasms and passions. To the expert in oceanography or in high energy physics, nothing seems quite as important as oceanography or high energy physics. The panel, when recommending a programme in a field in which all its members are interested, invariably argues for better treatment of the field—more money, more people, more training. The panel system is weak insofar as judge, jury, plaintiff and defendant are usually one and the same.

The panel is able to judge how competently a proposed piece of research is likely to be carried out: its members are all experts and are likely to know who are the good research workers in the field. But just because the panel is composed of experts, who hold parochial viewpoints, the panel is much less able to place the proposal in a broader perspective and to say whether the research proposal is of much interest to the rest of science. We can answer the question ' how ' within a given frame of reference; it is impossible to answer ' why ' within the same frame of reference. It would therefore seem that the panel system could be improved if representatives, not only of the field being judged but also representatives of neighbouring fields, sat on every panel judging the merits of a research proposal. A panel judging high energy physics should have some people from low energy physics; a panel judging low energy physics should have some people from nuclear energy; a panel judging nuclear energy should have some people from conventional energy; and so on. I should think that advice from panels so constituted would be tempered by concern for larger issues; in particular, the support of a proposed research project would be viewed from the larger perspective of the relevance of that research to the rest of science.

In addition to panels or the bodies like the President's Science Advisory Committee as organisational instruments for making choices, the scientific community has evolved an empirical method for establishing scientific priorities, that is, for deciding what is important in science and what is not important. This is the scientific literature. The process of self-criticism, which is integral to the literature of science, is one of the most characteristic features of science. Nonsense is weeded out and held up to ridicule in the literature, whereas what is worthwhile receives much sympathetic attention. This process of self-criticism embodied in the literature, though implicit, is nonetheless real and highly significant. The existence of a healthy, viable scientific literature in itself helps assure society that the science it supports is valid and deserving of support. This is a most important, though little recognised, social function of the scientific literature.

As an arbiter of scientific taste and validity, scientific literature is beset with two difficulties. First, because of the information explosion, the literature is not read nearly as carefully as it used to be. Nonsense is not so generally recognised as such, and the standards of self-criticism, which are so necessary if the scientific literature is to serve as the arbiter of scientific taste, are inevitably looser than they once were.

Second, the scientific literature in a given field tends to form a closed universe; workers in a field, when they criticise each other, tend to adopt the same unstated assumptions. A referee of a scientific paper asks whether the paper conforms to the rules of the scientific community to which both referee and author belong, not whether the rules themselves are valid. So to speak, the editors and authors of a journal in a narrowly specialised field are all tainted with the same poison. As Einstein said, 'Eigener Dreck stinkt nicht.'[1]

Can a true art of scientific criticism be developed, *i.e.*, can one properly criticise a field of science beyond the kind of criticism that is inherent in the literature of the field? Mortimer Taube in *Computers and Common*

---

[1] As quoted by Dyson, Freeman J., in a review of Sweber, S.S., *Mesons and Fields*, in *Physics Today*, IX (May, 1956), pp. 32–34.

Sense[2] insists that such scientific criticism is a useful undertaking, and that, by viewing a field from a somewhat detached point of view, it is possible to criticise a field meaningfully, even to the point of calling the whole activity fraudulent, as he does in the case of non-numerical uses of computers. I happen to believe that Taube does not make a convincing case in respect to certain non-numerical uses of computers, such as language translation. Yet I have sympathy for Dr. Taube's aims—that, with science taking so much of the public's money, we must countenance, even encourage, discussion of the relative validity and worthwhileness of the science which society supports.

IV

I believe that criteria for scientific choice can be identified. In fact, several such criteria already exist; the main task is to make them more explicit. The criteria can be divided into two kinds: internal criteria and external criteria. Internal criteria are generated within the scientific field itself and answer the question: How well is the science done? External criteria are generated outside the scientific field and answer the question: Why pursue this particular science? Though both are important, I think the external criteria are the more important.

Two internal criteria can be easily identified: (1) Is the field ready for exploitation? (2) Are the scientists in the field really competent? Both these questions are answerable only by experts who know the field in question intimately, and who know the people personally. These criteria are therefore the ones most often applied when a panel decides on a research grant: in fact, the primary question in deciding whether to provide governmental support for a scientist is usually: How good is he?

I believe, however, that it is not tenable to base our judgments entirely on internal criteria. As I have said, we scientists like to believe that the pursuit of science as such is society's highest good, but this view cannot be taken for granted. For example, we now suffer a serious shortage of medical practitioners, probably to some extent because many bright young men who would formerly have gone into medical practice now go into biological research; government support is generally available for post-graduate study leading to the Ph.D. but not for study leading to the medical degree. It is by no means self-evident that society gains from more biological research and less medical practice. Society does not *a priori* owe the scientist, even the good scientist, support any more than it owes the artist or the writer or the musician support. Science must seek its support from society on grounds other than that the science is carried out competently and that it is ready for exploitation; scientists cannot expect society to support science because scientists find it an enchanting diversion. Thus, in seeking justification for the support of science, we are led inevitably to consider external criteria for the validity of science—criteria external to science, or to a given field of science.

---

[2] (New York: Columbia University Press, 1961).

V

Three external criteria can be recognised: technological merit, scientific merit and social merit. The first is fairly obvious: once we have decided, one way or another, that a certain technological end is worthwhile, we must support the scientific research necessary to achieve that end. Thus, if we have set out to learn how to make breeder reactors, we must first measure painstakingly the neutron yields of the fissile isotopes as a function of energy of the bombarding neutron. As in all such questions of choice, it is not always so easy to decide the technological relevance of a piece of basic research. The technological usefulness of the laser came after, not before, the principle of optical amplification was discovered. But it is my belief that such technological bolts from the scientific blue are the exception, not the rule. Most programmatic basic research can be related fairly directly to a technological end at least crudely if not in detail. The broader question as to whether the technological aim itself is worthwhile must be answered again partly from within technology through answering such questions as: Is the technology ripe for exploitation? Are the people any good? Partly from outside technology by answering the question: Are the social goals attained, if the technology succeeds, themselves worthwhile? Many times these questions are difficult to answer, and sometimes they are answered incorrectly: for example, the United States launched an effort to control thermonuclear energy in 1952 on a rather large scale because it was thought at the time that controlled fusion was much closer at hand than it turned out to be. Nevertheless, despite the fact that we make mistakes, technological aims are customarily scrutinised much more closely than are scientific aims; at least we have more practice discussing technological merit than we do scientific merit.

VI

The criteria of scientific merit and social merit are much more difficult: scientific merit because we have given little thought to defining scientific merit in the broadest sense, social merit because it is difficult to define the values of our society. As I have already suggested, the answer to the question: Does this broad field of research have scientific merit? cannot be answered within the field. The idea that the scientific merit of a field can be judged better from the vantage point of the scientific fields in which it is embedded than from the point of view of the field itself is implicit in the following quotation from the late John von Neumann: 'As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from reality, it is beset with very grave dangers. It becomes more and more pure aestheticising, more and more purely *l'art pour l'art*. This need not be bad if the field is surrounded by correlated subjects which still have closer empirical connections or if the discipline is under the influence of men with an exceptionally well-developed taste. But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganised mass of details and complexities. In other words, at a great distance from its empirical source, or after much "abstract" inbreeding, a mathematical subject is in danger of degeneration. At the inception the style is usually classical; when it shows signs of becoming baroque, then the danger signal is up.'[3]

I believe there are any number of examples to show that von Neumann's observation about mathematics can be extended to the empirical sciences. *Empirical* basic sciences which move too far from the neighbouring sciences in which they are embedded tend to become 'baroque'. Relevance to neighbouring fields of science is, therefore, a valid measure of the scientific merit of a field of basic science. In so far as our aim is to increase our grasp and understanding of the universe, we must recognise that some areas of basic science do more to round out the whole picture than do others. A field in which lack of knowledge is a bottleneck to the understanding of other fields deserves more support than a field which is isolated from other fields. This is only another way of saying that, ideally, science is a unified structure and that scientists, in adding to the structure, ought always to strengthen its unity. Thus, the original motivation for much of high-energy physics is to be sought in its elucidation of low-energy physics, or the strongest and most exciting motivation for measuring the neutron capture cross sections of the elements lies in the elucidation of the cosmic origin of the elements. Moreover, the discoveries which are acknowledged to be the most important scientifically, have the quality of bearing strongly on the scientific disciplines around them. For example, the discovery of X-rays was important partly because it extended the electromagnetic spectrum but, much more, because it enabled us to see so much that we had been unable to see. The word 'fundamental' in basic science, which is often used as a synonym for 'important', can be partly paraphrased into 'relevance to neighbouring areas of science'. I would therefore sharpen the criterion of scientific merit by proposing that, other things being equal, *that field has the most scientific merit which contributes most heavily to and illuminates most brightly its neighbouring scientific disciplines.* This is the justification for my previous suggestion about making it socially acceptable for people in *related* fields to offer opinions on the scientific merit of work in a given field. In a sense, what I am trying to do is to extend to basic research a practice that is customary in applied science: a project director trying to get a reactor built on time is expected to judge the usefulness of component development and fundamental research which bears on his problems. He is not always right; but his opinions are usually useful both to the researcher and to the management disbursing the money.

---

[3] Heywood, R. B. (ed.), *The Works of the Mind* (University of Chicago Press, 1947), p. 196

VII

I turn now to the most controversial criterion of all—social merit or relevance to human welfare and the values of man. Two difficulties face us when we try to clarify the criterion of social merit: first, who is to define the values of man, or even the values of our own society; and second, just as we shall have difficulty deciding whether a proposed research helps other branches of science or technology, so we will have even greater trouble deciding whether a given scientific or technical enterprise indeed furthers our pursuit of social values, even when those values have been identified. With some values we have little trouble: adequate defence, or more food, or less sickness, for example, are rather uncontroversial. Moreover, since such values themselves are relatively easy to describe, we can often guess whether a scientific activity is likely to be relevant, if not actually helpful, in achieving the goal. On the other hand, some social values are much harder to define: perhaps the most difficult is national prestige. How do we measure national prestige? What is meant when we say that a man on the moon enhances our national prestige? Does it enhance our prestige more than, say, discovering a polio vaccine or winning more Nobel Prizes than any other country? Whether or not a given achievement confers prestige probably depends as much on the publicity that accompanies the achievement as it does on its intrinsic value.

Among the most attractive social values that science can help to achieve is international understanding and cooperation. It is a commonplace that the standards and loyalties of science are trans-national. A new element has recently been injected by the advent of scientific research of such costliness that now it is prudent as well as efficient to participate in some form of international cooperation. The very big accelerators are so expensive that international laboratories such as CERN at Geneva are set up to enable several countries to share costs that are too heavy for them to bear separately. Even if we were not committed to improving international relations we would be impelled to cooperate merely to save money.

Bigness is an advantage rather than a disadvantage if science is to be used as an instrument of international cooperation: a $500,000,000 cooperative scientific venture—such as the proposed 1,000 Bev intercontinental accelerator—is likely to have more impact than a $500,000 Van de Graaff machine. The most expensive of all scientific or quasi-scientific enterprises —the exploration of space—is, from this viewpoint, the best-suited instrument for international cooperation. The exchange between President Kennedy and Chairman Khrushchev concerning possible increased cooperation in space exploration seems to have been well received and, one hopes, will bear ultimate fruit.

VIII

Having set forth these criteria and recognising that judgments are fraught with difficulty, I propose to assess five different scientific and technical fields, in the light of these. The five fields I choose are molecular biology, high-energy physics, nuclear energy, manned-space exploration, and the behavioural sciences. Two of these fields, molecular biology and high-energy physics, are, by any definition, basic sciences; nuclear energy is applied science, the behavioural sciences are a mixture of both applied and basic science. Manned exploration of space, though it requires the tools of science and is regarded in the popular mind as being part of science, has not yet been proved to be more than quasi-scientific, at best. The fields which I choose are incommensurable: how can one measure the merit of behavioural sciences and nuclear energy on the same scale of values? Yet the choices between scientific fields will eventually have to be made whether we like it or not. Criteria for scientific choice will be most useful only if they *can* be applied to seemingly incommensurable situations. The validity of my proposed criteria depends on how well they can serve in comparing fields that are hard to compare.

Of the scientific fields now receiving public support, perhaps the most successful is molecular biology. Hardly a month goes by without a stunning success in molecular biology being reported in the *Proceedings of the National Academy of Sciences*. The most recent has been the cracking by Nierenberg and Ochoa of the code according to which triples of bases determine specific amino acids in the living proteins. Here is a field which rates the highest grades as to its ripeness for exploitation and competence of its workers. It is profoundly important for large stretches of other biological sciences—genetics, cytology, microbiology—and, therefore, according to my criterion, must be graded A+ for its scientific merit. It also must be given a very high grade in social merit, and probably in technological (that is, medical) merit—more than, say, taxonomy or topology. Molecular biology is the most fundamental of all the biological sciences. With understanding of the manner of transmission of genetic information ought to come the insights necessary for the solution of such problems as cancer, birth defects, and viral diseases. Altogether, molecular biology ought, in my opinion, to receive as much public support as can possibly be pumped into it; since money is not limiting its growth, many more post-graduate students and research fellows in molecular biology ought to be subsidised so that the attack on this frontier can be expanded as rapidly as possible.

The second field is high-energy physics. This field of endeavour originally sought as its major task to understand the nuclear force. In this it has been only modestly successful; instead, it has opened an undreamed-of subnuclear world of strange particles and hyperons, a world in which mirror images are often reversed. The field has no end of interesting things to do, it knows how to do them, and its people are the best. Yet I would be bold enough to argue that, at least by the criteria which I have set forth—relevance to the sciences in which it is embedded, relevance to human affairs, and relevance to technology—high-energy physics rates poorly. The nuclear forces are not being worked on very directly—the world of subnuclear particles seems to be remote from the rest of the physical sciences. Aside from the brilliant resolution of the τ-particle paradox, which led to the overthrow of the conservation of parity, and the studies of mesic atoms (the latter of which is not done at *ultra*-high energy), I know of few discoveries in ultra-high-energy physics which bear strongly on the rest of science. (This view would have to be altered if machines such as the Argonne Zero Gradient Synchrotron were exploited as very strong, pulsed sources of neutrons for study of neutron cross sections.) As for its bearing on human welfare and on technology, I believe it is essentially nil. These two low grades would not bother me if high-energy physics were cheap. But it is terribly expensive—not so much in money as in highly qualified people, especially those brilliant talents who could contribute so ably to other fields which contribute much more to the rest of science and to humanity than does high-energy physics. On the other hand, if high-energy physics could be made a vehicle for international cooperation—if the much-discussed intercontinental 1,000 Bev accelerator could indeed be built as a joint enterprise between East and West—the expense of high-energy physics would become a virtue, and the enterprise would receive a higher grade in social merit than I would now be willing to assign to it.

Third is nuclear energy. This being largely an applied effort, it is very relevant to human welfare. We now realise that in the residual uranium and thorium of the earth's crust, mankind has an unlimited store of energy—enough to last for millions of years; and that with an effort of only one-tenth of our manned-space effort we could, within ten or fifteen years, develop the reactors which would tap this resource. Only rarely do we see ways of *permanently* satisfying one of man's major needs—in this case energy. In high-conversion ratio nuclear reactors we have such means, and we are close to their achievement. Moreover, we begin to see ways of applying very large reactors of this type to realise another great end, the economic desalination of the ocean. Thus, the time is very ripe for exploitation. Nuclear energy rates so highly in the categories of technical and social merit and timeliness that I believe it deserves strong support, even if it gets very low marks in the other two categories—its personnel and its relationship to the rest of science. Suffice it to say that in my opinion the scientific workers in the field of nuclear energy are good and that nuclear energy in its basic aspects has vast ramifications in other scientific fields.

Next on the list are the behavioural sciences—psychology, sociology, anthropology, economics. The workers are of high quality; the sciences are significantly related to each other, they are deeply germane to every aspect of human existence. In these respects the sciences deserve strong public support. On the other hand, it is not clear to me that the behavioural scientists, on the whole, see clearly how to attack the important problems of their sciences. Fortunately, the total sum involved in behavioural science research is now relatively tiny—as it well must be when what are lacking are deeply fruitful, and generally accepted, points of departure.

Finally, I come to manned-space exploration. The personnel in the programme are competent and dedicated. With respect to ripeness for exploitation, the situation seems to me somewhat unclear. Our 'hardware' is in good shape, and we can expect it to get better—bigger and more reliable boosters, better communication systems, etc. What is not clear is the human being's tolerance of the space environment. I do not believe that either the hazards of radiation or of weightlessness are sufficiently explored yet positively to guarantee success in our future manned-space ventures.

The main objection to spending so much manpower, not to say money, on manned-space exploration is its remoteness from human affairs, not to say the rest of science. In this respect space (the exploration of very large distances) and high-energy physics (the exploration of very small distances) are similar, though high-energy physics has the advantage of greater scientific validity. There are some who argue that the great adventure of man into space is not to be judged as science, but rather as a quasi-scientific enterprise, justified on the same grounds as those on which we justify other non-scientific national efforts. The weakness of this argument is that space requires many, many scientists and engineers, and these are badly needed for such matters as clarifying our civilian defence posture or, for that matter, working out the technical details of arms control and foreign aid. If space is ruled to be non-scientific, then it must be balanced against other non-scientific expenditures like highways, schools or civil defence. If we do space-research because of prestige, then we should ask whether we get more prestige from a man on the moon than from successful control of the water-logging problem in Pakistan's Indus Valley Basin. If we do space-research because of its military implications, we ought to say so—and perhaps the military justification, at least for developing big boosters, is plausible, as the Soviet experience with rockets makes clear.

IX

The main weight of my argument is that the most valid criteria for assessing scientific fields come from without rather than from within the scientific discipline that is being rated. This does not mean that only those scientific fields deserve priority that have high technical merit or high social merit. Scientific merit is as important as the other two criteria, but, as I have argued, scientific merit must be judged from the vantage point of the scientific fields in which each field is embedded rather than from that of the field itself. If we support science in order to maximise our knowledge of the world around us, then we must give the highest priority to those scientific endeavours that have the most bearing on the rest of science.

The rather extreme view which I have taken presents difficulties in practice. The main trouble is that the bearing that one science has on another science so often is not appreciated until long after the original discoveries have been made. Who was wise enough, at the time Purcell and Bloch first discovered nuclear magnetic resonance, to guess that the method would become an important analytical tool in biochemistry? Or how could one have guessed that Hahn and Strassmann's radiochemical studies would have led to nuclear energy? And indeed, my colleagues in high-energy physics predict that what we learn about the world of strange particles will in an as yet undiscernible way teach us much about the rest of physics, not merely much about strange particles. They beg only for time to prove their point.

To this argument I say first that choices are always hard. It would be far simpler if the problem of scientific choice could be ignored, and possibly in some future millennium it can be. But there is also a more constructive response. The necessity for scientific choice arises in ' Big Science ', not in

'Little Science'. Just as our society supports artists and musicians on a small scale, so I have no objection to—in fact, I strongly favour—our society supporting science that rates zero on all the external criteria, provided it rates well on the internal criteria (ripeness and competence) and provided it is carried on on a relatively small scale. It is only when science really does make serious demands on the resources of our society—when it becomes 'Big Science'—that the question of choice really arises.

At the present time, with our society faced with so much unfinished and very pressing business, science can hardly be considered its major business. For scientists as a class to imply that science can, at this stage in human development, be made the main business of humanity is irresponsible—and, from the scientist's point of view, highly dangerous. It is quite conceivable that our society will tire of devoting so much of its wealth to science, especially if the implied promises held out when big projects are launched do not materialise in anything very useful. I shudder to think what would happen to science in general if our manned-space venture turned out to be a major failure, if it turned out, for example, that man could not withstand the re-entry deceleration forces after a long sojourn in space. It is as much out of a prudent concern for their own survival, as for any loftier motive, that scientists must acquire the habit of scrutinising what they do from a broader point of view than has been their custom. To do less could cause a popular reaction which would greatly damage mankind's most remarkable intellectual attainment—Modern Science—and the scientists who created it and must carry it forward.

548

SCIENCE AND THE FEDERAL PATRON

MICHAEL D. REAGAN

NEW YORK
OXFORD UNIVERSITY PRESS
1969

10| An Approach to the Priorities Problem

We come now to the nub of the matter: the making of choices in allocating government funds for science. This is what the agencies and departments, the Executive Office organizations and the advisory groups, the congressional science committees and the appropriations process, are all about. Their fundamental test comes in their ability to allocate resources in a pattern that will satisfy both the scientific community and the larger society that rational use is being made of funds and manpower.

For almost a decade and a half we lived in a protected world; we avoided many hard choices because the R&D budget seemed to expand fast enough to permit everything to be done simultaneously. All fields of science were pursued, and, as new research problems emerged, new funds were poured into projects to solve them. When the Apollo project came along, requiring billions of dollars annually, it was added onto atomic energy, defense research, etc.; it did not have to take funds away from them. In basic university research, the expansion of funds apparently kept pace rather well with expanding demands—at least no screams were heard from the suddenly affluent (by comparison with all past history) research laboratories of academia. Graduate fellowship programs were being established and increased each year, so potential conflicts between funds for research as such and funds for graduate science education did not arise.

Choices were of course being made, nevertheless: some fields received more support than others. More attention was paid to defense and space R&D than to research that might create new civilian technologies or help in solving pressing social problems. Indeed, in the 1950s the nation seemed hardly aware that it had any pressing social problems, let alone concerned to see if science and technology could aid in their resolution. In recent years, our awareness of problems has increased, and so has the volume of criticism of the choices that have been made. These have been criticisms of the overall R&D pattern more than of choices within and among the basic science disciplines. That is, the criticisms (at least those reaching public visibility) have been of alleged imbalances between military and civilian directions of the R&D effort, between development projects aimed at space exploration and those aimed at clearing up air and water pollution, between resources directed toward supersonic flight and those directed toward a workable mass transit system for our metropolitan areas.

Even before the R&D budget began leveling off (about 1964–65, therefore, the *priorities* among competing projects were being disputed. There may have been some funds for all, but it was increasingly said that the larger amounts went to the wrong places. These disputes have not been over choices among sciences as such, however, but over the social values embedded in the *uses* to which R&D was being applied.

Now a new note is added. Questions have been raised about *scientific* priorities as such: are we putting enough into chemistry compared with physics and biology? Is a new high-energy accelerator needed as much as more funds for little science in the universities? Should oceanographic studies be singled out for special attention and extra funds? These questions are arising because the total money spent on science has become so large; because an increasing proportion of the total (though no exact measure exists) goes into Big Science projects; and because the demand for basic research funds from an expanding universe of scholars and graduate students is increasing much more rapidly than are the funds available. Now the harder choices have to be faced at all points of the R&D spectrum: not just whether one area is receiving more than it merits; but whether this one or that one should be funded at all, and whether one will have to be cut down in order to increase another.

Even apart from Vietnam war costs, it seems dubious that the exceptional rate of growth of the R&D budget during 1950–64 will be matched again. Yet costs are rising, and so are the number of projects that might be done and the number of researchers to be supported. The end result is that hard choices will have to be made henceforth both in the development of science and in the application of science to social-technological problems. Indeed, to the basic researchers, it appears that the basic allocational problem is the proportionate share to be awarded to each of these ends of the spectrum. Their fear is that a tighter R&D budget will squeeze basic research more than technological development projects, because the social value of the former is less politically visible. In any case, the ability of the system we have established to come up with viable judgments in making comparative allocations decisions is the crucial test of a science policy, and we are now facing that test in its harder forms for the first time.

Furthermore, a recent assessment of the American effort in planning science and technology by European experts throws down a challenge for us to try something harder than what we have been doing. According to C. H. Waddington, a former member of the United Kingdom Advisory Council on Science Policy, the "strategic thinking" in American science has been remiss. We have tended, he asserts, to look at our present situation at a given moment and ask, "Where do we go from here?" Instead, he says, we should be more actively deciding where we want to go and then asking, "How do we get there?" Had we done the latter, he thinks we would not be emphasizing the development of a supersonic aircraft while relatively neglecting the development of ways to double the world output of food by the year A.D. 2000. His strongest criticism of American science planning is that we failed to formulate a comprehensive research program aimed at "ensuring that life is biologically enjoyable." [1]

Is Waddington criticizing our science planning, however, or our sense of values as expressed politically? The latter seems to me to be the case, but the very question points up what has to be the basic premise of priority planning: it cannot be a strictly scientific matter. The decisions cannot be made on scientific grounds only, but must incorporate a blend of scientific and social elements, of criteria of scientific merit—What pattern of allocations will produce the greatest advance in scientific knowledge?—with criteria of social merit—What pattern of allocations will produce the most important advances in the practical utilization of science and technology to improve human welfare?

Although the advancement of the public welfare is the ultimate ground on which public support of science rests, we built for some years a protected enclave in which all but criteria of scientific merit were firmly excluded—at least in the ideology and wishful thinking of the basic research community. This was never entirely so, and could not have been, in a system that provided even the bulk of basic research funds through mission-oriented agencies; but it is only recently that we have attempted to build this fact into our system of thought for science planning. We are only now realizing that the advancement of science and the advancement of social objectives through science must be kept in balance, even if balanced tension. The head of an organization particularly sensitive to the balance between social and scientific merit in a research program, James A. Shannon, director of the National Institutes of Health, poses the situation clearly:

> Our national program in support of science is entering a phase in which the decision whether to support a given field depends less upon technical considerations than upon social need. If we are to continue to have adequate support for the fundamental as well as the applied . . . then the aggregate effort must be to develop some sense of balance between the two approaches. Take, for example, the problem of water pollution, which has long since ceased to challenge the medical investigator. Water pollution, beyond a doubt, will yield to techniques of management—to an approach that relates the capacity of the nation's water resources to the ways in which we use them. The field of air pollution affords a comparable case. We certainly inhale numerous carcinogens in perhaps dangerous amounts, not only from cigarettes but from a wide variety of hydrocarbon wastes; but knowledge of these carcinogens does not provide the thrust for their elimination nearly so much as the social pressure of people living within our metropolitan areas who insist on clean air as essential for comfort.[2]

Our R&D effort can become badly imbalanced in either of two ways. Either by social pressures skewing science so that we ignore basic research's needs because we fail to see practical applications, thus leading us to try to solve problems technologically before the appropriate science is ready for them.

Or, in the other direction, by too completely separating science development from science use in an effort to protect the former, with the result that we produce an arid and precious science remote from the concerns of its human sponsors. The public that sees science as technological wonders would tend toward the first type of imbalance; some scientists would tend toward the latter.

Alvin Weinberg, in the articles cited in Chapter 2, took a balanced view—a politically realistic view. As criteria for establishing priorities he advocated a mixture of scientific and extra-scientific considerations. The competence of the scientists and the ripeness of a field were joined in his scheme by the technological and social merit of the field. Thus, the behavioral sciences he judged to be of high social relevance, but not yet developed sufficiently as sciences to warrant much support. Molecular biology, on the other hand, he gave high points to, both for its scientific qualities and for its social merit in terms of potential applications to health problems. The manned space effort he derided as important neither scientifically nor socially —perhaps reflecting primarily a personal value position more than the result of examination in the light of formal criteria.

In attempting to develop a single set of criteria to apply to the entire spectrum of R&D, from high-energy physics to the manned space program, Weinberg was perhaps overly ambitious. I would take the position that there can be no single set of criteria across the board; that we must categorize different areas and develop both substantive criteria and decision-making mechanisms appropriate to each area. In only a limited sense, at the level of OST and BOB, is it fruitful to attempt an across-the-board evaluation, and then not by comparing particular fields or projects at different points along the research-development continuum but by providing major directions and assessing the total balance between undirected basic research, applied research, and technological development in the nation's R&D program. For the most part, we must establish sub-universes of science policy and develop approaches individualized to each.[8] And in doing so, it is important that we distinguish in each area the varying proportions of scientific and non-scientific factors to be "cranked in" to the decision-making process. At many levels, science *policy* must also be in part science *politics*, not in the pejorative sense of politics, but meaning that non-scientific values and interests are legitimately to be considered along with scientific criteria. The correct mixture at each level is the important—and difficult—consideration.

The allocations processes must also explicitly recognize by-product values. Science does not exist in isolation; it is ineluctably related to other social spheres: education, industrial growth, regional development, for example. Sometimes the science-related values are the ones to which we will seek to give priority, as much as to science itself. In formulating allocations policies, therefore, we will sometimes be formulating policies *for* science in its own right and sometimes supporting science instrumentally, as a way of achieving related objectives. Again, we must particularize. Support of undirected basic research relates especially closely to support of graduate education; major space program decisions may especially affect regional economic growth. The important point is that in each area we have a multi-value focus. This in turn reinforces the point that in making science allocations it is not just scientists and criteria of scientific merit that are involved.

The question of priorities is an extension of the question of justifications for support of science that we examined in Chapter 2. There we analyzed the reasons given by scientists for federal support, both of science generally and of particular fields. Although there were some implications for priorities, none were stated explicitly. We found that reports on individual disciplines and areas each offered statements of needs and opportunities that justified federal funds, but almost no clues were given to ways in which the needs of one field might be compared with those of another. The NAS Committee on Science and Public Policy, which sponsored several "needs and opportunities" studies, has not felt it possible to arrange any priority listing among the disciplines on the basis of the individual studies. Perhaps the closest approach to comparative criteria that emerge out of such studies lies in the frequent references to manpower projections, showing increases in graduate students and professional investigators, and to proposal pressures: the number and dollar amount of projects proposed by investigators compared with the funds available from granting agencies. Chemistry, for example, was able to demonstrate that a lower proportion of its proposals were being funded than those of some other disciplines, and this fact has apparently been taken into account by NSF as justifying a priority for an increase in chemistry funds.

Not only do we lack comparative substantive criteria for justifying priorities; we also lack a clear mechanism for utilizing such knowledge of the situation as "needs and opportunities" reports are able to make available. Because there is no priority planning system as such, it is so far a matter of informal feedback: NAS or similar studies of fields are picked up by NSF and OST, perhaps referred to an FCST group, and eventually some incremental budgetary adjustments may filter through BOB and be reflected in agency budgets.[4] The concept of NSF as a balance wheel for basic research may be relevant here, since the practical meaning of giving a basic research field a priority may be to give it extra funds in the NSF budget, if gaps in its support are found to be developing from other agencies. Some organizational engineering is called for.

In any case, since scientists have been able to rationalize criteria for federal support of science but have not been able to provide clear priorities criteria or to suggest a mechanism through which comparative allocations determinations might be made, that task still lies before the science affairs community. Not pretending to greater insights than the spokesmen of science have themselves been able to bring to bear on these problems, I nevertheless venture now to suggest a set of categories for priority decisions—a set of categories that also constitutes a ranked priority listing for R&D support—and to comment on some of the considerations that an allocations system will have to grapple with. Perhaps doing so will move us a little bit further forward by provoking others to correct, amplify, or replace what I say.

A PRIORITIES FRAMEWORK

To propose categories of science and science-related activities within which allocations decisions are to be made requires analysis of the science affairs universe. To propose a priority ranking of the categories themselves requires both analysis and the application of a set of extra-scientific values. Ultimately, each of us has his own, individual scale of values; practicably, we can propose a pattern that would seem to fit the politically relevant values for this time in this society. The scheme below embodies the fundamental premise that human needs of an urgent, even if elementary, level—such as attacks on disease or making the environment humane—provide a more fundamental guideline determining what fields to support than do the needs and opportunities of science disciplines considered in themselves. The overall case for science support by the taxpaying public, that is to say, must rest upon social benefit, but this is not to be narrowly construed. The cultural enrichment of the community through greater understanding of the universe is as much a social benefit as the advance of the economy. Since a major justification for research support is that research pays, it would be logically contradictory not to relate priorities in research to areas from which a pay-off is most urgently desired. Development goals should thus play a role even in planning basic research priorities. With that premise understood, this is the priority pattern and set of categories I propose:

> *First priority* goes to those *social objectives which are defined as most urgent politically* and to which scientific research can most clearly make a contribution.
>
> *Second priority* goes to *science-related educational needs,* from the elementary schools to the graduate education-research laboratory level.
>
> *Third priority* goes to *undirected small-scale research*—so-called little science.
>
> *Fourth priority* goes to *Big Science:* those fields having very high equipment costs or requiring large teams of researchers or large facilities, yet which are part of basic research.

These categories cover the R&D spectrum, and they cover both science clearly related to extra-science missions and science done to add to the intellectual capital out of which eventual, but not now predictable, technological advances will be derived. Within each category, separate criteria and processes of decision-making need to be elucidated. Scientific criteria as such will be most important in the last two, social merit criteria in the first two. Just as the criteria need to be accommodated to each subject area, so also will the appropriate mix of those making decisions vary from one category to another.

No single "priorities planning board" could be established that would have a character appropriate to making decisions for all of these areas, although the overall balance among them must be looked at by a coordinating body with overall responsibility for science policy—presumably OST-BOB.

Now let us comment on each category in turn.

First Priority. Our area of first priority—urgent social objectives that require science for their achievement—is the one least encompassable in a purely scientific framework. The criteria are primarily those of social merit, politically defined. Yet this is not to say that a technical input is not also necessary, for what is socially desirable may not always be technically feasible. The technical "state of the art" necessarily affects the pace at which, for example, we can introduce effective air pollution control programs.

How we define social merit is the first question, the fundamental one, though. It is here that the European criticism mentioned above is perhaps most applicable: that our system of technological planning does not always seem to cause us to emphasize what we ourselves know to be most urgent. There is surely more unhappiness over pollution and more need for an improved urban transportation system than there is public demand for supersonic flight, yet the latter seemed for some time to receive greater governmental attention. Why? The answer may be that we really do not engage in technological planning at all, but that instead we let technology plan *us*. That is, we seem to do what the state of the art tells us will be possible, following the path of least social resistance. The supersonic plane and the man shoot are projects that might be called "technologically sweet." They excite the engineering imagination; they challenge our ingenuity—but within limits we know are possible. And they do *not* challenge our social system: there are few social-economic-political obstacles to their achievement, so long as the government puts up the money. They therefore constitute a path of technological least resistance.

Perhaps we follow this path because we lack a mechanism for forcing ourselves to think through the comparative advantages to be gained from pursuing any of several technological options—or a mechanism for asking ourselves, "Where do we most want to go?" and, "Can technology take us there?" And in the absence of such mechanisms, we let technology direct us by default.

At one level, this is basically a problem of our entire political system. The President and the Congress are supposed to provide leadership in analyzing choices open to us and in proposing social priorities. This they do not always do well. Vested interests in government agencies and outside groups are always active on behalf of their pet projects, and they often succeed by default because we do not sufficiently engage in conscious priority planning. This is why Rep. Daddario's concept of a Technology Assessment Board has much to recommend it, whether in the particular form he has proposed it or in some other form.

The need for more conscious appraisals of what we want from science and technology, and of the ramifications of what we are told is possible, might be partially met by a combination of OST-PSAC efforts along lines of technology assessment in this sense and related studies by the proposed Joint Committee on Science and Technology in Congress. PSAC has done several studies of social problems having technical aspects, including population control and pollution, but it has not been asked to broaden its net and advise the government regarding which social problems science and technology may best contribute solutions to. If it could do that, suggesting a range of problems and possible technical approaches, we might have a better information base upon which to make our political decisions. This is not to say that the social choices should be turned over to the scientists, but that by clarifying the relationships between social problems and the technical state of the art they would also be clarifying the larger social choices.

The essential role of the scientific community in the sphere of urgent social problems is to analyze and point up the contributions that research and development can make toward their solution. Given the degree of specialization in our society, it is not often going to happen that those most concerned about some societal problem are also those most knowledgeable concerning the contributions that science might make. And the most important aspect of the scientists' role will be to suggest ways in which science may be relevant when the non-scientist would not suspect its relevance. Often they will be able to suggest ways in which social requirements and statements of problems may be redefined because of technical potentialities unsuspected by social analysts. One thinks in this connection of the war-time history of the Office of Scientific Research and Development. OSRD was most effective because the scientists were free to think of new weapons possibilities that required new strategies, rather than just to operate within the boundaries of existing strategic requirements as defined by the military. In the field of mass transit, for example, scientists and engineers may be able to think much more imaginatively about possibilities than the transportation economists and practitioners of transportation who are "culture bound" by existing modes of transportation.

Choices of priorities among societal problems having technical dimensions will, to the extent that basic research is involved, also mean the setting of some priorities in basic research, too. To the extent that some area of basic research relates predictably to a priority problem, that research shares in the priority. Because imbalances in basic research support might thus be engendered, this possibility makes more important the balance-wheel function within the basic research sector, and it means that all sectors need to be kept in close communication with one another.

Second Priority. It would seem to fit both the scientific community's and the public's sense of needs to give a high priority to educational support. While some scientists are dubious about almost all efforts to engage in science planning, fearing the imposition of irrelevant or ill-informed non-scientific criteria, I think none would take exception to planning that has as its objective the maximizing of the future potential of science through building appropriate resources. And the most important resource is trained scientists themselves. Science education, from kindergarten through post-doctoral research training, is therefore perhaps the strategic focal point in the expansion of science resources for the future.

As we observed in Chapter 2, one of the major arguments offered on behalf of scientific research support by the Federal Government has been the contribution that research makes to graduate education. And we have seen that this relationship has been stressed ever more strongly in the past couple of years, to the extent that at least one spokesman would make the educational by-product of basic research almost the only reason for increased federal support at the present time. While science education has been a by-product of research support, it demands attention as a more autonomous goal. That it is becoming so is clear from the shift we have observed in the focus of federal programs from exclusive concern with the individual investigator and his research toward equal concern with the support of the educational institutions in which the research is conducted. For the universities are themselves part and an essential part, of science's resource base. If educational support has been a "spin-off" benefit of research support, it is equally true that research support is in part a "spin-off" of educational support. Educational and scientific criteria must therefore be carefully blended to get the most out of federal programs that affect both. There are some problems in achieving the appropriate blend.

To argue a high priority for science education is easy; to determine how much money is needed and what rate of increase is required each year are difficult. Brooks has pointed out that the educational justification for federal funds for science can be more easily quantified than the cultural or other justifications. One can know the number of students at every level; one can measure the rate of increase in graduate students for each discipline for the past *x*-years, and estimate the manpower needs of each field; an estimate can be made of equipment needs per student and researcher, etc. Some of the NAS Committee on Science and Public Policy studies of scientific fields' needs and opportunities have therefore relied heavily on graduate student enrollments, present and projected, in justification of desired support levels for fellowships and research.

Certainly the statistics of enrollments and equipment costs provide a useful base line for estimating support needs for science education. But Stephen Toulmin has brought out some difficulties that prevent such statistics from being adequate when used in their raw form:

1. Recent increases in graduate students in specific fields may reflect nothing more than "the effectiveness of past Federal support in attracting graduate students into one field or another."

2. We have no "reason to suppose that these projected rates of increase reflect either the probability of utility or the intrinsic intellectual value of the competing fields of science."

(This suggests that relative support levels for education in each discipline logically require prior determination of the intellectual/practical importance of each field.)

3. "If we tie levels of support for different sciences to the number of graduate students in the different sciences," says Toulmin, "we are giving support in direct proportion to the educational relevance."[5] Yet that relevance is not the *only* significant basis for determining needed levels of support in different fields. This is a point at which the precise blending of educational and intrinsic scientific criteria becomes most difficult and most important. In granting project awards, educational criteria are generally thought to be irrelevant, or at best ancillary to such criteria as the quality of the researcher and the promise of his project design. The use of graduate student enrollments to determine allocations for research support therefore contradicts in part the desire to maximize research criteria as such rather than educational criteria in granting project awards.

We may therefore need to distinguish those aspects of science education that can be supported in themselves from those that flow out of and must share criteria with research objectives. At the pre-college levels, science education efforts can presumably be separated almost entirely from research support questions. Equipment for laboratories and demonstration teaching purposes can be provided to elementary and secondary schools on something approaching a per capita enrollment basis, and teachers can be brought up to date in summer seminars without raising research support questions, or concern about the relative distribution of support between fields. At the undergraduate college level, the picture is becoming mixed; as acceleration in education occurs, and the belief grows that research experience is essential, it is harder to support science education without supporting the research of science faculty.

Finally, at the graduate level, the research and educational criteria come together in a tight merger. It is at this level that some additional tough questions need to be faced.

To aver that basic research and graduate education are mutual requisites, and that a major reason for supporting science is to produce more scientists through graduate research training, has become a truism. In fact, it has become an unthinking slogan, especially since the definitive statement of the position was made in the 1964 NAS report, *Federal Support of Basic Research in Institutions of Higher Learning*. Too many simplistic generalizations are being propounded, without recognition of their inconsistency with existing patterns of support for science and for education.

The problem is not that the research-education link is not as strong as claimed, for it is. The problem is that it states but a part of the truth, while policy must embrace a larger number of facets. For example: the scientist as educator is more than a researcher; science education includes the dissemination and reception of information and ideas, as well as the research experience; the man with the best project may not be the one who will do the best job of bringing his students into the work and insuring that they get the most out of it; the production of scientists requires educational support at all levels, not all of which are as closely tied to research experience as is doctoral work. The view that implicitly assumes that science education needs will be taken care of if all individual university-based project applications are funded is therefore a myopic one.

Since so much of graduate education support has taken the form of research project support, a crucial question lies in the relationship between project support as research and project support as a means to education. Should contribution to graduate education be a factor (an important one) in awarding research grants from NSF or NIH? What role does this factor play now? If the role is minimal, then is it the case that the vaunted contribution of basic research to graduate education is nearly ignored at a vital point?

A statement of the guidelines used by NSF in evaluating research proposals states that the principal criterion is the predictability that the research will "result in significant advancement of knowledge in a field of interest and importance." Six sub-criteria are then listed, after which comes this statement:

> Account is also taken of the contribution which the research will make to science education and training, particularly at the graduate level. This is generally judged on the basis of proposed student participation in the work. The extent to which the research will stimulate the total science enterprise of the institution is an additional factor.

It is also stated that proposals of middle-range quality "are supported at lesser institutions in order to encourage development and improvement of their research and instructional programs."[6]

From most comments of scientists upon the project research system, one gets the impression that these ancillary educational criteria are of very minor importance in the evaluative process. Whatever the agency's intentions, most scientists seem to feel that in advising on research awards, purely scientific criteria should prevail: the quality of the investigator and of the project. The peer judgments on which the system rests are judgments of scientific, not educational, competence; the value system of science would be skewed to the extent that scientific criteria were diluted by any others in determining research support. The two facets, most scientists would say, require different approaches. I think most of them would prefer that educational support as such be offered in other forms, other packages, to preserve the purity of the project system. Yet this position is clearly inconsistent with the notion that a major reason for supporting faculty research, and especially for measuring needed increases in the support rate, is the need thereby to support graduate education. This latter value, especially now that emphasis is being placed upon building up centers of excellence in the "underdeveloped" regions of the nation, seems logically to require that the educational aspects be placed on a par with the scientific ones. Either that, or else less should be made of the relationship and educational support should be considered a separate category to be handled by other types of programs: fellowships and traineeships, principally, and equipment support.

I may perhaps state the matter too starkly, for it may be contended that the educational value of a research project depends strongly on its value as research. From this point of view, it may be a waste of funds to support less-than-first-rate research at lesser institutions in order to support education. Perhaps more general institutional aid is needed for such institutions, in order to build them up to the point where project support can be both scientifically and educationally justifiable for them. This makes much sense, yet poses difficulties for the base-broadening objectives of current policy, for it implies a high degree of continued concentration of project support in the "best" places, at the same time that scientists are asserting that such support is essential to graduate education.

Undoubtedly, the research-education relationship is that of a continuum rather than an either-or opposition. Yet I think it important to state the matter rather starkly because so many misleadingly simple assertions have been made about the relationship. Furthermore, the priority given to some of basic research is affected by the relationship it bears to education support, since in this scheme education support as such receives the higher priority. It may carry some research *per se* along with it, to the extent that the case is made effectively that the research support makes a necessary contribution to the educational objective. My purpose is not, I repeat, to deny the assumed connection, but to urge scientists and science-education administrators to think through the particular modes of connection more explicitly and carefully.

Third Priority. The third priority, I have suggested, should be for undirected small-scale basic research—the investment capital upon which rest technology and the application of science to areas of social need. Although this is the heart of the whole R&D spectrum in the thinking of basic scientists, it does not receive the highest priority in this scheme for the reason that (like capital investment versus current operations in industrial planning) its long-run importance is greater than any single year's exact level. In any one year there is undoubtedly a minimum amount required for continuity of effort, but the appropriate amount above that, or for expansion, is not easily established. What we need to do is to ensure a healthy, continuing research program so that the well-springs do not dry up in areas not presently tied to a higher priority objective (but which may become tied in the future, and unpredictably so).

In analyzing the basic research allocations problem, the fundamental fact is that, in the nature of basic research, one cannot establish concrete objectives and then estimate the costs of reaching them. There is no way to measure the added "output" in knowledge or utility to be gained by an extra dollar in the basic science budget—and equally no way to measure the loss for a dollar subtracted. At least in the present state of our knowledge about the processes by which intellectual advances are translated into technological innovations, nothing but the vaguest predictions can be made as to the ultimate effect of either halving or doubling the basic research budget in a given year. The construction of a science budget cannot proceed entirely from the intrinsic elements of research but must be calculated in considerable part from essentially extrinsic factors, be these graduate student enrollments, proposal pressures, or whatever. There is no scientific method for constructing a science budget.

As is often the case, it is easier to begin by rejecting what one believes to be unworkable than by proposing what one believes to be appropriate. I therefore begin by rejecting the possibility of determining an independent total figure for basic research, whether based on a percentage of all R&D, a percentage of Gross National Product, or the number of scientists multiplied by an average support figure.[7]

The concept of "overhead," borrowed from economics and business practice, has been put forward as a convenient handle for rationalizing basic research support in relationship to the large sums of money put into technological developments. About the only thing to be said for this, however, is that it at least recognizes that there is a relationship between the R and the D, a relationship which basic scientists too often want to ignore in the interests of keeping science "pure." In the business world, I understand that the overhead concept is a slippery one: it is difficult to give it a precise quantity except after the fact. What is difficult in business is, I think, almost impossible in research and development. Unless and until we develop better ways of tracing back technological developments to their basic research origins, we cannot measure the amount of basic research to be allocated as overhead to a particular technological development. And without the ability to make such measurements, we have no way of knowing whether the appropriate overhead is 5, 20, or 90 per cent of total R&D.

As a practical matter, I would expect this argument to be less vigorously pushed now that the R&D budget has leveled off. Neither the number of scientists nor the research cost per scientist has leveled off, so continuance of the overhead percentage of recent years (9 to 11 per cent) would mean declining support per scientist—which is hardly what the research community is arguing for.

All of these shortcomings exist in even stronger form as criticisms of the suggestion that basic research be budgeted as a given percentage of Gross National Product. Tying basic research to GNP would have the advantage that the growth of GNP is steadier than growth of the R&D budget; but there is little else to recommend it, when we have no way of knowing what percentage of GNP devoted to basic research is needed for eventual technological utility nor what percentage is needed to achieve an acceptable rate of increase in scientific knowledge. While in principle some connection exists between the amount of research the society does and its rate of industrial progress, we know too little about the long, complicated chain of relationships involved to permit their embodiment in a research support formula.

Another approach to an overall support formula would be to proceed from the number of scientists to be supported. In one version (that of Seitz) the government should support all the "good scientists" at $50,000 each annually. Another (Brooks) has suggested full support for all the "most talented and creative people," a group which he estimates as perhaps 5 per cent of all those "capable of doing competent and significant basic and applied research." Since the scientific community demonstrates an unwillingness to make judgments of the comparative merits of different fields, it is difficult to believe that any workable system of public judgments can be devised by which scientists will tell us who among them represent the top 5 per cent. Furthermore, even if we had a way of identifying the elite, the problem remains of determining the amount of support for the remaining 95 per cent. Brooks's answer for the remainder appears to be to use faculty and graduate student numbers as the base, so far as university research is concerned. Verhoogen, on the other hand, proposes that "every scientist who is capable of raising a valid scientific question and of contributing significantly to its solution . . . should be given an opportunity to do so by means of a research grant." On the basis of $20,000 per year, and estimating that the "number of competent scientists with good ideas" is between 50,000 and 100,000, the little science research expenditure would perhaps amount to one billion dollars annually. The choice of competent scientists to receive these grants Verhoogen would determine by the total sum requested for all proposals rated "good" by advisory panels. The injection of proposal pressure at least provides some check beyond a simple head count.

I doubt that it is either sensible or practicable to support everyone calling himself a scientist any more than we would support every person with a Master of Fine Arts degree as an artist. And for reasons indicated earlier I am dubious about using graduate student enrollment as the crucial determinant, although it certainly needs to enter into the picture.

Finally, it appears to be a major shortcoming of all the formulae for overall support that they are farther removed even than is necessary from the intrinsic questions of basic research: what are the gaps in our knowledge and understanding; what are the areas most ripe for exploitation? And they ignore the fact that most of the basic research funds come from agencies with extra-scientific missions, which means that the needs of those missions must enter the picture, as well as the needs of scientists. Even though we are discussing undirected basic research, we are not talking just about NSF funds; yet it is only regarding NSF's small portion of basic research that one could meaningfully contemplate an autonomous basic research formula.

Turning to the more positive side, therefore, I would suggest that the construction of the basic research budget proceed from field-by-field analyses of needs and opportunities; from Weinberg-type criteria questions; from proposal pressures; and from higher education institutional needs. The combining of these related yet partly disparate criteria would be no simple feat, but it would provide a balanced approach, even if informed judgment would play a larger role than quantitative calculation.

Weinberg's criteria of scientific, technological, and social merit still provide the best checklist that has been suggested for evaluating the claims of scientific fields. A comparative priority of a kind would be provided by ranking fields that stood high on several counts above those with lower "scores." The problem is: who will do the evaluating and ranking? Much as it might hate to take on the task, COSPUP is the likely candidate. Composed of an interdisciplinary team which reviews NAS field reports before giving them its endorsement, COSPUP cannot help but have some feel for the strong and weak points of each field. It is in a better position than any other group to make the comparative assessment that will be needed if there is to be a priorities system at all. A COSPUP report on comparative merits need not be taken as gospel but would be most significant for further decision-making.

The kind of assessment that application of Weinberg-type criteria would provide could usefully be supplemented with proposal pressure information. Not just the number of proposals in each field, but more significantly their average quality as indicated by review panel ratings. If field A has more proposals than field B, but a higher proportion are low-rated, then field B would have a higher rating as a field. Proposal pressures should not become the dominant criterion in establishing priorities, however, because a priority may be needed exactly to fill gaps left in areas that are not currently popular and which therefore do not attract many proposals. Sometimes a more active approach is required, one in which relevant agencies seek to stimulate proposals to build up a field, rather than just respond to the current fads among investigators. It is at this point that needs and opportunities studies may be most important—in identifying gaps left by proposal pressures. In this sense, basic research planning partakes of the notion of management by exception: the giving of special attention to what is not being taken care of spontaneously. Given the explosion in research publications, it becomes increasingly difficult for individual investigators to review their own fields in order to determine where the best opportunities lie. This has to become a specialized function today, and NAS field reports and agency advices based on them and on analysis of proposal pressures can be most helpful in providing intellectual maps of the various fields, emphasizing gaps, as clues by which individual investigators—particularly perhaps the young ones not already totally immersed in a certain line of investigation—may select their own work areas. In this way, the next year's set of proposals may be encouraged into better balance.

Assuming that one could work out some qualitative weighting of the various components of a basic research budget, the next question is the practical one of how to organize the mechanism to accomplish an inter-disciplinary, inter-agency comparative review. Such a review might be accomplished along the following lines.

First would come single field reviews. The agency having the largest commitment to a given field (e.g. AEC for high-energy physics; NIH for the biomedical sciences) would establish a review panel to which *all* agencies supporting that field would send the proposals they received, with endorsing comments indicating the relevance of each proposal to their missions.

Second would come an inter-field review to compare the quality of marginal proposals in each field, and to identify areas of unusual activity or areas of insufficient activity. This second-level review might be performed by an interdisciplinary and inter-agency group of program administrators, perhaps organized by NSF as part of its balance-wheel role.

Thirdly, the results of the marginal assessment process would be embodied in an overall basic research plan for final review by OST-BOB. After such review, there would be a choice. The plan could be made binding on the constituent agencies, or the judgments about inter-field comparisons and suggested differing levels of support for each field might simply be forwarded to all the participating agencies as recommendations. Then NSF could try to compensate in its own allocations for what it felt to be out-of-line results in the decisions made cumulatively by all the other agencies.

The latter—looser—approach is probably not only the more realistic one in the light of normal agency jealousies over external dictates, but also better suited to fitting the basic research plan to mission considerations: an agency will sometimes feel that mission needs must override the more purely intellectual judgments that the panel review process would embody, and it will insist on doing either more or less than a disciplinary viewpoint alone would call for.

An approach such as this would seem to constitute a way of striking a reasonable balance between agency autonomy and over-centralized administrative priority setting. Basic research support in the American system is dispersed among too many agencies, and derived too strongly as a by-product of extra-scientific missions, for there to be a very rational overall structure if everything is left entirely to agency-level decisions. That same extreme diversity, however, makes it unwise, as well as administratively impossible, to impose a central plan on the agencies by fiat from above. As compared with existing practices, however, some movement in the direction indicated (i.e. toward the injection of a larger central-review element) is, I believe, in order today. It will become more necessary over time if we are correct in our expectation that the priorities question will become more rather than less pressing as the increasing costs of science bump against the ceiling of political resistance to ever-expanding budgets.

Fourth Priority. Our last priority goes to Big Science—the segment of basic research that departs most obviously from the classic picture of the individual investigator pursuing his curiosity wheresoever it may lead. Big Science is characterized by organized team efforts, by tremendous equipment and organization costs, and, in consequence, by a degree of planning and programming unknown to, and unneeded by, little science. Furthermore, Big Science is likely to be done in research centers separated in more or less degree from the regular university campus, and there are but a few such centers for each field. This in turn carries the implication that Big Science fields typically have a relatively small number of practitioners with very high costs per investigator.

The prototype of the Big Science field is high-energy physics: a basic installation costs hundreds of millions of dollars to build and many millions annually to operate. Radio astronomy, oceanography with its ships, and certain lines of investigation requiring major computer installations are other components of Big Science. The characteristics emerged first in the physical sciences, but are now spreading to the life sciences. One can even foresee Big Science in the social sciences, as we learn to formulate large-scale surveys involving large populations through the use of investigatory teams and computers. In educational sociology, for example, one can easily envisage very large-scale projects attempting to isolate familial and environmental influences upon educational achievement of the child.

So far as high-energy physics and radio astronomy are concerned, Big Science has also meant very pure science: our cultural enrichment by learning more about the nature of matter and the origins of the universe is the primary goal; practical applications of the knowledge are extremely remote. But oceanography is both pure science and a field of vast discernable applications for navigation, food supplies, weather analysis, and mineral resources, among other things. And in biology, Big Science is emerging in the form, for example, of genetic experiments involving multitudes of test animals, and biomedical applications can be readily envisaged. So Big Science is becoming applied as well as basic, and the criteria involved in making choices among competing Big Science programs have to include the comparative social significance of missions as well as the respective intrinsic scientific merits.

Some Big Science—for example, a large-scale social survey—can be organized without requiring any single huge instrument or other great capital investment. But in oceanography, astronomy, and high-energy physics major facilities are required and raise capital investment questions not met elsewhere in basic science. As the recent competition among 85 locations for the next-generation accelerator showed, some extremely non-scientific influences loom large in planning for Big Science facilities. The assumption that a major scientific installation will in some (almost mystical) way contribute greatly to a region's economic growth through the attraction of high-salary, high-technology industries leads to vigorous political struggle over who will get which Big Science facilities. Further, the costs of such facilities are sufficiently great to make an impact even on today's large federal budget; more particularly, anything that will cost $100 million or more seems inevitably to become a matter of political hassling. Take Project Mohole, for example. When its costs were estimated at $5–$20 million no one but NSF and the scientists involved were much concerned. When estimates reached $120 million and a contract of $35–$40 million for a drilling platform became involved, political hell broke loose.

Non-scientific criteria thus loom large in the political choices in which Big Science becomes embroiled. Its environment is that of the larger world, not just the internal world of science. At the same time, choices here perhaps involve a more manageable number of variables than in the complexities of little science. The number of agencies concerned with any one Big Science program will be small; the educational dimension is minimized numerically; and in at least some Big Science fields there does not have to be a concern with utilitarian payoff, for none is expected. Concentration can therefore be placed on the field itself, and on the state of the federal budget. Planning is generally simpler, in at least some respects, when a few big decisions have to be made rather than a continuing series of diversified smaller decisions. And Big Science facilities do have to be planned for, both because of their costs and because by their nature there can be few of a type. Every campus cannot have its own accelerator or oceanographic vessel. Rather, a few national centers are constructed, and an essential element of the operational planning is the provision for access to those centers by investigators from a large number of home institutions, as well as the gathering of a permanent center staff. The managerial arrangements—the prototype is a nonprofit consortium of universities—must be planned as well as the physical facility itself.

Such planning of capital equipment and its organizational blueprint does not occur spontaneously or as the simple result of proposal pressures. Conscious administrative decisions are required, decisions that include but are not limited to scientific considerations as such. The decision-making framework for Big Science therefore must be more tightly knit than that for little science. And it must be competent in management, budgeting, and administrative politics as well as in the science itself.

One of the big differences between Big Science and little science is that in the latter area one can contemplate supporting all fields to some extent each year, but in the former there has to be a "waiting list" of investments, for the costs preclude simultaneous funding of all scientifically worthwhile projects. This means a much tighter competition, and a more urgent problem of establishing inter-field priorities. Which in turn means that reviews transcending disciplinary panels are more essential. Consensus within a field on its next Big Science facilities need is but the first step. Impact on adjoining fields, which Weinberg stresses, has to be assessed by a group broader than those immediately concerned, and then OST-BOB level judgments have to be made to establish an inter-field rating. Given the size of Big Science budgets, such planning has to be made over a number of years. Ten-year projections of facilities in relationship to expected field activities are needed, partly because large facilities have very long lead times between the initial go-ahead signal and the point of operability.

Because Big Science priority setting involves saying "no" to whole fields in a given year, it will be even harder than in little science to find any *a priori* substantive criteria on which all participants would agree. The integrity of the institutional process by which the decisions are made, and in particular an assurance that all affected parties are represented in the decision-making, becomes more important than the criteria themselves. Carl Kaysen proposes a high-level committee representing both the affected sciences and the funding agencies, to scrutinize each year the entire shopping list in relation to available funds. The funds he would set at some fixed proportion of total basic research expenditures, perhaps in the neighborhood of one-half. His description of the proposed committee's functioning merits extended quotation:

> The evaluation process in practice could be expected to involve a fusion of two elements. The first is an element of professional judgment as to the "ripeness" of particular projects, in terms of their ability at that time to make a substantial contribution to the forward progress of the particular science in which they were to be used. The second is an element of political compromise, so that neither any single science nor any single group of closely related problems is the recipient of the lion's share of the funds over any period of years. This political element is necessary. In the absence of an objective standard for judging whether, say, particle physics ought to be developed faster than radio astronomy, and in the presence of a budgetary constraint, the allocational decision must inevitably represent somebody's preferences or prejudices. The present proposal incorporates an explicit mechanism for registering the preferences and prejudices of those who are both affected and knowledgeable. The mechanism is broadly a representative one, though informally rather than formally such, and similar in character to consultative organizations in other areas, and which the Government relies for advice with respect to decisions affecting the interests of particular small groups in the society. The result of this process would be an approved list of projects, which typically would involve funding commitments over a number of years, and which in total would fall within the limits of the formula, with the exceptions set forth above.
>
> The user representatives on the allocating committee might include people drawn from the President's Science Advisory Committee, the National Science Board, and the

> outside scientific advisory groups to the various operating agencies; while the fundling agencies might be represented through the Office of Science and Technology. Behind this top committee might well be a structure of appropriately chosen subcommittees screening the requests from each field of science. Special ad hoc arrangements might be needed from time to time to deal with proposals cutting across traditional fields.[8]

Between this category and the preceding one of little science there is bound to be some budgetary competition. One accelerator's construction and operating costs equal the support of hundreds, even thousands, of individual campus researchers. OST and NSF need to be alert to the trade-offs between Big Science and little science, to ensure that the demands of the former do not impinge too heavily on the latter. Perhaps NSB-NSF representation on Kaysen's committee would sufficiently ensure representation of the little science viewpoint.

One final point: why is Big Science given the lowest priority in this scheme? There are two reasons. First, its cost in relation to little science, and the fear that a higher priority for it would squeeze out too much of the latter. Secondly, Big Science is less closely involved in the educational function of science support. Because its installations are often national research centers set apart from campuses, and because the number of scientists using any one Big Science facility is comparatively small, no great number of graduate students are trained through these facilities, compared with the training function of little science. Since we give high priority to the educational justification for science support, we give less to the area that contributes proportionately less in this regard.

In this chapter I have tried to suggest a set of priorities that accord with the values of the society as patron, and which fit not too badly with the justifications that scientists themselves have offered for federal support. And I have tried to sketch some of the considerations involved within each category, to show that the setting of priorities cannot be done in the same way for every area of science policy, but must be fitted to the circumstances of each.

The reader who rejects the priority order given to the four categories may still find the categories themselves useful in organizing thought about the priorities problem. This is certainly the most intractable problem in federal science policy, one that no one has wanted to face. It has only recently, therefore, begun to receive the attention it demands. Much more hard thought by both scientists and administrators will be needed before a satisfactory priority system is developed.

HARVEY BROOKS

The Problem of Research Priorities

SINCE THE PEAK of federal science funding was passed in the United States in about 1966, there has been evident a growing gap between research opportunities and financial resources in science. In academic science available support measured in dollars of constant purchasing power per eligible active investigator has declined by more than a factor of two.[1] Such a development was inevitable eventually, and had been predicted by many, but it burst upon the scientific world as a surprise after centuries of nearly uninterrupted growth of science and technology. Of course, during most of this time the scientific community was so small that its claim on economic resources was scarcely noticeable, so that science could advance almost independently of the state of the economy. Even in the Depression of the 1930s the national scientific enterprise continued to grow in both funding and employment, in each year of the decade except 1932–34. The average annual real growth was between 7 percent and 9 percent between 1930 and 1940.[2] Thus, an actual decline in support and manpower is a relatively new phenomenon, even though particular institutions, such as government laboratories, have suffered severe temporary cutbacks.[3]

When science funding was increasing, questions of priority received little overt notice because worthy new projects and new investigators could be supported with little detriment to work already under way. It is only recently that competition among projects has become so severe that the scientific community has become aware of many worthwhile projects which could not find support and many able scientists who could not find professional opportunities commensurate with their training. Some of this, of course, is part of the general phenomenon of expectations rising faster than resources in our society, but some of it represents a real constriction of resources in relation to opportunities. As a result, there is a feeling that the cost to scientific progress arising from wrong choices of scientific direction may be considerably higher than in the past.

Priorities in science cannot be set without bringing in considerations external to science itself, especially if the projects compared are only distantly related. There is nothing inherent in scientific logic which can say that molecular biology is more important than elementary particle physics, or cosmology more important than evolutionary biology, although some distinctions can be made on criteria such as degree of generality, philosophical implications, or elegance and simplicity. These, however, are primarily aesthetic rather than rational criteria. In the past, the relative priority accorded to many disciplines, such as physics or electrical engineering, has resulted from their connection with certain national "political" missions, such as defense or the development of nuclear energy. Although this connection may originally have had an intellectual justification, the evolution of the field has in many cases outgrown this connection. Low energy nuclear reactions, for example, were rather closely related to nuclear reactor and nuclear weapons design; but out of this early work in nuclear physics there evolved the subject of high energy particle physics, whose connection with energy was limited to the use of the word "energy" in labeling the field. The changing priority of national missions has been reflected in the allocation of resources to the basic scientific fields originally attached to them. Thus it is that real support for the physical sciences has dropped by nearly a factor of two since 1967.[4] Public disenchantment with the military-space-atomic energy syndrome of the past quarter century has thus been visited on the fields of pure science which rode on its coattails for so long. When the only public function to which the electorate seemed willing to devote large resources with no questions asked was national security, it was natural to justify the support of science under this label. The magnitude and complexity of the defense budget, and the tendency of Congress to concentrate its attention on the big hardware items, meant that research received little detailed scrutiny, and this became a device for protecting the internal autonomy of science. As the subject which benefited most from association with the military-space emphasis, physics is now the field that has suffered most from public disenchantment.

In a time of reordering of social priorities, it is natural for the public to question priorities even in basic science, often without understanding the looseness of the connection between the intellectual and social priorities. The defense–space umbrella which protected some fields of basic science now serves as the basis for questioning their continued priority.

Another pressure for the establishment of scientific priorities arises from the belief that science is inherently "rational" and that therefore science, of all activities, should be most capable of developing rational criteria for choices among fields. Why shouldn't the setting of research priorities be as "scientific" as science itself?

One part of the problem is, of course, the confusion between "science" and the total federal R & D budget, which are often treated as interchangeable terms in popular and newspaper discussion. Most public discussion of priorities is in terms of large technological projects such as the SST, the space shuttle, or the fast breeder reactor, although such technological projects have only a peripheral relation to the development of the scientific disciplines, despite the fact that they consume more than 80 percent of the funds expended on R & D. The argument has been still further confused in the last two decades by a tendency to pursue some major technological projects largely for their own sake, rather than for a clearly defined social purpose. Atomic energy and space technology were pursued for a variety of social purposes which tended to change with time. The technical content of the programs was much more constant than their stated social purposes. For a while, it appeared that even the ocean sciences would be pursued for their own sake in their own special federal agency.[5] Such efforts might be classified as "pure technology," by analogy with pure science.

They were technologies pursued in an integrated way in terms of their own inner logic for a variety of loosely defined political purposes. The confusion is even further compounded by the large component of purely technical apparatus development associated with some areas of pure science such as high energy physics or space astronomy. How is the layman to distinguish between the importance of a manned space flight and the importance of a giant particle accelerator? Yet, in the eyes of the scientific community the accelerator is only ancillary to the asking of a fundamental question about nature, while in the case of manned space flight, scientific knowledge is a mere by-product of an activity pursued with a largely political purpose.

One of the central issues of the growing debate about scientific priorities is who should be involved in the process, and how? To what degree should the scientific community itself have central responsibility for the determination of the broad strategy of science as well as its more detailed tactics? Increasingly, there has been a debate between the public and its representatives on the one hand, and leaders of the scientific community on the other, as to where the line between strategy and tactics should actually be drawn.

The scientific community perceives that the strategic decisions about science are governed by budgetary and economic considerations and by political appeal, and that the views of working scientists receive a hearing only at the margins. The needs of the disciplines, for example, are seldom weighed against the requirements of "pure technology," as became apparent in the battles between ground based astronomers and the space agency a few years ago. Scientists watch hundreds of millions of dollars committed to technological spectaculars such as Apollo or the SST, while major scientific facilities are underutilized for lack of a few million dollars of operating funds.

On the other hand, political administrators point to hundreds of millions of dollars expended on biomedical research by the National Institutes of Health (NIH), largely under the control of study sections and advisory panels made up of scientists whom they view as beneficiaries of these same expenditures. They observe strategic choices being made on the basis of criteria which, although they allude to the cure of disease, are governed more by the internal logic of scientific inquiry than by the possible use of the results. Many politicians and some "nonestablishment" scientists believe the priority setting processes of science to be dominated by self-serving elites oblivious to democratic accountability and larger human concerns.

This particular set of issues has come to a focus in the recent debate over the priority given to molecular-level investigations in biomedical research and more specifically in the debate between the "environmentalists" and the "molecular biologists" in respect to priorities for cancer research.[6] In Europe, the issue is being joined even more sharply in the agitation over public participation in the formulation of "science policy," as described by Dorothy Nelkin.[7]

The Total Resources of Science and Technology

A national science policy is a policy for the allocation of scientific resources. It can be discussed in various domains ranging from basic science or academic science alone to the whole field of research and development, or indeed in the total process leading from research to fully commercialized or deployed technology. Technological, managerial, and social innovation form part of a seamless web; categorization of any part of the process as a separate activity is partly artificial, and for the same reason, a clear separation between social priorities and scientific priorities can also be only approximate.

A number of observers have pointed to the lag in United States' expenditures for R & D during the last decade in comparison with such expenditures in other advanced industrial countries.[8] In part, this may be closely tied to a lag in capital investment by United States' industry, which in turn reflects both a decline in rates of personal saving and a decline in after-tax profits of industrial firms. Thus, estimates indicate that between 1965 and 1976, profits after tax, with adjustment for inflation and also with fixed capital adjusted for its replacement cost, dropped from 11.8 percent to 4.8 percent.[9] Since the results of most industrial R & D are embodied in investment goods rather than consumer goods, it is scarcely surprising that the decline in investment is reflected in declining industrial R & D. Similarly, in government, emphasis on social programs, which are analogous to consumer expenditures, has been reflected in declining federal R & D investment in real terms.[10]

One of the central issues of science policy is what proportion of the national product should be invested in R & D, and how this should be divided among basic research, applied research, and development. Whether in fact this is a meaningful question is a matter of intense debate. Many industrial research directors, for example, stress the artificiality of the distinction between basic and applied research so far as industry is concerned, and administrators of medical research often take a similar view. In the 1960s, the director of the NIH tried to avoid any classification of biomedical research as basic or applied. Similarly, many students of science policy argue that the aggregate R & D cannot be determined apart from the particular menu of social and political goals that specific R & D programs are intended to support.

There is, however, at least one sense in which total resources going into R & D are meaningful from a policy standpoint. At some level of expenditure, the productivity of the expenditure is limited by the supply of talent available. This situation may have obtained during the period from about 1955 to 1965, when salaries of scientists and engineers rose appreciably relative to wages and salaries generally, there was a large influx of scientific and engineering manpower from abroad, and many technicians and skilled workers in industry were upgraded to engineers despite a lack of formal educational qualifications.[11] But since the midsixties, R & D activity has been limited by financial rather than human resources, as indicated by a steady fall in relative salaries, as well as by the statistics on support per investigator mentioned previously.

A possible science policy would be one which treated scientific and technical manpower as the scarcest resource. Spending on research and development, or possibly on various subcategories of R & D, would then be based on the amount of talent in each category, and the question of priorities would reduce to the question of the allocation of talent. In support of this approach, one might quote the substantial body of economic research that indicates that the average social returns to R & D are considerably higher than returns to investments in physical capital.[12] By this argument, it would be advantageous to invest more money in R & D provided a sufficient level of talent is available and the mix of projects is chosen so as to maximize the total social return within a given level and mix of manpower.

The foregoing line of argument seems to presuppose a rather sharp threshold of talent and motivation for scientific research. In this view there is a pool of such potential talent in each age cohort whose proportion of the cohort is quite inelastic with respect to available research funds, provided quality control is retained through peer review. The contrary view is that there is a continuous grading off of talent and motivation and that, in the long run, as more money is invested in R & D, the talent drawn into it is increasingly marginal, and it is impossible in practice to define the point of diminishing returns. The peer review process itself tends to modify its standards to match the available talent.

A second argument against the manpower allocation view is the simple economic argument that research on the productivity of R & D investments has given evidence only on *average* returns, but what is significant for deciding on increases in investment are *marginal* returns, i.e., the incremental return per incremental dollar invested. This economic argument is especially cogent if indeed there is not a sharp threshold of qualified talent.

These arguments tend to be supported by the observation (particularly in basic research) that research productivity among scientists is highly skewed; most of the significant research publications are produced by a small minority of scientific workers.[13] Derek Price has argued, for example, that aggregate scientific output tends to rise as the square root of the number of active scientists,[14] indicating that marginal productivity goes inversely with the square root of the total number of scientists engaged.

The above criticism might be partially answered by adding a constraint on the average salary of scientists and engineers. The policy would be to keep the total technical work force fully engaged without a rise and fall in the average "price" of technical people relative to other forms of labor. The work of R. B. Freeman suggests that in the private economy the price elasticity of technical employment is quite high[15] in the long term, about 0.7 (that is, a 7 percent decrease in employment for a 10 percent increase in average compensation).

In a manpower allocation policy such as that sketched above, federal R & D expenditures would serve primarily as a "balance wheel" to private R & D. Total federal R & D support would be adjusted gradually to bring the total technical man years of effort to some target value related to the total number of trained people available. The problem of priorities would then consist of the selection among competing projects that could be carried out with the residual pool of manpower not fully absorbed by private industrial R & D.

In contrast, the present de facto policy in the United States is that the scarcest resource is not technical talent but discretionary dollars in the federal budget. The limit on aggregate federal R & D expenditures is set by complex trade-offs among competing demands for the federal dollar, especially social welfare and military spending. When competing demands were relatively low, and available revenues rising rapidly, as in the early sixties, R & D expenditures rose so rapidly as to place inflationary demands on technical resources. By contrast, in the seventies, discretionary federal dollars were eroded by competing demands and failure of federal revenues to keep pace, and this resulted in many critical scientific and technical resources being left underutilized. Because of the cumulative nature of scientific and engineering skills, including the long educational path, as well as their rapid depreciation with disuse, the R & D system cannot adjust efficiently to changes either in priorities or in total expenditures. Hence, the underutilization of technical manpower may have a particularly high social cost.

Indeed, the trade-offs tend to be made on a sector-by-sector basis rather than overall; thus, defense R & D competes with defense expenditures in general; agricultural R & D with the Department of Agriculture expenditures in general, and so on. In fact, the trade-offs are so many-faceted that it is probably misleading to speak of them in terms of any single binary competition.

Both the manpower and fiscal models of aggregate R & D expenditures imply global R & D budgets, the one expressed in terms of technical man years of effort, the other in terms of money. Resources are to be suballocated in such a way as to maximize some sort of output or a weighted combination of outputs constituting a single-dimensional criterion of merit. In the manpower allocation, it is assumed that, because it cannot fully capture the benefits of research, the private sector will tend to underinvest in R & D from the standpoint of maximizing the social benefit potentially derivable, and therefore government supplementation is required.

Most proposals for allocation of a global research budget focus either on academic research as an aggregate or on basic research, assuming that applied research and development activities will reach a natural level determined by competition with other operational social objectives. The argument for singling out academic or basic research is that its success is more dependent on long-term stability of support. However, it is impossible to ignore the impact of a fluctuating external employment market on the conduct of academic or basic research. Thus, there is some requirement for overall stability in the R & D enterprise as a whole.

The manpower allocation model corresponds rather closely with the practice in the Soviet Union, where the output of the system of advanced education is fine-tuned to the number of jobs available in the whole R & D enterprise, which in turn is funded according to a gradually increasing total determined under the current five-year plan for research and development. Thus, the entire Soviet R & D system is based on what can be described as a "level of effort" approach, with the total funding being adjusted to keep the scientific work force fully occupied, something which is possible in the Soviet system because the entire enterprise, including salary scales, is closely controlled centrally by the government. Planning for the agricultural research system in the United States follows a somewhat similar pattern. The problem with such a system is that it tends to lead to rather low mobility and flexibility. The level of support is tied to institutions whose budgets are quite predictable from year to year, and the allocation of resources within the institutions is made centrally in terms of only gradually changing priorities.

The manpower model has several attractive features, but runs a large risk of inflexibility. If such a system of allocation is to retain vitality, a scientist might have greater assurance of productive employment than at present, but should probably have less assurance of employment in exactly the place or activity he preferred. There would have to be greater adaptibility to changing priorities. In the Soviet system, it is very difficult to gain access to the system, which is very competitive in its early stages, but once access has been gained, a scientist who succeeds is likely to stay in the same institute and career line for his entire working life. In the United States, access is easier, but forced exit is much more probable, and the frequency of mobility and career change has been much higher, at least until recently. During the period of tightening funds and the disappearance of university expansion, a more complex pattern may be emerging. Insecurity and mobility may have increased in the early career stages, but the institution of tenure, combined with the rapid expansion of the recent past, may be introducing a rigidity which did not exist earlier. Although the system has a high degree of adaptability, many feel that it produces too much wastefulness at the margins. There is a growing consensus in the United States' scientific community that a better compromise between stability and flexibility is possible and desirable, especially for the first decade or so of a scientific career.

Truth versus Utility

The federal government supports research for essentially pragmatic reasons—because it is an important means by which agency goals can be achieved more effectively or more efficiently. In other words, social utility is the primary criterion for federal support, and for choice among areas of support.[16] The operational consequences of this principle, however, are by no means obvious. For one thing, the scientific and technical enterprise needs a certain measure of internal autonomy in order to pursue even the most pragmatic goals. This is true because the ends we can pursue are constrained by the possibilities presented to us by nature, and in the words of Charles Fried[17] we cannot affirm "truth as a constraint on the pursuit of the useful, while denying truth any power to set its own agenda." In addition, "only the pursuit of truth for its own sake can develop and maintain the standards which are necessary to ensure the objectivity and the value of any research, pure or applied."[18] Even in the most applied research activity, "once the search is underway, utility is bracketed and reality must be the goal, less the desired utility itself never be reached."[19] These principles, in fact, express the implicit agreement between science and society which was first formulated by Francis Bacon, and which has served well both the growth of knowledge and the realization of its utility.

The frequently demonstrated usefulness of "useless" research is sometimes used as an argument against any kind of social guidance of research. Like the "invisible hand" of the classical free market, it is argued that the autonomous working of the intellectual free market of ideas will produce socially optimal results at the least cost. The most eloquent exponent of this view was Michael Polanyi, who coined the term "republic of science" as a symbol of the idea of scientific autonomy.[20] But this concept scarcely seems viable in a polity in which a large fraction of the resources invested in the enterprise of science comes from the public, and in which the total investment is as large as it is in the modern industrial states.

By now, most politicians concede the necessity for some degree of self-governance and internal agenda setting in the scientific community, and most scientists concede the necessity for some political and public input to the setting of the general directions and goals of scientific research. The issues are where the lines should be drawn and the appropriate processes by which the scientific and political communities should mutually negotiate the scientific agenda. For the most part, it is conceded that scientists cannot determine social goals, and politicians or the public should not determine scientific methods or tactics, nor influence conclusions. The problem is how to reach agreement on what constitutes ends and what constitutes means, where the line between strategy and tactics in science is to be drawn. The problem is frequently formulated in terms of the relative roles of internal and external criteria in setting research priorities.

The institutional expression of science's need for autonomy is the so-called peer review process. In principle, this is a process for using expert judgments to select the best projects within a defined field of science in terms of "scientific merit," that is, in terms of criteria internal to science. The cardinal principle of peer review is that judgments of scientific merit should be made entirely by scientists, and moreover by scientists who are active in the same field of research or one closely allied to that proposed by the investigator. Even in the case of institutionalized research in government and industrial laboratories, the fundamental judgments regarding the selection of scientific personnel are made by peers, especially in the most productive laboratories. In this paper, I intend to assume that peer review does indeed select the "best science," and that there exist more or less universalistic criteria of scientific merit which any competent group of scientists will agree upon, at least in application to concrete scientific proposals. There appears to be ample empirical evidence from recent research by sociologists of science that such "objective" criteria of scientific choice do exist; therefore, the merits of this proposition will not be debated further here.[21]

The legitimate controversy over the merits of peer review applies not to its validity in rank ordering projects or scientists within a field, but to its broader use in the formulation of science policy and the allocation of resources *between* fields of research. Peer review is a process which operates most successfully when it is used only in the "truth" dimension and not in the "utility" dimension of scientific choice. In fact, it is common knowledge among students of science policy that peer review has been much less satisfactory when attempts were made to use it to evaluate proposals in applied research, for example, the RANN (Research Applied to National Needs) program of the National Science Foundation[22] or in some of the more clinically oriented programs of the NIH.[23] Even when the practical goals of a research project are agreed upon, there is usually much less consensus among reviewers concerning the merit of a scientific proposal. Frequently the viability of the practical goals themselves is also a bone of contention among different reviewers.

There is another important difficulty with peer review, and that lies in the definition of a field of science. Generally speaking, the broader the intellectual territory covered, the less consensus there will be on the ranking of a group of proposals within that territory. Also, the consensus is usually greater at the extreme upper and extreme lower ends of the quality spectrum. But in the middle ground, which constitutes the majority of projects and scientists, the disagreement tends to widen as the field to be covered widens. Furthermore, many fields of science can be categorized in a way which implies either a predominance of internal criteria or a mixture of internal and external criteria.

Thus, solid state physics and materials science embrace a large area of overlap in actual subject matter, but projects in solid state physics can be judged almost exclusively in terms of truth criteria, whereas, the term materials science may imply a considerable element of utility in judging merit.

In fact, the categorization of subject areas in science presents many opportunities for controversy with respect to peer review; a field is often given a useful sounding label in order to increase its political attractiveness, whereas peer judgments would have been more unanimous if the field had a "pure" label.

However, the peer review process relates to the setting of priorities among fields of science in an entirely different way. It is frequently used as a signaling mechanism to indicate to program administrators where the most significant opportunities lie within a much broader domain of science. This comes about through "proposal pressure," which represents what the scientific community has pluralistically decided is most worth doing, with some sort of additional weighting by the quality of the ideas and people involved in specific proposals as judged by peer reviews. Since the selection of senior scientists in universities itself involves a much broader comparison among distant fields, the proposal pressures coming from university investigators reflect these broad comparative judgments within the academic community.

Of course, proposal pressure can be used as an indicator of scientific opportunity only with a considerable judgmental factor added, for there are many factors which tend to distort it. For example, the mere announcement of the availability of money for certain kinds of research will attract proposals, and this effect will be much enhanced if there is a general shortage of research funds or if funds are only available for a limited number of research areas, with many gaps. Researchers will tend to go where the money is, but if money is more or less equally available in a large number of fields, then those fields with the most significant scientific opportunities will tend to attract the largest number of high quality proposals.

Proposal pressure is also difficult to interpret when projects differ greatly in size or require coalescence of the activities of large groups of people, as in large national or international programs such as the International Biological Program or the International Decade of Ocean Exploration. Although many of these programs involve individualistic subprojects, they have to be closely interrelated and require the use of common facilities and logistic support, and hence, a considerable measure of central planning. This planning is usually done with the aid of committees of scientists closely involved in the general field of work. In some degree this may be regarded as an extension of proposal pressure in the sense that the planning groups are engaged in the identification of scientific opportunities which are thus signaled to administrators.

Another problem with the use of proposal pressure is its tendency to reflect momentum generated in the system by previous actions. Earlier support of certain fields of research generates personal, institutional and even bureaucratic commitments within the supporting agencies which tend to become self-perpetuating. In other words, prior support itself generates proposal pressure which might then be interpreted as an indication of a need for additional support. Moreover, support results in the training of students who may then become future claimants for support in the same field.

One can imagine large numbers of very high quality proposals in a particular field, each one of which makes very good sense by itself, but which are sufficiently alike in objectives and subject matter so that supporting all of them would have dubious justification in terms of the overall balance within a much broader field of endeavor. If all similar proposals were considered simultaneously by the same set of reviewers, the overconcentration of effort might become apparent, but, in practice, proposals are usually received at different times, often by different agencies, and are reviewed by different referees. Comparison of projects tends to occur more automatically with large projects involving expensive instrumentation. A sophisticated program officer or advisory committee can make allowances for the effects of overconcentration; hence, it is difficult to know in practice how serious the effect is. Systems of peer review, such as those used in the NIH study sections, where a committee reviews a large number of proposals at the same time, are probably more effective in avoiding overconcentration of research effort on fashionable topics than systems which involve only individual mail reviews carried out at different times.

Charges have been made that the project system tends to favor "safe" proposals and to reject "off-beat" but highly original projects which do not fit accepted paradigms in the field. It is also charged that it favors experimental work for which there is already a well-developed theory at the expense of "long shot" experiments not favored by theoreticians. It may also favor projects which promise quick, publishable results at the expense of projects requiring much longer to come to fruition. Again, these are hazards which can be compensated for by sophisticated scientific judgment of program administrators and advisory groups, but there exist few data to indicate the extent of validity of such criticisms in practice.

In theory, proposal pressure can be used to compare widely distant fields, since it does not involve much substantive comparative analysis of the scientific content of projects, but only a general accounting of numbers and dollar amounts of proposals previously evaluated as of high quality. In practice, the sociology of research tends to vary from field to field, so that proposal pressure may be difficult to interpret on a comparative basis. Thus, a given number of proposals in organic chemistry may have quite different significance as to the needs of the field from proposals in, say, molecular biology. Proposal pressure is also subject to manipulation by an entrepreneurial program officer who can simply go out and generate proposals among his constituency in order to prove the neediness of the area of science for which he is responsible. However, abuses such as this are usually fairly evident to higher level management in an agency, especially if the strategic decisions are made by managers with scientific training and experience.

All of the considerations above suggest that proposal pressure is an imperfect indicator of the grass roots priorities of the scientific community, and cannot be used as the major guide to the allocation of resources, especially among widely different fields of science. On the other hand, it does constitute an important signaling system that should not be ignored in the priority setting process. When interpreted by scientific administrators with good judgment and wide knowledge of both the substance and the sociology of the scientific enterprise, proposal pressure can be a better indicator than might be supposed. For example, if money is pumped into a new field, and the quality of the resulting proposals is very low, as often happens, this is a caution light on further increased support that ought to be heeded, no matter how politically appealing the field. Conversely, the first indication of emerging new areas of science often comes as the result of unusually imaginative and original research proposals submitted by scientists with outstanding records of prior accomplishment in adjacent areas of research.

The use of the peer review system as an indicator of research opportunities places emphasis on internal criteria, on truth rather than utility. Its use for priority setting implies an implicit priority for the "best science" regardless of considerations of social benefits or consequences. It must be recognized that any other scale of judgment implies a conscious decision to support less good science at the expense of better science for reasons lying outside science.

The question then arises, what should be the role of scientists in dealing with the utility dimension of priority setting, and indeed what should be the role of the public? Clearly, the special skill which scientists can bring to bear is the capacity to predict the probable or possible consequences of proposed research. Although scientists are the only people with the requisite knowledge to do this, it is still an unaccustomed role, and they do not do it very well. When scientists are asked to defend their basic research in terms of utility, the result is frequently naive and forced, a job of salesmanship rather than thoughtful analysis.

Furthermore, the role is a peculiarly difficult one, involving an inherent conflict of interest, especially in a period of constricted research funding. The incentive to oversell basic research on grounds of its utility becomes greater as the competition for research funding becomes keener. At the same time, the excesses of a few entrepreneurs may ultimately discredit the whole exercise of forecasting the consequences of research. The problem is exacerbated by the attitude of the news media which tend to overemphasize highly speculative or uncertain applications of basic research, or, more recently, equally speculative and uncertain adverse consequences.

Furthermore, as we move from applied towards fundamental research, we soon reach a point where the probable results of research, as well as the possible practical consequences of a variety of possible outcomes, become so uncertain and so diverse that the only honest thing we can do is to fall back on a generalized faith in the average beneficence of new knowledge. The problem is that any one practical consequence is so improbable or speculative that it would be dishonest to use it as a justification for supporting the research, and yet the sum total of possible benefits, each rather unlikely by itself, may add up to justify it. In many cases, the information obtained which may lead towards a better future assessment of benefits or adverse consequences of possible applications may be the principal justification. This is essentially what Charles Fried means when he says that truth must be allowed to set its own agenda.

The difficulty today is that neither scientists nor the public have the former unqualified belief that all knowledge is ultimately to man's benefit. In the last several decades, too many new technologies have emerged whose benefits are ambiguous, to say the least, and critics are quick to point out that, although science may not have "created" these technologies, they could not have been created without the spectacular advances of scientific knowledge in the twentieth century. Perhaps one of the best examples is modern chemistry, which has only recently come to be seen by some critics as the ancestor of many industrial products that pollute our environment. As long as one was confident that any piece of scientific knowledge would have beneficial consequences with, say, a 90 percent probability, one did not have to concern oneself with forecasting the consequences too accurately. But this confidence no longer exists, whether justifiably so or not. Pessimism about the consequences of knowledge is part of the general climate of our times, and although scientists are more optimistic than most, more and more of them share in the general pessimism. Indeed, there seem to be as many scientists ready to search out uncertain and highly speculative adverse consequences as there were scientists a few years ago ready to exaggerate the potential benefits of research. In part, perhaps, this is the result of the fact that an increasing fraction of research is "defensive," i.e., devoted to the identification and avoidance of harm rather than to the creation of products and processes intended to satisfy a potential market. Thus, there is a growing domain of research which might be said to have a vested interest in the exaggeration of disbenefits, just as many scientists have had a vested interest in the exaggeration of potential benefits.

There is a complementarity between our ability to assess scientific merit and our ability to assess utility. For a project whose utility is very difficult to assess, and highly speculative, scientific merit is usually easy to assess, and is amenable to peer review. On the other hand, science which is more "applied" may be easier to assess from the standpoint of utility but harder to assess for scientific quality. We have already seen that the process of peer review has run into serious difficulties and criticism in the applied area. There is a large gray area in between "pure" science at one end, and highly targeted applied research at the other, where the difficulty of assessment arises primarily from the inability to agree on the relative weights to be assigned to truth and utility in evaluation. For example, there is much useful research which is regarded as very pedestrian when weighed on the scale of scientific quality. An example is the testing of chemicals for toxicity, the carrying out of chemical analyses, or some kinds of work in systematic biology. These gray areas are usually battlegrounds for academic scientists and scientists from industry or government, with the latter tending to assign greater weight to utility.

Unfortunately (or perhaps fortunately) the complementarity between usefulness and scientific merit is not complete. Research undertaken primarily for purposes of utility frequently turns up fundamental issues which lead to research projects of high scientific merit by anybody's standards. An example is some of the crystallographic and metallurgical work carried out in connection with transistor development. Conversely, very fundamental work leads to vistas of applied work of obvious and great utility, as in the case of the laser. Particularly in the fields of materials science and medical research the interplay between utility and truth has been extraordinarily fruitful.

But in cases where utility is difficult to assess and highly speculative, it is probably better to ignore it altogether as a basis for priority setting. Conversely, where scientific merit is difficult to assess, but utility is clear and certain, one should probably drop scientific merit as a basis for priority setting. This may be what happens in practice, although it is always difficult to "sell" research of no apparent utility. For example, high energy physics and galactic astronomy have been supported on a rather generous scale for decades primarily on the basis of criteria of scientific merit, although there have been occasional arguments that the instrumentation development brought about as a by-product of research in these fields has turned out to have high utility. Still, one would probably not have supported the research primarily for the sake of this "fall out" in instrumentation.

It is thus probably in the gray area in which both truth and utility play a role that the greatest effort needs to be made in improving our ability to assess consequences, and in raising the level of discourse between the scientific community concerned with truth and the political community concerned with utility. Biomedical research is a good example of this kind of area, and the dialogue has begun to be joined.

Who Decides?

Of course, the public, and its representatives through the political process, are the ultimate arbiters of research priorities. But that simple statement has little operational significance. In hardly any other field of activity is there a bigger discrepancy between constitutional and informally understood responsibilities. In practice, the public must delegate a large part of its constitutional responsibility to the scientific community, but for this to happen, the scientific community must have and deserve the trust of the public. For only the scientific community can really manage science. When the public steps in, through the political process, to manage science in detail, it not only destroys science but does not get the utility it is after. Furthermore, the capacity of the scientific community to subvert attempted political management is much greater than in most other fields of endeavor, but the more such subversion is necessitated by ill-advised political management, the greater the mistrust of scientists. This leads to a dangerous vicious circle. Hence, the formal decision-making responsibility for priorities in science is much less significant than the informal interactions by which the internal processes of the scientific community interface with the political and administrative process. This relationship is not very susceptible to discussion in terms of rights and privileges, or freedom and accountability. There is no alternative to a largely self-governing science which is nevertheless responsive to the basic aspirations and expectations of the polity. The debate hinges on what is basic and thus pertains to the political process, and what is tactical and thus pertains to the autonomous governance of the technical community. If trust disappears, then the public will take back the delegation which it has made to the scientific community during the last three decades, a delegation which was eloquently articulated in *Science, the Endless Frontier*.[24] To some extent, it is apparently already taking back some of this delegation. The question is how long the process will continue and how far it will go or should go in the best interests of both the public and the scientific community.

Clearly, one aspect of the relationship between the scientific and political communities is that it should not place too great strains on the intellectual honesty of scientists. If the cost of being honest about one's ignorance of the potential benefits of one's research is the total loss of support, then a kind of Gresham's Law will set in which rewards those scientists most willing to stretch the bounds of intellectual honesty in explaining the benefits of their research, or in minimizing its possible adverse consequences or uses. This is not a healthy situation, and in the long run can only stimulate the mistrust between scientists and the public. Some biologists are now regretting the haste with which they brought before the public their speculations regarding the possible consequences of recombinant DNA research. It is not that the issues raised were illegitimate or not properly matters of public concern, but rather that airing of the debate produced the reaction that the scientists had not gone far enough in advocating stronger restrictions. Similarly, many scientists and engineers in the field of nuclear energy feel bruised by public criticism which they think, justly or unjustly, was the consequence of their greater openness in discussing the details of nuclear technology in public than was characteristic of other older, more accepted technologies. Again the public concerns initially raised by a minority of scientists were not without foundation, but they could not have been so well argued or so well publicized without the existence of a much larger body of technical literature (about nuclear safety and the biological effects of radiation) than existed in almost any other field.

On the other side of the coin, the ever-mounting competition for research funds may lead to an extravagance of scientific promises which can only increase mistrust of science and decrease funding still further.

There is a sort of centralized model of priority setting in science in which it is argued that the political process should decide the social priorities of the country, and then the scientific enterprise should be deployed to match these priorities. To a small degree, this is what happens in the multiagency system for the support of science which we have in the United States. That is, the political process allocates money to health, defense, space, agriculture, environmental protection, and so forth, and then each executive branch agency with these social responsibilities is required to come in with a plan for the R & D which will support these social goals. However, means and ends are not so easily separable in the real world. The public does not know what it wants until it knows what it can get and how much it will cost. Social priorities are constantly being changed by the creation of new knowledge, while at the same time the search for knowledge is being partially redirected by social priorities. Within the last decade, for example, geophysicists began to believe that as a result of basic advances in their science, the prediction of earthquakes should be a possibility within the next decade. As a result, the federal government has created a new research program in earthquake prediction. This does not mean that earthquakes are regarded as any more serious a social problem than they were before. The public's priorities for earthquake research rose sharply only when advances in basic science revealed a new opportunity to do something useful which was not conceivable previously.[25]

This example illustrates why research priorities can only be arrived at as the result of a constant dialogue between the political process and the scientific community. Science not only helps in the fulfillment of social goals, but also generates new social goals, and the latter may be a much more important function than the former, though much less predictable. During the last decade or so, there has been a large acceleration of the dialogue between the public and scientists. In the early 1960s, when science in the political process first began to come to public attention through the creation of the President's Science Advisory Committee, the scientists' role was actually quite narrow and technical, mostly confined to national security policy. It was concerned with the assessment of alternative means to largely agreed upon ends, and even in the field of the nuclear test ban the context was largely the same as that for weapons systems. It is only in the last decade that expertise has entered much more pervasively into the political process, in the Congress as well as the executive. There are many scientists now on congressional staffs; the Congress has a science advisory committee in the form of the Office of Technology Assessment; the General Accounting Office has moved massively into the arena of technology and science policy assessment. The federal government and the Congress are swimming in technical studies on energy, environment, health research, nuclear proliferation, and hundreds of other subjects with a high scientific content. It is all chaotic and halting, not following any discernible plan, and it may seem wasteful and costly to the detached observer. But it is the first phase of an important process, which ultimately will promote a stronger connection between science and overall priority setting in the government, and not just the setting of priorities for R & D in isolation.

The process described in the preceding paragraph is primarily governmental, and still attracts little public attention. The other phase of priority setting relates to the direct role of the public in guiding research priorities—the much discussed subject of public participation. This topic has received much more extensive discussion in Europe than in the United States, though at a more theoretical level.[26]

It has come to the fore chiefly in connection with energy policy, where public concern about the long-term safety of nuclear energy has sparked a wide public debate about alternative energy sources and a shift in R & D priorities towards renewable energy sources and conservation. In this country, there is little question but that the very rapid rise in the ERDA budget for solar energy development and demonstration has been largely the result of popular pressures rather than a scientific consensus. On the other hand, it is easy to overestimate the thrust of public participation by itself. In most instances, such changes in priority as have resulted from public input have depended on political activism from an energetic minority in the scientific community. This is certainly true in the solar energy example.

Indeed, the principal effect arising from new mechanisms of public participation, as provided for example in the National Environmental Policy Act, has been to provide a new forum in which minority views in the scientific community itself could get a wider hearing, often bringing pressure on the scientific establishment to reconsider its own opinions and priorities. Much the same applies in the case of the public debate on recombinant DNA research. Without the participation of scientists, it is unlikely that this would have ever gotten onto the political agenda. Hence, in practice, one may say that public participation has provided an avenue for including a wider range of scientific views in the political agenda. This is probably a healthy development, but it would be misleading to attribute it primarily to public participation per se. It is rather a byproduct of the mechanisms created for public participation.

There is a danger here in the sense that insofar as public participation is simply an umbrella for quasipolitical debates within the technical community, the hidden political agendas of the technical actors on one or both sides of the debate may be overlooked because of the general respect in which scientific "objectivity" is held by the public at large. In the short term, this may lend a credibility to some scientific inputs that they do not deserve on their merits. In the long term, it may increase distrust of scientists as political actors, and lead to further invasion of lay judgments into the tactical, as opposed to the strategic, domain of scientific priority setting.

An illustration of this problem may lie in the field of research on aging. For a variety of reasons there has been a considerable upthrust of interest in this field in the last few years and, largely under public pressure rather than scientific influence, it has begun to be accorded a higher priority in health research. Yet, in all probability, this new thrust would not have appeared without a strong input from a small minority of the scientific community. The possibility of the prolongation of life has a great deal of political allure, and cautions put forward by other scientists concerning the possible social consequences of these practical results tend to go unheeded. Few in the scientific community would quarrel with the desirability of fundamental investigations of the physiological process of aging, such as investigations into why normal cells in the body cease to divide after a certain number of divisions. There could indeed be close relations between the phenomenon of aging and the cellular phenomena connected with cancer. But some scientists fear popular pressures may seize upon fundamental advances to push practical applications prematurely without careful assessment of the social consequences. In this case, one will have to question whether the alliance between a dedicated minority in the scientific community and understandable populist pressures will be in the long-range interest either of science or of the public.[27] Some will say it is better to bring this all out into the open and let the public decide, but others will question whether bringing in the public at this stage will not create undesirable risks.

Social Priorities versus Research Priorities

Much public unease about the present allocation of resources to federal R & D in the United States stems from an implicit dissatisfaction with political priorities. For example, 63 percent of federally supported R & D goes to space and defense activities, and even when privately supported R & D is considered, there is a heavy concentration in defense-related work. If development of nuclear power is added to defense and space, 71 percent of federal R & D is accounted for.[28] This fraction has declined from about 92 percent in 1963. Nevertheless, in 1978 the downward trend in defense-space-nuclear research was reversed; the largest *percentage* increase in federally supported *basic* research between fiscal year 1977 and fiscal year 1978 was in the Department of Defense. It is often pointed out that 25 percent of the world's technical manpower is engaged in defense-related research. Nearly one-fifth of all PhD physicists in the United States derive some research support from the Department of Defense.[29]

The question suggested by these facts is whether the present allocation is to be regarded as a distortion of R & D priorities, or whether it is to be considered as merely reflecting political priorities determined through the democratic process. Once we have decided as a nation to devote more than 7 percent of our GNP to space-defense-nuclear activities as a whole, the concentration of R & D in these areas follows almost automatically. All represent highly research-intensive activities compared with most other economic activity. Defense production, for example, requires almost ten times as much R & D per dollar of output than does civilian construction. Shifting technical manpower out of defense-related work would imply either heavy displacement of skilled manpower to nonresearch activities (or technical unemployment) or finding ways to make other civilian activities much more research-intensive.

Some critics of defense spending have argued that the size of the military budget is in part driven by R & D spending in the sense that research makes existing weapons systems obsolete and creates the need for replacement. At the same time, constant innovation, especially by the United States, in defense equipment helps to fuel the arms race. From this, it is argued that arms spending around the world can be gotten under control only by getting military R & D under control. By this argument, R & D priorities *do* drive political priorities primarily through the creation of opportunities that would not otherwise be selected by political leaders. I am doubtful that research is the main driving force for military spending; the argument is difficult to verify.[30]

Within the last two or three years, a very interesting debate has arisen concerning present priorities *within* the field of biomedical research. This debate has several dimensions. The one I want to consider as an illustration of the problem of setting priorities has to do with the relative roles of curative medicine and general environmental factors in improving the nation's health status. Critics of biomedical research priorities are quick to point out that there has been very little improvement in the health status of the American population in the last two decades despite the increasing resources devoted to biomedical research and despite the rising proportion of GNP channeled into health care delivery. The explanation usually given for this is that curative medicine has reached the point of diminishing returns, and that environmental factors, including life styles, diet, environmental pollution, social conditions, and even the quality of working life and the sense of self-worth in work, are much more influential factors in the determination of health than our ability to cure specific disease conditions.[31] Furthermore, most research on curative medicine relates to diseases of middle and old age, where the cure of specific diseases has only a small statistical effect on life expectancy. This critique of the biomedical enterprise comes to a particular focus in the case of cancer, where there is growing evidence of the importance of diet and of environmental contamination as a causative factor. Moreover, since the incidence of cancer increases rapidly with age, cure has little impact on statistical life expectancy, although the ability to cure diagnosed cancers has been improving at a steady rate of 0.5 percent per year over the last thirty years.[32] The implication of these observations for the research enterprise might be a rather massive switch of research effort away from the treatment of cancer and towards the study of environmental factors. More broadly, it implies a shift away from emphasis on the health care delivery system as such and towards environmental and social research having little obvious connection with medicine.

The criticism I have outlined does not question the efficacy of biomedical research when measured in its own terms. There has been dramatic progress in the understanding of specific diseases, and in the development of specific cures as in the case of polio, measles, tuberculosis, and many diseases of genetic origin. But this progress, it is argued, has been only marginally relevant to health. In addition, it has led to the creation of a high technology medicine which absorbs an ever growing proportion of health care resources without concomitant benefits in the reduction of mortality or morbidity. There has been little or no relationship between the effort in either research or care technology investment and the potential contribution to improved health. We have sought to push the frontiers indiscriminately in every direction that opportunity presented without reference to cost or to potential health benefits measured on a population basis. We could afford this opportunistic, or "technology push," approach when health care absorbed a minor proportion of national income and when choices of health technologies were made primarily in the marketplace, but in an era when health care costs are rising much faster than the cost of living index, and when a growing proportion of costs are being paid for through insurance or tax money, we can no longer afford to ignore priorities either in health care investment or in R & D.

The same issue seems to arise in health care as in defense. Is the expense and general direction of investment in the health care system driven by R & D priorities, or are R & D priorities driven by the nature and evolution of the health care delivery system? In other words, should the criticism summarized in the preceding paragraphs be directed at the research enterprise itself, or should it be directed at our overall social and political priorities in the health area? If our social priorities were changed, would not R & D priorities adjust more or less automatically to them? Does the momentum of the present system arise from R & D or from the health care establishment itself? Undoubtedly, the answer to these questions is not an either/or one. The coupling between R & D and the social delivery system to which it relates operates in both directions. The question is therefore whether changing R & D priorities would by itself have sufficient leverage on the system as a whole to effect the desired change in our social priorities. I am dubious that it would. Furthermore, one could argue that the current debate over social priorities in the health system is itself largely the product of research findings. A process of change may be taking place in health priorities, driven by research findings, but rate of change must be commensurate with the growth of our knowledge.

It must also be pointed out that as we look at *fundamental* research in the biomedical domain it is not so clear that the critique calls for a radical change in priorities. Probably the most important advance in recent years in our ability to detect carcinogens in the environment has come from the development of bacterial tests for mutagenisis, typified by the Ames test.[33] This is a development made possible by recent advances in fundamental biochemistry and cellular biology. Even for the future, it is not apparent that the shift of interest from curative to environmental factors in disease calls for radical shifts in the priorities for research at the fundamental level, although some such shift may be called for.

What the two examples discussed above seem to show is that the debate over R & D priorities cannot be conducted about R & D as an isolated and separate activity. Furthermore, I do not believe that R & D by itself has sufficient leverage on social priorities to make it a particularly fruitful area for discussion, especially if one is talking about priorities among research areas at the very fundamental level. Clearly, fundamental research should not be guided solely by internal criteria and changes in emphasis with respect to the technologies of health care, and more broadly health status improvement will and should affect relative emphases even at the fundamental level. Nevertheless, it would be a great mistake to think that we can have much influence on social and political priorities by conscious tampering with fundamental research priorities in accordance with external criteria. The most important activity is to maintain vigorous communication and feedback all along the chain between basic research and ultimate application to society's problems, but not to operate the research enterprise by "command and control" from the center.

REFERENCES

[1] Smith, B. L. and Karlesky, J. J., *The State of Academic Science: The Universities and the Research Effort* (New York: Change Magazine Press 1977), chap. II, pp. 9–48.

[2] Bush, V. et al., *Science, the Endless Frontier*, reprinted July, 1960, National Science Foundation (Washington, D.C.: U.S. Government Printing Office, 1960) table 1, p. 86.

[3] Weiner, Charles, "Physics in the Great Depression," *Physics Today* (October 1970): 31–37.

[4] Smith and Karlesky, *op. cit.*

[5] Brooks, H., *The Government of Science*, (Cambridge, Mass.: MIT Press, 1968), pp. 31–32.

[6] Cairns, "The Cancer Problem," *Scientific American*, 233, (November, 1975): 64–78; cf. also Maguire, A., "Toxic Substances: A View From the Hill," p. 43, in Technical Information Project, *Toxic Substances and Trade Secrecy*.

[7] Nelkin, D., *Technological Decisions in Democracy: European Experiments in Public Participation* (Beverly Hills, Calif.: Sage Publications, 1977).

[8] National Science Board, *Science Indicators 1974*, National Science Foundation, (Washington, D.C.: U.S. Government Printing Office, 1975).

[9] Mullaney, T. E., "Optimism in the Midwest," in column, "The Economic Scene," *New York Times* (Sunday, December 11, 1977).

[10] Shapley, W. H., Phillips, D. I., and Roback, H., *Research and Development in the Federal Budget FY1978*, (Washington, D.C.: American Association for the Advancement of Science, 1977), p. 139.

[11] Hollomon, J. H. and Harger, A. E., "America's Technological Dilemma," *Technology Review* (July-August, 1971).

[12] Mansfield, E., "Contribution of R&D to Economic Growth in the United States," *Science*, 178 (February 4, 1972); Mansfield, E., "The Effects of R&D Expenditures on the Economy," Testimony before Committee on Science and Technology, U. S. House of Representatives (April 28, 1976).

[13] Cole, J. R. and Cole, S., "The Ortega Hypothesis," *Science*, 178 (October 27, 1972): 368–374.

[14] de Solla Price, D. J., "The Science of Scientists," *Medical Opinion Review*, 10 (1966): 88–97.

[15] Freeman, R. B., *The Market for College Trained Manpower: A Study in Economics of Career Choice* (Cambridge, Mass.: Harvard University Press, 1971), chap. 7; Freeman, R. B. and Brenneman, D. W., "Forecasting the Ph.D. Labor Market: Pitfalls for Policy," National Board on Graduate Education, National Academy of Sciences (April, 1974).

[16] Shapley et al., *op. cit.*, pp. 17–18.

[17] Fried, C., "The University as Church and Party," *Bulletin of the American Academy of Arts and Sciences*, 31 (December, 1977): 38; Barber, B., *Science and the Social Order* (New York: Collier Books, 1962), p. 139.

[18] *Ibid*, p. 41.

[19] *Ibid*, p. 38; cf. also Brooks, H., "Applied Research: Definitions, Concepts, Themes," in *Applied*

Science and Technological Progress, a report to the Committee on Science and Astronautics, U. S. House of Representatives by the National Academy of Sciences (June, 1967), especially pp. 22–26.

[20] Shils, E, "A Great Citizen of the Republic of Science," *Minerva*, XIV, (Spring 1976): 1–5; cf. "Can Science be Planned?" chap. 3 in Brooks, H., *The Government of Science*, (Cambridge, Mass.: MIT Press, 1968).

[21] Cole, S., Rubin, L., and Cole, J. R., "Peer Review and the Support of Science," *Scientific American*, 237 (October, 1977): 34–41.

[22] Simon, H. et al., "Social and Behavioral Science Programs in the National Science Foundation," National Academy of Sciences, Washington, D.C., (1976).

[23] Carter, G. M., "Peer Review, Citation, and Biomedical Research Policy: NIH Grants to Medical School Faculty," RAND Corporation R-1583-HEW, (December, 1974).

[24] Bush, V. et al., *op. cit.*, pp. 31–33.

[25] Shapley et al., *op. cit.*, pp. 26–27.

[26] Nelkin, D., *op. cit.*

[27] Neugartin, B. L. and Havighurst, R. J. (eds.) "Extending the Human Life Span: Social Policy and Social Ethics," NSF/RA 770123, National Science Foundation, Washington, D.C. (1977), pp. 23 and 26.

[28] Shapley et al., *op. cit.*, pp. 23 and 26.

[29] Bromley, D. et al., *Physics in Perspective*, vol. I, (Washington, D.C.: National Academy of Sciences, 1972) p. 848.

[30] Brooks, H., "The Military Innovation System and the Qualitative Arms Race," *Daedalus* (Summer, 1975): 75–98.

[31] Knowles, J. H., "The Responsibility of the Individual," *Daedalus* (Winter 1977): 57–80.

[32] Levin, D. L., Devesa, S. S., Godwin, J. D., and Silverman, D. T., "Cancer Rates and Risks," U.S. Department of Health, Education, and Welfare, NIH 75-691, (1974).

[33] McCann, J., Choi, E., Yamasaki, E., and Ames, B., "Detection of Carcinogens in the Salmonella/Microsome Test: Assay of 300 Chemicals," *Proceedings of the National Academy of Sciences*, 72 (1975): 5,135.

PART VI

THE DISPUTE SURROUNDING THE ALLOCATION OF FEDERAL FUNDS FOR SCIENTIFIC RESEARCH AND ASSOCIATED SUPPORT FACILITIES

BACKGROUND AND SUMMARY

As the selections in Part V attest, concern about the way federal R&D funds and facilities have traditionally been allocated has been a persistent theme in the literature on decisioni-making in American science. Recognition of the fact that, in the words of Donald K. Price, "the project grant system, and the peer review method by which it distributed funds, has produced a concentration of grants in a limited number of universities" has been instrumental in making that concern an increasingly salient feature of the on-going debate over U.S. science policy. Until very recently, however, most scientists seem to have assumed that the benefits of this allocational pattern so obviously outweighed its liabilities that no overt challenge or long-term threat to the peer review system was likely to be forthcoming. In the last two years, the confidence on which that sanguine assumption was founded has been badly shaken indeed.

Beginning in the spring and summer of 1983, during the first session of the 98th Congress, some twenty-one universities, circumventing the peer review process altogether, have sought or obtained research facilities by direct intervention in the Congressional appropriations process. This unprecedented, so-called "pork barrel" approach has focused attention as perhaps never before on the question of the respective roles of members of the scientific community and Members of Congress in making decisions about the allocation of federal R&D resources. Harvey Brooks' warning that the traditional science-government relationship had become engulfed in a fundamental controversy over "what is basic and thus pertains to the political process and what is tactical and thus pertains to the autonomous governance of the technical community" has proven to have a bearing and a scope greater than many sientists would have imagined possible.

The particulars of the lobbying efforts made by various universities to obtain research facilities outside the normal peer review channels, and of the funding legislation to which these efforts gave rise during the 98th Congress, will not be recounted here. An overview of the events dating from 1983 and early 1984 is presented in the earlier portion of Philip M. Smith and Albert H. Teich's "University Research Facilities and Pork Barrel Politics" which has been reproduced in a later section of Part VI, and some of the more recent developments are discussed in the three brief science- and news-journal articles that follow that selection. Those who wish to familiarize themselves with the immediate background of the events that have provoked the current controversy are encouraged to proceed directly to the accounts provided in those items. Suffice it for now to say that the advent of "academic pork barrel", which Smith and Teich fear "may mark the emergence of a new, more contentious phase in the politics of science" (p. 3), was greeted with universal alarm by the major scientific and research-university organizations: the American Physical Society, the Association of American Universities, the American Association for the Advancement of Science, and the National Academy of Sciences (among others) have all issued statements or resolutions calling for firm adherence to the peer review system and warning universities and Members of Congress that continued "end runs" of conventional departmental and congressional review processes would have serious negative consequences for the integrity and advancement of science. At the close of the 98th Congress, however, there was little to suggest that the "entrepeneurial" universities were likely to take these admonitions seriously. The division of opinion within the academic research community was reflected within the Congress as well, and as the Reader went to press the ultimate disposition of the dispute seemed very much an open question.

A. GENERAL BACKGROUND

As a matter of historical record, it is important to realize that the "geopolitics" of decision-making surrounding the allocation of funds for university research facilities in the 1980s differs in some material respects from that which characterized earlier periods. Although the government invested heavily in academic research programs during the early and mid-1960s, federal programs to finance facility construction had largely dried up by the early 1970s. With a chronic shortage of funds earmarked for the purpose, the competition between universities concerned to maintain and/or upgrade their research programs through the addition of new laboratories and research centers was destined to intensify. The fact that several such institutions, primarily but not exclusively from the ranks of the universities that have traditionally received less federal R&D support, have become increasingly aggressive in soliciting the necessary funds from Congress via "special interest amendments"—and that Members have been responsive to their concerns even to the point of diverting funds authorized for other programs—might therefore be said to reflect a problem which is peculiar to the 1980s. As Robert C. Cowen [1] has suggested, "The neglect of facilities that has marked federal support for more than a decade is bringing university research to its knees."

By the same token, however, a review of the historical record might caution one from assuming that the factors that have driven certain research universities and Members of Congress to by-pass the peer review system are of recent origin only. Allegations of an inequitable geographic and institutional distribution of federal R&D resources were very much in evidence even during the relatively halycon days of the 1960s, and the intense competition provoked by the lack of funds for university research facilities may simply have served as a catalyst or trigger to bring long-standing grievances or pressures to the surface. In any event, Congress has grappled before with the question of whether R&D dollars should be concentrated in a relatively few "centers of excellence" or be distributed more broadly in an effort to "share the wealth." It is therefore relevant and important, in seeking to understand the background and larger implications of the current dispute, to trace the history of congressional interest in the geographic and institutional allocation of R&D.

---

[1] See his "Scraping the Bottom of the Pork Barrel."

The first two selections in Part VI, a chapter from Thomas P. Murphy's *Science, Geopolitics, and Federal Spending* written by Richard D. Heimovics, and a portion of a chapter from Anton G. Jachim's *Science Policy Making in the United States and the Batavia Accelerator*[2], cover roughly the same terrain. These accounts, both dating from the early 1970s, focus on the activities of successive congressional committees and subcommittees which took an active interest in R&D allocational issues in the early and mid-1960s. While either, standing on its own, would suffice to give readers a sense of the principal developments during this period, both are included here because each has virtues that the other lacks.[3] In the summary that follows, the two accounts will be interwoven.

Heimovics and Jachim take a similar starting point in their analyses. They observe that although the federal government's role in the support of scientific endeavors assumed new prominence in the immediate post-War period, it was only in the early 1960s, in the midst of a dramatic upsurge in federal expenditures, that Congress undertook in a systematic fashion to evaluate what was happening with R&D outlays. The establishment by the House of a Select Committee on Government Research chaired by Representative Carl Elliot (D–Ala.) on September 11, 1963 represented the first in a series of initiatives to bring federal R&D policy under more comprehensive congressional supervision. The Committee was directed under House Resolution 504 "to make a complete, full, and thorough investigation of the numerous research programs being conducted by sundry departments and agencies of the Federal Government." Heimovics reports that "It was the Committee's intention to find, reinforce and develop mechanisms for effective control and coordination of R&D" (p. 24).

In Heimovics' view, the Elliot Committee "served an important integrating process which brought together a membership not wholly unfamiliar with R&D problems" (*ibid.*). Although unable to carry out the comprehensive investigation it was charged to undertake, the Committee held extensive hearings culminating in the issuance of ten studies. One of these studies, entitled "Impact of Federal Research and Development Programs," "described in detail how federal R&D funds were distributed and included comments on the part educational institutions play in research and development. The thrust of this study documented the need for a broader distribution of R&D dollars on a geographic basis, while at the same time maintaining a high-level technical competence on federal research projects" (pp. 24–5). The Select Committee died with the defeat of Representative Elliot in the 1964 congressional election, but it served an important function in calling attention to the problems of geographic and institutional concentration in R&D distribution and more generally to "the size and complexity of the government's role in scientific endeavors" (p. 24).

---

[2] Chapter three ("The Organization of Science and Government: Some Consequences") of Jachim's book has been reproduced in its entirety in Part V of this reader. Much of chapter five—that portion focusing exclusively on allocational issues that arose in the field of high-energy physics during the mid-1960s—has been excluded from Part VI.

[3] Heimovic's contribution to the Murphy volume, entitled "Geographic Distribution of Federal Research and Development Funds," discusses (under separate headings) the legislative background and principal accomplishments of each of the relevant congressional committees and subcommittees and also provides tables disclosing the geographic breakdown of federal R & D distribution by agency and by state and region between 1963 and 1968. (Several of the tables for FY 1964 have not been included here.) The Jachim selection, while less "factually" oriented, offers a series of illuminating selections from hearings in which congressmen directly challenged the "decisional criteria" employed by various research-sponsoring agencies; also, and perhaps more importantly, Jachim traces the origins of special R & D "development" programs initiated during the 1960s to build up weak research-universities and regions and discusses some of the problems encountered in this effort to offset the "concentration" problem associated with the normal allocational procedures followed by federal agencies.

The Elliot Select Committee was not the only congressional body which undertook during the course of the 88th Congresss to examine the allocational question nor, in Jachim's view, was it the most effective in bringing to light the diverse perspectives that might be brought to bear in this context. During the course of the Select Committee's hearings, Jachim claims, "the orientations of scientific leaders tended to prevail.[4] The supposition that "competence" was a far more important consideration than "regionalism" in the decision-making policies of granting and contracting agencies was exposed to more exacting scrutiny during contemporaneous hearings conducted by a Senate subcommitttee chaired by Hubert Humphrey (D-Minn.) (See pps. 83–6). In a series of exchanges with officials at the Department of Defense and Commerce, as well as with Jerome Weisner, then-presidential science advisor and director of the Office of Science and Technology, Humphrey himself pressed the issue, forthrightly inquiring of the witnesses whether any mechanisms had been or were likely to be adopted to introduce considerations other than that of "excellence" into the agencies' R&D decision-making procedures. Although witnesses did not seem to share his view that the prevailing pattern of R&D allocation was "deplorable", one important by-product of Humphrey's questioning was that several witnesses acknowledged that there might be a need for new development programs "to build up weak research-universities and regions.[5]

Hubert Humphrey was, in Jachim's view, "the harbinger of an intensifying conflict." "Premises that are clear throughout his questioning had in earlier interactions been implicit—perhaps even ignored. They now became explicit. In the Senate he had said in 1962, 'With all due respect to that great institution, the Massachusetts Institute of Technology, they are just not that good. And the Institute of Technology at the University of Minnesota is just not that relatively unable to do the job.' " (p. 86).

As Jachim's account indicates, congressional discontent with unequal distribution of federal R&D dollars was not restricted to members of the Elliot Committee and was therefore unlikely to wane with the demise of the latter. The lead in this field appears subsequently to have passed on to a new House subcommittee on

---

[4] Page 82 (not included in selection).

[5] Jachim reports that this idea was already being championed in a slightly different context by J. Herbert Hollomon, assistant secretary of Commerce for Science and Technology, who was interested in promoting the ability of small industrial firms to generate or use R&D. In the course of the Humphrey hearings, Hollomon "discussed something he had been proposing, a new program, that would not touch the current system of R&D contract letting, but which would develop the ability of small firms . . . based on the model of the early Agricultural Extension Service" (p. 85).

Science, Research and Development, which in fact had been appointed by George P. Miller (D-Calif.), chairman of the House Committee on Science and Astronautics, on August 22, 1963, a few weeks before the Elliot Select Committee came into being.

The Daddario Subcommittee, so-called after its chairman, Emilio Q. Daddario (D-Conn.), undertook what Heimovics describes as the "first comprehensive congressional look at the government's expanding involvement in R&D" on the basis of a report submitted to it in 1964 by the National Science Foundation. The findings of the NSF report established a firm factual basis to support allegations of a significant geographic concentration of R&D resources: it "documented that approximately 70% of federal research and development funds for all performers were allocated to nine states and the District of Columbia in both fiscal years 1963 and 1964" and also showed that ten states—with California (31%), Massachusetts (12%), and New York (9%) leading the way—had about 80% of the total federal obligations to educational institutions for R&D. (See Tables and discussion pp. 25–8.)

During extensive hearings conducted by the Daddario subcommittee, as reviewed by Jachim (pp. 86–91), spokesmen for such agencies as DOD, NASA, NSF, and HEW-National Institutes of Health readily acknowledged their contributions to this broad distributional pattern. They insisted, however, that the geographic and institutional concentration of the R&D resources at their disposal, as allocated through the peer review process, was an unintentional and, under the circumstances, unavoidable by-product of the agencies' commitment to "quality" or "technical competence" as the primary criterion in deciding who should receive grants. The concentration phenomenon, they suggested, was not so much the result of agency distributional policies *per se* as the reflection of a pre-existing state of affairs that those policies tended to reinforce. As Leland Haworth of the NSF put it, "to achieve high quality results requires going where the best capability exists. That capability is now quite concentrated geographically. Hence the maintenance of high quality results will result in concentration unless and until we build up a broad geographic base of capability" (p. 89). In a similar vein, a spokesman for the DOD, conceding that concentration of defense R&D was likely to continue "for the foreseeable future", explained that "we tend to follow competence where we find it and therefore consider that greater uniformity in the geographical distribution of our R&D contracts and grants can only be achieved to the extent that more uniform competence is developed" (p. 88).

In general, the witnesses called before the Daddario subcommittee emphasized a familiar set of propositions, namely that "competence to do quality research is paramount, that the peer-evaluational system is best, that researchers do not follow the dollar, but follow stimulating research leaders and tend to cluster around them, and that R&D dollars tend to go to these groups of people, not to particular universities or laboratories" (p. 91). The suggestion that research-sponsoring agencies were not more directly responsible for the concentration problem was not, of course, immune to criticism, but Jachim reports that the "few challenges" made during the Daddario hearings along these lines "seemed to be anticipated by the witnesses." On the critical question of how to attain a "broader base of geographic capability" or "more uniform competence," the latter made "suggestions of sorts but there * * * was remarkable agreement on the nature of this change" (p. 87). A proposition forwarded by AAAS representative Paul Gross—to the effect that "the Government's total objective in support of science would be better served if immediate research competence were not the only criterion for the distribution of funds and if some grants for research were made on a formula basis or by selection of especially promising institutions in parts of the country in which they do not now exist"—reflected what Jachim characterizes as the emerging "official consensus" of the scientific leadership: "Redistribution yes, but with separate development programs administered by each agency. By all means, the regular program and the development program must be kept separate and the funding for the new programs should not affect, should be an addition to, the regular programs" (*ibid*).

These proposals "to leave the regular allocational system alone and provide more money for additional programs" (p. 91) did not to strike all observers as an especially promising way of resolving the tension between the criterion of "competence" and geographical considerations in research allocations. They also seemed to present another important problem: "How new programs and new budgets could be added to existing expenditures, and the expenditures level for regular R&D programs be increased as research become more expensive, troubled the congressional inquirers, since one of their major concerns was to gain some control over the growth curve (in expenditures) in research" (p. 88). Hence, while the Daddario committee reports "inferred that steps should be taken to assure, particularly in the long run, a more uniform geographical distribution of R&D funds" (Heimovics, p. 28), how best to achieve this objective remained something of an unresolved problem.

In the midst of the Daddario subcommittee's investigations, Congress took some direct action in an effort to assure a more equitable allocation of R&D resources on the part of one important research-sponsoring federal agency. The NASA Authorization Act of 1966 (passed on June 28, 1965), contained an "unusual amendment", sponsored in the House by Representative J. Edward Rousch (D-Ind.) and in the Senate by Walter F. Mondale (D-Minn.) which sought to effect a direct change of emphasis in the decision-making equation NASA had traditionally employed in distributing its R&D funds. The relevant provision stipulated that: "It is the sense of Congress that it is in the national interest that consideration be given to geographic distribution of Federal research funds whenever possible, and that the National Aeronautics and Space Administration should explore ways and means of distributing its research and development funds whenever feasible."

As the account offered by both Heimovics (pp. 45–6) and Jachim (p. 91) reveal, the amendment was prompted by congressional concern that approximately 80% of the NASA R&D budget was being spent in coastal states. So strong were the sentiments of some midwestern Members of the House on this score that nine of them voted against the NASA authorization bill (which passed the House by a roll-call vote of 389-11) even with the Rousch amendment attached; Melvin R. Laird (R-Wisc.), then-chairman of the House Republican Conference, explained that "This is the only method available to those of us in the central states * * * [to express] our deep concern and sincere disappointment over the failure of a national space agency to follow through on their earlier committments to recognize our section of the country." During Senate floor debate, William Proxmire (D-Wisc.) lamented a Senate committee's action which had made the "innocuous but laudatory" Rousch provision "more innocuous and less laudatory" (*ibid.*) and urged adoption of the companion Mondale amendment to underscore the importance of geographic distribution. His colleague from Wisconsin, Senate Democrat Gaylord Nelson, in reaction to objections voiced by NASA Administrator James Webb to a proposed requirement that the agency award 50% of Voyager Phase I R&D contracts to states then receiving few NASA funds, suggested there were "some fallacies" in the claim that the policy should be one of putting the money "where the competence is" (Jachim, p. 91). Despite the hopes of its supporters, however, adoption of the amendment to the 1966 NASA authorization act apparently did not effect any marked change in that policy bias. Jachim reports that Walter Mondale subsequently described it as "one of the least used paragraphs in the U.S. statute books" (*ibid.*).

Its deliberations on the Rousch-Mondale amendment was not the only context in which the Senate's concerns regarding R&D allocations became apparent during the 89th Congress. The ongoing oversight efforts of the Daddario subcommittee (which had commenced hearings late in 1963) soon came to be complemented on the Senate side by the Subcommittee on Employment and Manpower of the Senate Committee on Labor and Public Welfare, which became active in reviewing various aspects and implications of federal R&D expenditures. In 1965, Subcommittee Chairman Joseph Clark (D-Pa.) turned the chair over to Gaylord Nelson to conduct hearings on "The Impact of Federal Research & Development Policies on Scientific and Technological Manpower", thereby affording Nelson, whose grievances, we have seen, were evident during debate on the NASA authorization act, an opportunity to play a more prominent role in investigating the distributional implications of federal R&D policy.

The tone of the subcommittee hearings was set early when Nelson, presiding pro tempore, suggested that an important part of their purpose was to determine to what extent existing R&D policy "promotes the development of various regions, or hinders the development of others", and "to what extent new policies might be in order" (Heimovics, p. 42). "Whenever the geographic distribution of funds is questioned or even examined," Nelson went on to suggest, "someone is certain to cry 'pork barrel politics.' It will be claimed that we are only trying to see that one state gets its share of the federal money, even if this is not the wisest or most economical way to do a job." While disavowing the notion that the Subcommittee had any intention to adopt "this narrow kind of approach to the problem," Nelson added pointedly that "it often seems that those who cry 'pork barrel politics' are the 'haves' who oppose the 'have nots'." And he warned, in prescient terms, against "shortsighted" dismissals of allegations of inequity in R&D distributions: "it is obvious that more and more states are worried about this issue, [and] that unless we devise national policies, we shall [all] end up the victim of pork barrel politics." (*ibid.*).

The witnesses called before the Nelson subcommittee "included the usual list of scientific leaders" and agency officials. James Webb returned to face Nelson once again, and Heimovic reports that a "highlight" of the hearings was the former's response to questions raised by the latter "about the distribution of federally funded R&D activities of NASA" (p. 44)—but the fact that the "list also included spokesmen from 'have not' areas and organizations" gave the hearings what Jachim terms "a somewhat different flavor" (p. 91).[6] In Jachim's view, the Nelson subcomittee hearings disclosed that "the questioners were beginning to understand . . . some aspects of the allocational system." (p. 92). On the one hand, the attempts of NSF's Leland Haworth and others to reiterate that problems of geographical and institutional concentration would best be resolved by further movement in the direction of the "two programs" approach ("the regular one based on project grants to individuals and groups, and development programs of various kinds") appear not to have carried as much weight as they had in the past: "It was . . . the allocational system of the regular programs that was being challenged." On the other, representatives of universities that have traditionally received less federal R&D support, such as University of Wisconsin President Fred H. Harrington, who observed that the prevailing allocational system "results in a tendency to lose faith in the democratic process", and Robert C. Edwards, President of Clemson University, who forthrightly suggested that Congress consider the possibility of distributing at least some R&D funds on a population basis, helped to clarify the salient issues: "The core of the implicit conflict of values begins to stand out, that is, meritocracy vs. equality—the wider distribution of educational and economic opportunity in this case" (*ibid.*).

The failure of scientific leaders and agency officials to heed seriously the objections of the "have nots" to the prevailing system clearly was a source of consternation to Senator Nelson. Near the end of the hearings, the Senator repeated the sentiments expressed in his opening remarks, castigating "the rigidity of attitude and apparent lack of concern" about the concentration problem on the part of those advocating continued emphasis on "competence" as the principal criterion in allocative decision-making (Jachim, p. 92). Nelson's critical posture was reflected in the final subcommittee report, which was issued, along with recommendations, in December of 1965. The report expressed the committee's "disappointment" that agencies had presented "a paucity of material" when asked to appraise whether a continuation of current policy would produce a "greater and greater imbalance in the Nation" (*ibid.*) As Heimovics notes (p. 45), Senators Javits of New York and Murphy of California refused to go along with its findings or recommendations,[7] but the activities of the Nelson subcommittee surely heightened congressional concern about the long-term effects and implications of agency R&D programs and distributional policies.

---

[6] Jachim's reference here (unaccompanied by any more detailed description) is to "the Clark hearings of 1965." In the account he proceeds to offer, however, Senator Nelson plays a very prominent role, and it may be assumed that much of the material cited is drawn from the hearings conducted by his subcommittee.

Passage of the Rousch-Mondale amendment to the 1966 NASA authorization act, along with the ongoing investigations being conducted in the House by the Daddario subcommittee and by the Nelson subcommittee in the Senate, served to alert executive branch officials to the extent and depth of congressional interest in R&D allocational matters. On these heels of those initiatives, Jachim reports, science adviser Jerome Weisner sought to impress upon President Lyndon Johnson "that there was trouble ahead for the scientific community . . . if proponents of per capita or geographic distribution R&D resources became too strong" (p. 93). On September 13, 1965, after consultations with Weisner and others, Johnson issued a memorandum to the heads of departments and agencies entitled "Strengthening Academic Capability for Science Throughout the Country." The memorandum called for new efforts of the part of agencies supporting R&D in universities, stipulating in part: "Research supported to further agency missions should be administered not only with a view to producing specific results, but also with a view to strengthening academic institutions and increasing the number of institutions capable of performing research of high quality."

Jachim speculates (p. 93) that President Johnson was probably prompted to take this action as a defensive measure, i.e. to fend off increasing pressures for reform of R&D policy emanating from the Congress, but Heimovics reports that in commenting on the memorandum with members of his Cabinet on the day following its release, the President emphasized his personal view that "Research funds are still concentrated in too few institutions in too few places of the country" (p. 29). In any event, although the presidential directive cleary was intended to effect some kind of redistribution, Jachims points out that "there was no clear indication of how it was to be done except as 'determined by each agency or department.' Agencies, it was suggested, might take redistribution into account, all other things being equal, where research capability of comparable quality exists. . . . Some kind of block grant approach was suggested in the call for more university administered programs. Thus the plea for the differentiated programs emerged anew, with implementation of the new university program to be decentralized in part to university administrators" (p. 94).

A number of federal agencies, heeding the signals and recommendations that had pre-dated the Johnson memorandum, had indeed established special "development" programs during the mid-1960s. The problems inherent in a system whereby agencies were entrusted to "provid[e] their own remedies" (Jachim, p. 94) to the concentration problem, however, became apparent to congressional observers before 1965 had elapsed. In hearings held in the late fall of that year before the Reuss subcommittee on Research & Technological Programs of the House Committee on Government Operations, NSF associate director Henry Riechen was asked to describe the orientation and purpose of the Foundation's new Science Development Program. His response: "We think of a center of excellence as an institution that is as much as possible uniformly excellent. A major part of our effort in the science development program is to raise the level of excellence everywhere in the institution." Reichen's statement revealed that "The criterion of competence (quality) . . . had been incorporated into the NSF's development program" (Jachim, p. 95). "The subcommittee members clearly did not see this as the development program they had in mind", and further inquiry reinforced their suspicion that the program appeared to be contributing to the very problem it ostensibly had been designed to alleviate. The subcommittee's report observed that the four initial recipients of NSF development grants—Washington University-St. Louis, Western Reserve, the Case Institute of Technology, and the University of Oregon, could not be deemed "research deprived; three of the four, in fact, rank with the top 40 institutions in terms of Federal money received." The committee resolved that "emphasis should be placed on improving development institutions rather than lagging departments within already important research institutions." (*ibid.*).

---

[7] That these Senators "were from states receiving the largest shares of federal R&D dollars", Heimovic suggests, "may have had something to do with their dissenting views" (*ibid.*).

In 1966, another new oversight committee became active in the investigation of R&D allocational issues. The creation of a Subcommittee on Government Research of the Senate Government Operations Committee, Heimovics suggests, was "Partly in response to President Johnson's memorandum, partly * * * a midwest protest, and partly to provide a platform for [chairman-designate] Senator Fred R. Harris who faced a difficult re-election in Oklahoma" (p. 29). The primary sponsor of the Senate Resolution that created the Subcommittee, Carl T. Curtiss of Nebraska, pointed to the worrisome "imbalance" in prevailing R&D expenditure patterns and suggested that "Congress has an obligation to do equity and justice in determining where this money is being spent, loaned or given" (*Ibid.*). To help the new subcommittee "arrive at policymaking criteria for the equitable distribution of research and development grants" (Heimovics, p. 30), Senate Resolution 231 directed the NSF (in Jachim's words) "to formulate and transmit recommendations as soon as possible on what changes should be made in [current] law and administration" (pp. 96–7).

Investigations conducted by the Harris Subcommittee underscored once again the extent of the concentration problem. Commenting on tables showing the distribution pattern for 1964 by both state and functional areas that he had entered into the Congressional Record, Senator Karl E. Mundt (R–SD) said he found it "distresssing to note that more than half of all research and development expenditures from Federal sources were spent in four States" and that one of these, California, received "more than the total of the lowest 43 states" (Heimovics, p. 30). The problems of "have not" areas and organizations in trying to attract R&D resources became abundantly apparent in the course of the subcommittee's extensive hearings, as reviewed by Jachim (p. 96–9). The bias of the peer-review system in allocating funds for university research was dramatically illustrated in an example provided by Robert Kaman, President of Oklahoma State University: "One member of our faculty has been unable to obtain federal support for 10 successive proposals since his coming to our campus, but he previously obtained support for all three proposals he made while he was a member of the staff of a well-known eastern university." Senator James B. Pearson of Kansas drew a familiar lesson from this and other examples: "[I]t is literally the case that the awarding of large research grants can make or break a college or university and can also have a decisive effect on the character and vitality of the surrounding local community" (Jachim, p. 97).

Increasing congressional dissatisfaction with the point of view expressed by the scientific and agency leadership on R&D allocational issues became evident in exchanges with NSF Director Haworth and OST Director Donald F. Hornig, who together "bore the brunt" of the Harris subcommittee's attack. Haworth's continued advocacy of the two-programs approach to the concentration problem elicited a very skeptical response from Senator Harris, who subsequently suggested to Hornig that he and Haworth "have been a little patronizing in your treatment of this committee * * * in saying that these things are not as important as I think, but that you are doing a whole lot about it anyway * * *." (Jachim, p. 98). The chairman also observed that he did not place much store in the notion that the data-gathering activities being undertaken by OST and NSF would shed light on the problem or contribute to its solution: "If you are part of the established order, and your general feeling is that things are not so bad as [your] critics think they are, and that things are really in pretty good shape—concerning distribution—then I think you will tend to do nothing at all. If on the other hand, you are outside of the established order * * * it seems to me that you would tend to look for ways in which things might be done to improve the situation." (Jachim, p. 98).

At the conclusion of its extensive hearings, the Harris subcommittee reached a number of general conclusions (recounted by Heimovics, p. 41). In essence, it recommended "that for two reasons—economic impact and impact upon the quality of education—the distribution of federal research and development funds as a matter of national policy should be made on an equitable basis throughout the country and among institutions of higher learning" (*ibid.*) Implicit in its concession that there was "no easy way" to accomplish this objective, however, was the subcommittee's acknowledgement that it had not in fact been able to formulate an alternative set of policy-making criteria to those traditionally employed by research-sponsoring federal agencies.

The relevant portions of the Heimovics and Jachim selections end at the conclusion of the Senate Harris subcommittee's activities in the second session of the 89th Congress.[8] Unfortunately, no similar account of congressional involvement in R&D allocational issues in the period since 1966 appears to be available. However, the question of the geographical and institutional distribution of federal R&D resources continued to attract congressional attention during the 1970s. This period is covered in the next selection in Part VI, which incorporates three brief passages from a comprehensive history of the House Committee on Science and Technology entitled *Toward the Endless Frontier,* written by former Representative Ken Hechler (D-WV)[9] and published as a Committee Print in 1980.

---

[8] As noted earlier, the remainder of Jachim's chapter is devoted specifically to developments in the field of high-energy physics. The latter portion of the Heimovics selection is devoted to a review and appraisal of studies published by the NSF concerning the geographic distribution of R&D funds for fiscal years 1965 and 1968 (see pps. 46-57 and Tables 2-6 through 2-10).

Hechler recounts the repeated efforts of former Representative Walter Flowers (D-Ala.) in the period between 1974 and 1977 to effectuate a change in the R&D policies of the National Science Foundation. Flower's basic objection, made "frequently both in committee and on the floor", was spelled out during a 1974 debate over the funding level for the NSF's Research Applied to National Needs (RANN) program: "Certain institutions in certain states continue to receive the lion's share of the National Science Foundation's grants. It seems to me that NSF has a definite obligation to see that these awards are distributed more equitably across our Nation. I am not persuaded that quality research people and institutions deserving of grants and contracts are located only in those few areas now receiving special attention" (p. 512).

Flowers reiterated his argument in the years to come, objecting that an over-concentration of NSF funds in research-rich states like California and Massachusetts was occurring at the expense of many states in the deep South. His exasperation seems to have increased with each passing year. In 1975, he allowed that perhaps he was "batting [his] head against a stone wall", and in 1977 he characterized his on-going efforts to redress the imbalance in NSF"s allocation of its research funds as "an annual exercise in futility" (pp. 516, 536). Some of Representative Flowers' efforts were supported by other Committee members. In 1975, for example, newly-appointed Chairman of the Subcommittee on Science, Research and Technology James W. Symington (D-MO.) "insisted that the subcommittee recommend strong language * * * emphasizing the need for wider geographic distribution, in accordance with the terms of the amended NSF act of 1968"—which, reflecting the congressional concerns so evident through the 1960s, had admonished the NSF "to avoid undue concentration" (p. 516). Two years later, however, the subcommittee rejected Flowers' proposal to strengthen the "admonition" of the 1968 law by making avoidance of undue concentration "the objective" (rather than "one of the objectives") of NSF policy. At the suggestion of Richard L. Ottinger (D-NY), "Flowers weakened his amendment to make it read 'an objective' rather than 'the objective' and it sailed through without opposition" (p. 536).

Hechler notes that Flowers' overtures in 1974 "furnished the subject of much debate and conferences within NSF" (p. 512). Ultimately, however, they appear to have made little impression on the Foundation. "Aside from some token exceptions", Hechler reports, "the net results continued to show that those institutions and geographical areas which were already benefitting continued to do so" (*ibid.*). The committee report accompanying the NSF authorization bill for FY 1976 stressed the need, as Symington had urged, to avoid undue concentration and faulted the Foundation "for failing to take legislative intent into account." But despite this reaffirmation, "there was very little affirmative action demonstrated by the NSF in carrying out the intent of Congress in this respect" (p. 516). The effect of the weakened Flowers amendment adopted two years hence was similarly "nebulous." "About the only concrete result was that, working through Flowers, NSF did manage to pass along advice to Alabama educational institutions on how to sharpen up their proposals and applications for grants. Whether or not by coincidence, there appeared to be some increase in Alabama grants after Flowers had made his many protests. The whole exercise, however, did not result in any substantial changes toward a wider distribution of NSF funds nationwide" (p. 536).

---

[9] In his earlier capacity as Representative from West Virginia, Hechler had taken an active interest in R&D allocational issues. (Heimovics cites remarks Hechler had made during debate on the 1966 NASA authorization on p. 19 of his article.)

The fact that the effort to effectuate a change in the NSF's grant-awarding process appears to have been (by Hechler's account) something of a one-man crusade may be symptomatic of a relative decline in salience of R&D allocational issues in Congress during the 1970s. This in turn may due to an apparent amelioration of the chronic imbalance in the overall geographic distribution of federal R&D funds. In their 1977 study, *The State of Academic Science: The Universities in the Nation's Research Effort,* Bruce L.R. Smith and Joseph J. Karlesky report that those regions with traditionally smaller shares of federal research funds showed "the largest rate of growth" in the decade between 1963 and 1974: the East South Central region experienced a 129 percent increase during this period; the West South Central, 105 percent; the South Atlantic, 102 percent; and the Mountain region, 101 percent (p. 45). These apparently impressive growth rates do not necessarily imply that these regions succeeded in closing the gap altogether—Smith and Karlesky acknowledge that the overall geographical distribution of research funds "remains concentrated" (*ibid.*). Nonetheless, while "there are many ways to argue the issue," the authors contend that "the most fundamental conclusion is the absence of severe geographical imbalance in the distribution of funds by most accepted measures" (p. 46).[10]

As the title of their study suggests, however, Smith and Karlesky are principally concerned with the state of academic science rather than broad issues of geographic distribution. And their overview of the "Institutional Concentration of Federal Funds for Academic Research" (from which the citations in the preceding paragraph are drawn) indicates that substantial evidence could have been produced to support both Representative Flowers' allegation that "too few institutions" continued to receive "the lion's share" of academic research expenditures and Hechler's contention that the changes that were forthcoming in this domain—which is perhaps more critical for our purposes—were on balance only of marginal significance.

---

[10] Readers interested in assessing the merits of Smith and Karlesky's conclusion in this context may wish to consult the NSF statistical tables revealing (along a number of axes) the geographical distribution of federal R&D funds for FY 1982, reproduced in the next selection in Part VI.

Between 1963 and 1975, the top 100 universities consistently accounted for "more than 85 percent of all academic research expenditures"—a reflection of the fact that "despite criticisms of the peer review system and proposals for geographic distribution formulas," the NSF and other government agencies have continued to base their grant decisions fairly strictly on considerations of 'scientific merit' or 'excellence' (pp. 40, 43). A "gradual decline" in the share of federal research funds going to the top 10 universities, which received "a full third" of total academic research expenditures in the early 1960s, can be discerned (by 1975, the figure stood at 25.8 percent), and data provided by the NSF (see Table 3, p. 41) also indicates that the top twenty universities, recipients of half the total in 1963, got about 40 percent a dozen years hence. Smith and Karlesky report that "some substantial shifts" in the positions of individual institutions were apparent during the same period, as a number dramatically increased their share of research funds while others fell back. (A list of some of the universities in the top 100 that experienced significant gains in this respect is provided on Table 4, p. 41). In general, however, the pattern of "concentration at the top" remained substantially unchanged.

The persistence of a "relative institutional concentration of funding" is one among many indications of a pervasive "stratification" apparent within the community of research universities. In the excerpt from the concluding chapter of their book reproduced here, Smith and Karlesky suggest that the stratification phenomenon has of late become more pronounced: "One of the major findings of this study is the negative impact of recent trends on less distinguished science departments and less distinguished scientific investigators. By most measures—research funding, graduate student enrollments, or other indications of the vitality of the research effort—the most serious impact of recent trends has been felt in second-rank departments and institutions." While the leading science departments have been able to maintain and even to strengthen their position, indications are "sufficiently clear" that "a very rapid deterioration in the relative position of many weaker departments could occur in the near future." This prognosis of an increasing gap in the research capabilities of the more and less prominent science departments is all the more likely to be borne out "if the somewhat austere funding for academic research continues" (p. 229).

Whether indications that the tendency toward stratification has accelerated should provoke alarm or be greeted as a welcome phenomenon is not altogether clear, Smith and Karlesky suggest.On the one hand, "it is extremely important that the highest quality science continue to be nurtured and protected in a period of relative austerity if the United States is to continue its scientific leadership", and the "adjustments" that appear to have taken place may in fact contribute towards realization of that objective. On the other hand, "there is a legitimate concern that the erosion may have proceeded too far", posing a threat to the time-honored ideal of "a broad base of scientific competence in many universities" and to the maintenance of healthy "competitive pressures" in the university science system (pp. 229–30).

The fact that there are "a variety of pressures pushing in the direction of increased stratification", Smith and Karlesky suggest, poses an important set of questions that must be confronted by public officials, university administrators, and scientists: "What kinds of research must be preserved as a matter of policy, and in which universities? Is it enough to have a relatively small number of clearly elite centers of scientific excellence?" (p. 230). Although there is "little in our national experience to provide a ready basis for answering such questions" and no "one clear and obvious path that the nation should choose" (pp. 230, 231), neither the prospect of "an extremely narrowly based system" with only "a dozen or so major research universities" on the one hand, nor the continuation of the current "broadly diversified systems", with some two hundred "research-intensive, doctoral granting institution", on the other is, in the authors' view, either likely or desirable. The "most appropriate alternative", they suggest, is "a somewhat slimmed down but still broadly based and competitive system" (p. 232).

Having indicated what kind of university science system they believe best promises to maintain the country's research capability under conditions of fiscal austerity, Smith and Karlesky offer some suggestions regarding the support policies that are most likely to ensure its realization. They are "skeptical about any revival of the NSF Science Development Programs tailored to 'needy' or 'emergent' departments or universities"—but for reasons that are quite different from those of congressional critics of the 1960s. Whereas some members of Congress regarded such development programs, at least potentially, as a corrective to the bias inherent in the peer review system, Smith and Karlesky suggest that the problem with "programs designed to aid special constituencies" is that "they inevitably tend to introduce political considerations into the process of awarding research grants." These political considerations are "at odds with the principle of competition on the basis of scientific merit—a principle that has served the country well. The dominant pattern of federal support for university research is, and should remain, the competitive award of project grants and contracts * * *." (p. 233).

There are, however, ways of helping to maintain a "broadly based" system without compromising the principle of competition based on merit, and the preservation of that principle does not necessarily entail that "project support alone—and no other form of assistance to the university research effort—can be justified." Smith and Karlesky cite some examples—the NIH biomedical research formula grant (which "assists universities, up to a certain ceiling, with flexible funds to meet needs not covered direct project grants") and a similar NSF formula grant that was phased out after the 1960s—to support their contention that forms of institutional support that "that do not conflict with the merit principle" are available which might "help keep financially troubled universities engaged in creative research." Though it "would not persuade universities with little or no research capacity to compete for project funds", the revival and/or strengthening of this formula grant approach, and its possible extension to other research sponsoring agencies, could "significantly assist" those universities already committed to research "to retain a high-quality * * * capacity" (pp. 232-3).

The preference for forms of institutional support "geared to a merit formula" as against development grant programs is founded on Smith and and Karlesky's conviction that "readjustments" in the university science system will best be determined by "intense competition" between and within universities "rather than by a central plan or design." There is, however, one important "uncertainty" that "complicates" this competitive approach—"the weakening financial base of many universities." Indeed, the general financial difficulties being faced by these institutions could result in a "sharp deterioration in the total scientific capabilities of a wide range of universities" that would "go beyond healthy competitive adjustment in the system" (p. 233).

An important part of the problem here, the authors go on to suggest is that "[t]he federal government continues to view universities as it does other suppliers of research services, principally as sites where research of interest to federal agencies is carried on." There is, in other words, a "fundamental difference between universities and the federal government on the issue of institutional support": the latter "does not recognize . . . a national responsibility for the continuing health of the universities themselves." While persuaded that "the current federal view is inadequate and that measures need to be devised to strengthen the institutional base of universities within the continuing framework of the competitive project system", Smith and Karlesky consider a significant change in federal policy unlikely "for the foreseeable future." This in turn may have some serious consequences: while institutional funds might not "basically alter" the tendency towards stratification which "has roots in many developments of the past decade", the absence of such support cannot but contribute to a problem of growing dimensions (pp. 234-5).

B. THE 98TH CONGRESS: THE DISPUTE INTENSIFIES

Continued relative austerity in federal funding of academic research in the past decade has no doubt served to reinforce the tendency toward an increasing "stratification" within the university science system. As Smith and Karlesky seem to have anticipated—and for reasons we can by now well appreciate—this development was destined to lend new urgency to the latent, ongoing dispute both within the scientific community and between the scientific leadership and certain Members of Congress over the appropriate criteria to be used in allocating federal R&D funds and facilities. When viewed in the larger historical perspective, the emergence of so-called "academic pork barrel" in 1983-84 is perhaps not altogether surprising. It may well be that there are sound "scientific" reasons for opposing the introduction of so-called "political considerations" into the processes by which research needs are assessed and resources distributed. But recent events might be said to underline the point that the scientific leadership has resisted demands for reform of the allocational system—in this case, the repeated calls of certain Members of Congress to counterbalance the commitment to "excellence" with a no-less-serious commitment to some kind of "equal distribution" criterion—at its own peril. A similar lesson may be gleaned from the NSF peer review controversy of the mid- and late-1970s, and in their commentary on that debate Ian Mitroff and Darryl Chubin posed what, from the point of view of democratic theory, might be seen as the relevant question in this context as well: "If scientists refuse to be reflexive, skeptical and probing of their own institution—its organization and management—then can they really decry congressional "incursion" into their policies and practices? Part of the responsibility of autonomy and self-governance is self-scrutiny. This is a reasonable expectation, yet too few dissenting voices within the scientific community tend to be heard." [11]

One of the striking features of the developments of 1983–84, however, is that among those who conspired to "subvert" the peer review system in an effort to gain new facilities were the representatives of some of the more prominent and traditionally more successful grant-seeking institutions (not particularly likely candidates for the "dissenters" to whom Mitroff and Chubin had alluded). This may help explain why commentators such as Philip M. Smith and Albert H. Teich, whose "University Research Facilities and Pork Barrel Politics" reflects a keen appreciation of the larger bearing of the ongoing dispute over "excellence" and "equity" in research allocations, have tended to attribute the advert of "academic pork barrel" to a relatively recent and discrete set of problems—steming from a sharp decline in capital investments in university R&D facilities across the board—rather than to more general, long-standing grievances of systemic inequities in the allocational system. As Smith and Teich note, the lack of federal support for new facilities and a variety of related developments in the past decade or so have made it increasingly difficult for many universities (especially but by no means exclusively the less distinguished research universities) to cope with what a 1981 AAAS study termed "a substantial backlog of research [facility] and equipment needs." In their view, it is indeed "quite possible," as others have suggested, that "the pork barrel phenomenon is a measure of the severity of the facilities problem and the despair of academic administrators in finding funds for new facilities." But Smith and Teich's primary purpose is not to speculate on causes: "[w]hether or not one accepts the notion" that the development is symtomatic of these "deeply rooted" facility-related problems, the important point on which the authors want to insist is that "the scientific community needs to take seriously the pork barrel phenomenon" (pp. 12–13).

Whatever its roots or origins, the salient fact, Smith and Teich suggest, is that "the current academic pork barrel is different in some significant ways from past instances of academic competition for research facilities" (p. 4). The "major difference" lies in the fact that "past competition and lobbying have generally been for announced facilities that have been subject to considerable discussion and planning by the scientific community and by public officials charged with the management of research and research policymaking in the Executive Branch and Congress" (p. 9). The tendency among members of the scientific community to regard the departure from past practices as a "temporary abberation" is one that ought not, in Smith and Teich's view, to be indulged: the new style of lobbying "may well not go away by itself" (p. 12), and "[c]arried to extremes, it could constitute a serious threat to the political autonomy which science has historically enjoyed in this country" (p. 4).

---

[11] "Peer Review at the NSF: A Dialectical Policy Analysis" (1979), p. 200. [Reproduced in Part V.]

Under the framework of political autonomy that was put in place immediately following World War II, agency science programs have all followed certain "tenets", including program stability ("so that long-term work could be undertaken") and program guidance by outsiders, which Smith and Teich argue "have stood well against many tests and examinations" despite persistent debate "about excellence and equity and the quality of the peer review process and possible biases" (pp. 7-8). However, as the two separate appraisals of the "new lobbying efforts" they offer are designed to highlight, Smith and Teich are concerned that the advent of "academic pork barrel" threatens to unravel "what could be called the 'traditional process' for setting priorities within disciplines and for allocating funds for research facilities", and therewith the general framework of scientific self-regulation in university research (p. 8).

In the concluding portion of their paper, Smith and Teich undertake a preliminary examination of the pros and cons of various alternative courses of action that the scientific community might pursue in attempting to respond to the recent developments. As the authors see it, a range of options present themselves, each of which has advantages and disadvantages. The possibilities include: a "Return to the Status Quo Ante" (p. 14); a "New Facilities Support Program" to provide universities with funding specifically for facilities (p. 15); the development of "New Funding Mechanisms" not generally employed in academic research, whose chief virtue might lie in their capacity to redress "perceived or real biases in the existing peer review system" (pp. 15-6); "New Financing and Expensing Mechanisms" to assist universities in generating support for research facilities (p. 16); and finally the option of allowing "the academic pork barrel to grow unhindered, or even to embrace it formally in some kind of way" (p. 17).

No effort will be made in this context to enter into the particulars of Smith and Teich's appraisals of each of these possible courses of action. But it is pertinent to call attention here to one significant liability the authors' see in choosing to accept "academic pork barrel" as a more or less permanent way to go about distributing academic research facilities, namely that it threatens to turn academic research funding "into a free-for-all akin, say, to federal water projects." Smith and Teich regard this as "a very serious matter": "To the extent that the public—and especially the Congress—begins to see academic research and the supporting facilities as a matter of distributing resources among essentially private interests rather than supporting a nationwide activity in the public interest, the current broad base of political support [for science] will start to erode" (p. 17).

The above-cited passage is important if only because it makes explicit a concern that is implicit in the very use of the term "pork barrel"—namely that the alleged preoccupation of Members with their own electoral fortunes renders Congress as an institution peculiarly unsuited to make decisions about the location of projects (including research projects) in a manner that will serve or promote "the public interest." This issue has of course been much discussed and debated by political scientists, and it happens that perhaps the best-known work by a political scientists on the political benefits and political costs confronting Members of Congress in deciding whether to support, oppose, or remain neutral on projects in their own or others' districts or states is a case study of federal water projects.

C. CONGRESS AND THE POLITICS OF GEOGRAPHIC ALLOCATION

The selections from John A. Ferejohn's *Pork Barrel Politics: Rivers and Harbors Legislation, 1947–1968* (1974) that leads off the next section of Part VI should help readers appraise the controversy surrounding so-called "academic pork barrel" from another vantage point, and additional selections in which prior congressional involvement in disputes surrounding the location of scientific research facilities is discussed may help to set the current debate in sharper relief.

Early on in the chapter from his book reproduced here, Ferejohn observes that "[t]here appear to be three separate reasons why congressmen value projects in their districts. First, projects are valuable in reelection campaign as part of an incumbent's record in office. Second, they maximize discretion over other legislative issues. Third, they ensure that a project issue will not be held against the congressman by groups who could enter or back an opponent candidate in the next election" (p. 49). Water projects, Ferejohn argues, are particularly valued by congressmen because they show "voters that their congressmen can do things for them in Washington," and they "provide benefits for a few well-organized groups in the district" like construction workers, contractors and local businessmen (p. 49–50).

Once he has established the value of projects to congressmen, Ferejohn explicity addresses the logic of "pork barrel politics" through a typology of the local political forces a congressman faces in making a decision whether to support or oppose a project (Table 3.1, p. 53). The distribution of benefits from different kinds of projects (like navigation, power, flood control and recreation) suggest that some will be more attractive to Congressmen than others and that this will vary depending on the composition of proponents and opponents in each district. "The political benefits and costs of a project to a congressman depend first on who stands to gain and who to lose if the project is built, and second on how these people or groups will reward or punish the congressman for his decision. In many cases his decision is simple, but in others he faces uncertainty. In such situations the rule of thumb available to the congressman is that only those people who stand to gain or lose large amounts if the project is built (or not built) might change their political behavior as a consequence of the congressman's decision" (p. 58). However, one of the factors that complicates this calculation is that congressmen do not know precisely who cares enough about a project issue to potentially switch votes on that basis.

A second issue explored by Ferejohn concerns the question of what motivates congressmen to support or oppose projects outside their district. Ferejohn notes that "the observer of public works decision-making is struck by the fact that congressmen regularly express support for projects that do not have a direct and positive effect on their constituency" (p. 61). He offers two explanations for the behavior. First, members of the same state delegation cooperate with each other in securing projects. This occurs for several reasons. For example, they cooperate in order to maximize the leverage of their state delegation within Congress, to improve their chances for higher elected office by appealing to a broad state-wide constituency, and to produce benefits for their district that occur from the ripple effects of projects in neighboring districts. Second, members of the same committee cooperate with each other to secure projects. In the case of the Public Works Committee, Ferejohn observes that members "join * * * or choose to stay on it because they desire a particular kind of decision on some project from their peers. It is not surprising, then, that committee members support each other's projects" (*Ibid.*). As a result of these state level and committee factors, Ferejohn argues that it is not unusual for a congressman to be on a committee and build up some seniority, expertise, and influence even when he has no obvious constituency interest in a committee's jurisdiction: "A member's increased influence on a committee raises the ability of each congressman in that member's state to get projects from the agencies under that committee's jurisdiction; in turn, it raises the influential member's ability to obtain other kinds of projects outside his committee's jurisdiction for his own district (p. 68)."

Ferejohn's analysis suggests that the political economy of "pork barrel politics" is complex and extends beyond the push and pull of forces within a member's district to state-wide considerations and the structure of Congress itself. Simple theories of congressional motivation and strategy in supporting or opposing projects are therefore unlikely to provide very accurate explanations or predictions of member behavior.

Another look at location decision confronting Congress, this time with specific reference to research facility siting, is provided in two case studies drawn from Thomas P. Murphy's *Science, Geopolitics, and Federal Spending* (1971). In the case of NASA's Project Apollo, decisions regarding the location of the Manned Spacecraft Center at Houston and related facilities at Michoud and Slidell (in Louisiana) and at the Mississippi Test Facility, were a combination of economic and logistic factors. Murphy concludes: "There are a few cases of circumstantial evidence suggesting political considerations in the facility decisions. However, there were some powerful political figures in virtually every major metropolitan area where NASA might have put a facility. The series of location decisions led to an integrated system of facilities and in none of the cases involved did NASA select an unlikely or unsuitable site" (p. 218). In other words, the importance of "tying" the new facilities into an established network of facilities limited the location choices available to a few sites and at one of these sites (Houston) the fortuitous combination of locational desirability and political influence came together very neatly.

NASA's decision to locate the Manned Spacecraft Center in Houston is also the subject of the brief excerpt from Ken Hechler's *Toward the Endless Frontier* (1980). However, Hechler suggests that substantial credit for the choice of Houston over 20 other cities submitted proposals should go to the entrepreneurial work of Albert Thomas during the late 1950's and early 1960's, Whether Houston would have been chosen without Thomas' extensive efforts must remain an open question in light of some obvious advantages of the location.

A different set political dynamics—and outcomes—is traced in the second case study from the Murphy volume, which concerns the Atomic Energy Commission's decision to locate the world's largest proton accelerator in Weston, Illinois.[12] The AEC took a different approach than had NASA had used in selecting a site. According to Murphy: "to forestall the political fury that world surely be evoked by a choice of California or even if the Midwest were bypassed again for a non-California location, AEC planned to select a site before the request for funding was submitted and to do so on the basis of an objective nationwide study" (p. 302). Through a contract with the National Academy of Sciences, the AEC hoped to find an optimal location for the accelerator using carefully specified criteia covering the size of the site, the availability of water, its proximity to other accelerators, and to commercial, industrial, educational, cultural, housing and transportation facilities. Weighing these factors and prioritizing them was, of course, a serious problem. Nevertheless, the academy succeeded in narrowing the choices down to a list of six finalists, including Weston. In choosing these six sites the Academy essentially concluded, according to Murphy, that "while none of these sites was perfect in all respects, all could be adapted to be adequate to the needs of the facility. In a sense, [the Academy] provided the Atomic Energy Commission with a well-balanced opportunity to select whatever region it wished since there were three midwestern entrants, one from the East coast, one from the West coast, and Denver which should be considered as representing the Rocky Mountain region" (p. 309). Although there was substantial criticism in the Congress and elsewhere over this set of potential sites alleging that the Academy Committee knew that Weston would be chosen if it were on the final list, the AEC was able successfully to defend the selection of Weston as the site that "offers the best prospects for attaining the objective of successful mobilization and maintenance of the best possible staff to construct and utilize this unique national facility most effectively" (p. 313).

Murphy concludes that the decision to locate the accelerator at Weston was again a combination of political, economic, and logistic considerations. He writes: "To say that politics had not been involved in the decision would be naive. However, this was not a political payoff to any one Senator, Congressman, or state. It appears that the Midwest made a strong regional campaign * * * Given a predisposition to put it in the Midwest, the Weston site was so well located that it was difficult to justify not using it" (p. 321). But regional pressures alone do not account for the decision whether or not to build the accelerator at all. Murphy notes that "[p]erhaps the most effective politics in the entire case was that practiced by the high-energy physicists. They succeeded in pushing through a major expenditure at the height of the Vietnam War. In many respects, they held the high cards" (p. 322).

---

[12] A more in-depth account of the decision-making process surrounding the location of the Batavia Accelerator near Weston is provided in the book-length case study by Anton Jachim, portions of which have been reproduced in Parts V and VI.

In summary, the selections in this section describe several of the factors and forces that come into play in the Federal government's decision of whether to fund and where to locate R&D facilities. They underscore again the tension between the roles of the scientific community, agency administrators and Members of Congress in making decisions that have important implications for U.S. science policy. This theme will be explored again in the readings in the Part VII.

A. GENERAL BACKGROUND

Science, Geopolitics, and Federal Spending

Thomas P. Murphy
University of Maryland

Heath Lexington Books
D. C. Heath and Company
Lexington, Massachusetts
Toronto London

2

Geographic Distribution of Federal Research and Development Funds

Richard D. Heimovics

On March 3, 1842, Congress approved a grant of $30,000 to Samuel F. B. Morse for "applied research" to cover expenses for his electromagnetic telegraph system. The "Morse grant," if not the very first technical grant made by the federal government, is a congressional landmark for many reasons. The auspiciou event was a precursor of bigger things to come.

Later in the nineteenth century, congressional interest in our agriculturally dominated economy was manifested in the establishment of land-grant colleges and the provision for agricultural experimental stations in association with these schools.[1] From these significant events, federal involvement in research and development has grown sharply. Senator Carl T. Curtis of Nebraska captured the implication of the expanding federal role in R&D in a 1966 statement:

> Many of these programs – and the justification for their funding – have not been based on any particular logic or rationale. They have, like Topsy, 'just growed.'[2]

At the beginning of World War II, the annual federal obligation for R&D wa only $74 million. By 1950 and the first days of the Korean War, it had risen to over $1 billion; by 1955, it had reached $3.3 billion. The crash missile program o the Eisenhower administration pushed the total up to $4.4 billion in 1957 when Sputnik heralded increasing federal involvement in R&D. The amount has increased two hundred and thirtyfold since 1940 with the federal budget for resea and development reaching almost 17 billion dollars in 1970. Accordingly, times have changed and so has the level of interest in federal R&D expenditures.

For Sir Francis Bacon, science was power; today science is politics – at least one would think so by looking at the congressional interest in shares of the 17 billion dollar federal R&D outlay. The location of federal scientific installations and expenditures for research and development has become a geopolitical issue, for not everyone has received the same size bite of the pork pie; in fact, some slices have been rather lean.

The Impact of Research and Development

Three basic questions must be considered in any discussion of federal policy toward R&D:

a. What are the relationships between distribution of federal spending for R&D and the geography of the nation?
b. How do these relationships differ for different types of R&D spending?
c. How have these distributions and relationships changed over time?

The intriguing question of the political implications of how R&D should be organized to achieve a national policy toward science and technology will be left for subsequent examination. The aim here is to examine evolving congressional interest in the geographic distribution of R&D dollars. This chapter may be best considered a "data" chapter on which subsequent interpretation will be based.

First, let us set the stage for our considerations. The degree of interest in the geographic distribution of federal expenditures for research and development has increased with rising expenditures for these purposes in recent years. Accordingly, competition among states has been keen, giving rise to broad economic and political considerations. At the base of all debate regarding the distribution of R&D funds is the question of whether research and development dollars should be concentrated in a relatively few "centers of excellence" or should be more broadly distributed among the 50 states in order to spread the wealth. The implications are not simple.

There is a natural tendency for scientific activity to agglomerate around established centers of scientific accomplishment. And perhaps a good case could be made that this is probably the most efficient way of conducting scientific research from the point of view of science. Yet a policy of deliberately locating scientific research in a backward area of the country to encourage economic development may be a good step toward attacking the many problems of regional growth and prosperity. Research and development involves well-paid employees. These individuals in turn reflect and influence the economic and cultural standards of the community. Alliances between R&D oriented institutions and universities can naturally enhance the status and productivity of both.

Growth based on R&D increases the economic base of a community and generally attracts both new industry and dependent subcontracting industries. States with strong R&D programs and resources tend to grow stronger by attracting additional scientists and hardware productivity closely tied to where the research minds are located.[3] In a political context, R&D contracts are plums. Congressmen whose districts prosper tend to be reelected. Awards made on a basis of political expediency do not preclude scientific competence; however, questions can be raised about whether they could be in the national interest over the long run.

In trying to untangle the complex question of where federal research and development dollars go, it is important to recognize different perceptions of the problem. A Congressman wants to know how his state and district will receive their fair share of the huge R&D expenditures. The economist wants to know how R&D can stimulate growth of the nation's total economy. Regional economic planners are curious about how federal R&D expenditures can stimulate regional growth. The scientist wants the most productive and comfortable place to perform his research. He wants to know how the characteristics of a particular location can stimulate and support the best of scientific inquiry. Increasingly, weather conditions seem to be given much consideration by scientists concerned with location decisions. The educator needs to know in what ways concentration of federal R&D funds can assist his university to develop and increase its educational capabilities. Lastly, the political scientist wants to know not only where the R&D dollars go, but why and how they got there. It would be interesting to highlight these different perceptions.

During the mid-sixties, former Representative J. Edward Roush of Indiana was a most vocal critic of the unequal distribution of the federal government's research and development funds. In comment on the floor of the House in 1965, Congressman Roush stated:

> It seems quite apparent the present imbalance in the geographic distribution of Federal research and development funds is creating an artificial monopoly of such activities in a few certain areas. Such a monopoly can only have a debilitating effect on the capabilities and development of scientific talent in other sections of our Nation.[4]

Representative Roush's concern focused on what he saw as a high concentration of federal R&D expenditures in areas of the country other than Indiana and the Midwest.

During floor discussion of the NASA appropriations for fiscal year 1966, Representative Ken Hechler of West Virginia echoed Representative Roush's concern.

> In the rapid expansion of Federal funds for these purposes during recent years the natural forces at work have resulted in a concentration of such expenditures in only a few sections of our Nation. This natural action is producing unnatural effects upon our economy. This maladjustment of the geographical distribution of such funds contains a tremendous potential for economic ill health in a great many sections of the country.
>
> Ideas are translated into action and production and economic expansion in the immediate areas where they are originated. What we are seeing now is the continued concentration of funds for idea production in a relatively few areas is drawing off the trained scientists and engineers from other sections. We are seeing areas of equal or close to equal competence bypassed to some extent, their growth rate stunted.
>
> The problem has now reached a stage so severe and the potential benefits to the entire Nation are so great that we can no longer ignore the importance of including geographic distribution among the factors to be considered.[5]

Congressional concern in geographic distribution has been aptly described in an editorial titled "The Research and Development Pork Barrel" in the July 2, 1965, issue of *Science*, a publication of the American Association for the Advancement of Science. Dr. Philip H. Abelson, a distinguished scientist and the editor, pointed out:

> The present allocation of funds for research is not in the long-term national interest. One can only be amazed that congressmen from the underprivileged states have been so remiss in safeguarding interests of the nation and their constituents.[6]

Of course, not all the interest in the location of federal R&D is congressional. The educator's concern has been equally vocal. This point is expressed in a 1967 statement by Edwin Young, President of the University of Maine, to the Sub committee on Government Research of the Senate Committee on Government Operations. His comments highlight the interest by his university in the effects of federal spending for research and development on the future of higher education. President Young noted:

> It is indisputable that Federal expenditures for research and development have contributed mightily to the creation of a large number of superior centers of research. The benefits of this to the nation are evident in the wonders of science and technology that never cease to amaze us in our daily lives.
>
> However, it is equally apparent that these centers are distributed unevenly about the country. In a recent year, more than half of all Federal research funds were concentrated in twenty-five colleges and universities. Indeed, more often than not, they are clustered in developed regions of relative affluence.
>
> This fact should not surprise us, for the relation between scientific research and economic development is symbiotic. Research aids economic development, which in turn adds research demands and potential, and so the felicitous wheel turns. The rub comes when a less favored area seeks a place on the wheel. . . .
>
> In short, if our goals are to increase and broaden our national research capacity and at the same time foster economic development in some of our less favored regions, a program of institutional grants to existing universities designed to develop in each State at least one center of reasonable competence in those aspects of science and technology best suited to the needs and capacities of the region is the most efficient way to go about it.[7]

The impact of science and technology on regional economic development can be observed from a much more detached viewpoint by the economist whose concerns are not necessarily tied to specific geographic interests. The problems surrounding the proper distribution of federal R&D dollars were vividly noted by one such economist, William H. Miernyk, Professor of Economics and Director, Regional Research Institute, West Virginia University, in a statement to the Subcommittee on Government Research in July 1967.

> There can be no disagreement that the geographic distribution of Federal research contracts and grants is very uneven. And this leads to the question: Is the present distribution of Federal research funds "fair" or "equitable"? Residents of states which receive less than proportionate shares of the Federal research budget undoubtedly feel that it is not. When it comes to the disbursement of Federal funds, the residents of most states no doubt feel that they are entitled to a "fair" share. But research contracts and grants cannot be awarded solely, or even primarily, on the basis of equity considerations. As taxpayers, most of us feel that Federal funds should be spent efficiently. And given the distribution of research capabilities in the Nation we cannot have it both ways. Unless the geographic distribution of research capabilities can be altered – and I will return to this point below – research administrators in Washington are faced with a hard choice. They must either attempt to achieve greater uniformity in the distribution of Federal research funds, at the expense of efficiency, or they must place contracts where they think the work will be done most effectively in the face of criticism that some states are being treated unfairly. In general, I suspect that most government research administrators are more concerned about the efficient use of research funds than they are about the geographic distribution of these funds.[8]

The effects of research and development are sufficiently diffuse so that it is hard to establish a close tie between regional R&D facilities and regional growth, except perhaps where it concerns special regional problems such as agriculture or water resources. There are many factors involved in regional economic development – raw materials; wage rates and the supply of skilled labor; the availability of capital; managerial and technical skills; transportation; and proximity of markets. However difficult it is to measure the effects of federal research and development programs, it is clear that such effects on regional development are both substantial and diverse as well as subtle. Programs in defense and space, in basic health research, in agriculture research, oceanography, natural resources and environmental control all involve extensive expenditures and so are obviously related in varying degrees to regional development. However, it is difficult to make general statements regarding the nature and magnitude of their regional impact.[9]

Other factors must be considered. Our society is mobile and the flow of information is highly fluid. A telephone in San Francisco can tie into a computer bank in Boston. Vast amounts of data in the form of graphs or tables can be transmitted visually by telephone. Research undertaken in one regional area can have its impact on distant regions. For example, our national program for producing nuclear energy has had its economic impact in the form of lower cost electrical power provided by large-scale nuclear power plants far removed from where the basic research took place. Thus, research and development can have a significant effect on the economic development of many areas, but it is difficult to predict where the particular benefits of any given effort will be realized.

Dr. Donald F. Hornig, Director of the Office of Science and Technology for President Johnson, in testimony before the Subcommittee on Government Research of the Committee on Government Operations of the U.S. Senate in July

1967, pointed out a number of examples of how the location of research facilities had had significant local economic impact. Dr. Hornig noted that:

> a. The Instrumentation Laboratory and Lincoln Laboratory at MIT had a major effect in "spinning off" many new science based companies in the Boston area.
> b. The existence of Stanford University and other research centers nearby is partly responsible for the location of many industries in the Palo Alto area.
> c. The presence of a major federal facility at Huntsville (Alabama) has encouraged other companies to come to that area.[10]

However, he contrasted these examples with situations where the economic impact of federal research facilities programs have not extended beyond direct effects of their payrolls and needs for services.

> a. The Brookhaven National Laboratory has not spawned industry in its vicinity; ordinary suburban development reaches almost to the gates of the laboratory.
> b. The University of Wisconsin at Madison, a major university and important center of scientific research, has spawned little industry in its immediate vicinity. Neither have the University of Illinois nor other great centers of research.
> c. The Arnold Engineering Development Center of the Air Force at Tullahoma, Tennessee, which supports research and development and conducts tests in the fields of aeronautics, aerospace, propulsion and ballistics, has not produced much nearby industrial development. Neither has the Los Alamos Scientific Laboratory in New Mexico or the Naval Ordnance Test Station in California.[11]

Dr. Hornig concluded that "we should proceed with the clear understanding that a strong center of science and technology does not automatically provide the conditions for local economic development."[12]

One further prefatory remark regarding the geographic distribution of R&D concerns private industry's reasons for choosing where to locate R&D programs. Among the three sectors – government, industry, educational institutions – industry by far has been the largest performer of R&D. Through the past 15 years during the tremendous growth in our nation's R&D spending, industry's share of total R&D outlay has fluctuated from 70 to 75 percent.[13] There are many reasons for industry's R&D location decisions.

Chambers of commerce, public utilities, local and regional political leaders and other interested parties and development organizations traditionally act to encourage industrial and other employing organizations to locate within a particular region. Other reasons contributing to the growth of science-based industry in a particular region have been identified by many authors and in many studies. Among the factors given, one could list the following:

a. Sufficient population concentration required to generate other institutions necessary to attract and support science-based industry.
b. The presence of cultural and educational facilities attractive to good professional people.
c. The presence of a good research-oriented industry or a strong technological school.
d. The existence of attractive state and local tax incentives for industrial location.
e. The presence of technically oriented entrepreneurs who develop R&D business for their region.
f. Regional leadership building on regional resources and supported by industrial and intellectual capacity as well as adequate financial underwriting.

Perhaps one of the best summary statements identifying factors which determine regional potential in science was made by Dr. Charles Kimball, President of Midwest Research Institute (MRI) before the Senate Subcommittee on Government Research, May 18, 1967. Identifying the prerequisites for regional potential in science, Dr. Kimball stated:

> There are perhaps four main factors: First is the overall attitude of the region and, second, the economic and industrial base of the region. . . Third, there is the adequacy of financial institutions in the region in terms of their size, tradition, structure, and sophistication. Will they recognize and support, to any degree, emerging centers of competence which have a strong technical orientation? The fourth factor is the degree of development of regional intellectual resources – the colleges and universities and their general outlook and orientation. Is it inward or outward? What is the level of real communications with the business and financial community in the region? For exampl the best scientific university in the world will contribute little to regional development if its communications channels are not open or active, and if the university management feels or implies that the business and financial community does not understand what it is trying to do. By the same token, the most aggressive business community in the country will have a very hard time capitalizing on intellectual resources that are indigenous to that region, if those resources do not want to be capitalized on or, more specifically, do not want to be bothered.[14]

The Elliott Committee

Over the decade 1955-1965, federal expenditures for R&D swelled from $2.7 billion to more than $15 billion. Beginning in the early sixties, concern about the federal government's role in scientific endeavors began to arouse public attention and catch the congressional eye. Although somewhat slow to respond, Congress finally decided to inquire into what was happening with the mammoth federal R&D outlays. On September 11, 1963, the House voted unanimously to establish a Select Committee on Government Research to examine the federal government's role in shaping scientific explorations. The Committee was created by unanimous passage of House Resolution 504 which directed the Committee "to make a complete, full, and thorough investigation of the numerous research programs being conducted by sundry departments and agencies of the Federal Government. . . ."[15] It was the intention of the Committee to find, reinforce and develop mechanisms for effective control and coordination of R&D.

The Committee was chaired by Representative Carl Elliott, an Alabama Democrat, and included four other Democrats: John Fogarty, Rhode Island; George P. Miller, California; Melvin Price, Illinois; and Phil M. Landrum, Georgia; and four Republicans – Clarence J. Brown, Ohio; John B. Anderson, Illinois; James C. Cleveland, New Hampshire; and Patrick Minor Martin, California. With the completion of the 88th Congress on January 4, 1965, the Committee ended its work after having accomplished the valuable service of drawing attention to the size and complexity of the government's role in scientific endeavors.

Unfortunately, the Committee was unable to provide the thorough job of a comprehensive investigation that it was charged to undertake. One obvious reason for its failure can be identified. Chairman Elliott faced a difficult reelection campaign in 1964. His home state had failed to redistrict so that all the House members in Alabama had to run at-large in the state instead of in their old districts. The statewide leadership under Governor George Wallace wished to penalize Elliott for his moderate attitudes toward racial issues and his general cooperation with the Johnson Administration. The White House used its political power to help establish the Select Committee in the hope that if Elliott were the chairman of a committee dealing with a subject so significant to Alabama, it might provide him with sufficient statewide visibility to be reelected. Congressman Elliott, however, was purged by the Wallace tactics as well as the Goldwater strength in Alabama and lost his bid for reelection. The Select Committee died with his defeat.

It should be recognized, nonetheless, that the Select Committee served an important integrating process which brought together a membership not wholly unfamiliar with R&D problems. For example, Congressman Miller was also the chairman of the Science and Astronautics Committee; both Anderson and Price were on the Joint Committee on Atomic Energy, and Price served on the Armed Services Committee. Together, their insights and investigation provided a valuable overview of the size and scope of federal R&D involvement.

Extensive hearings were held by the Committee, resulting in the issuance of ten studies. Committee Study No. VI, "Impact of Federal Research Development Programs," described in detail how federal R&D funds were distributed and included comments on the part educational institutions play in research and development. The thrust of this study documented the need for a broader distribution of R&D dollars on a geographic basis, while at the same time maintaining a high-level technical competence on federal research projects.

The Daddario Subcommittee

Continued congressional discontentment over unequal distribution of federal research and development dollars did not wane with the demise of the Elliott Committee, but rather resulted in a variety of additional reports and hearings showing how a handful of states apparently dominates the federal R&D program. The first comprehensive congressional look at the government's expanding involvement in R&D occurred with the submission in 1964 of a report to the Subcommittee on Science, Research, and Development of the House Committee on Science and Astronautics. In response to a request from that Subcommittee, the National Science Foundation (NSF) prepared a report titled *Obligations for Research and Development, and R&D Plant, by Geographic Divisions and States, by Selected Federal Agencies, Fiscal 1961-1964.* The report was based on data derived from a survey conducted by NSF during the period April-July 1964. Concurrently, five other reports were submitted by the Subcommittee dealing with federal involvement in R&D.[b]

In his letter of transmittal accompanying one of the Subcommittee's reports, the chairman, Representative Emilio Q. Daddario of Connecticut, pointed out its purpose:

> . . . I should like to suggest that the major aim of this report is to help point the way toward the development of useful Federal policies in the support of research and development in the years ahead. . . . However, preparation shou be made now in order to insure that all parts of the Nation possess the technological and scientific quality essential to the welfare of the people during that period.[16]

The findings of the National Science Foundation report to the Subcommittee were significant. The study documented that approximately 70% of federal research and development funds for all performers were allocated to nine states and the District of Columbia in both fiscal years 1963 and 1964. California was the major recipient with 35% of the federal total in both years, or over four times that of the next leading state, New York (8%). Maryland received 5% and only three other states (Texas, Massachusetts and Pennsylvania) had as much as 4%. Interestingly, all of these states are either on the oceans or the Gulf Coast.[17] The breakdown for FY 1963 appears in Table 2-1.

---

[b] The Subcommittee on Science, Research, and Development was comprised of the chairman, Democratic Representative Emilio Q. Daddario, Connecticut, five Democrats: J. Edward Roush, Indiana; Thomas G. Morris, New Mexico; John W. Davis, Georgia; Joe D. Waggonner, Jr., Louisiana; Edward J. Patten, New Jersey, and the Republicans: R. Walter Riehlman, New York; Charles A. Mosher, Ohio; Alphonzo Bell, California; James D. Weaver, Pennsylvania. The five reports (Committee Prints) produced by the Subcommittee were: No. 1 "A Statement of Purpose"; No. 2 "Fiscal Trends in Federal Research and Development"; No. 3 "Scientific-Technical Advice for Congress Needs and Sources"; No. 4 "Geographic Distribution of Federal Research and Development Funds"; No. 5 "Indirect Costs Under Federal Research Grants."

Table 2-1
Geographic Distribution of Federal R&D Funds, Fiscal Year, 1963

	Dept. of Defense	*NASA (Space Agency)*	*Other Agencies*	*Total*	*% of Total*
		(in millions of dollars)			
Alabama	$ 27.8	$ 102.5	$ 7.0	$ 137.3	1.3
Alaska.	2.2	1.3	2.1	5.6	**
Arizona	127.1	6.1	6.3	139.5	1.3
Arkansas	**	14.2	2.9	17.1	0.3
California. . . .	2,261.9	1,236.0	309.9	3,807.8	38.5
Colorado	203.5	5.7	12.4	221.6	2.2
Connecticut . .	71.0	7.8	44.2	123.0	1.2
Delaware	23.3	0.3	0.9	24.5	0.4
Florida	129.7	104.7	13.4	247.8	2.3
Georgia	2.2	5.0	8.1	15.3	0.3
Hawaii	1.4	0.1	2.6	4.1	**
Idaho	0.6	0.2	26.8	27.6	0.4
Illinois	84.0	13.1	94.0	191.1	1.9
Indiana	31.7	2.5	15.5	49.7	0.5
Iowa.	2.9	2.5	13.1	18.5	0.3
Kansas.	13.5	0.9	6.0	20.4	0.3
Kentucky	0.6	0.1	4.9	5.6	**
Louisiana	2.0	170.1	9.9	182.0	1.8
Maine	0.2	0.2	2.6	3.0	**
Maryland	241.3	52.3	31.0	324.6	3.3
Massachusetts .	328.8	42.5	80.6	451.9	4.6
Michigan	96.9	8.4	28.2	133.5	1.4
Minnesota . . .	41.6	8.3	19.0	68.9	0.7
Mississippi . . .	0.6	0.1	3.3	4.0	**
Missouri	16.4	201.5	14.9	232.8	2.2
Montana	3.1	0.1	1.6	4.8	**
Nebraska	0.3	**	3.4	3.7	**
Nevada	1.1	0.6	122.8	124.5	1.3
New Hampshire	11.0	0.6	2.6	14.2	0.2
New Jersey . . .	202.9	57.2	27.7	287.8	2.5
New Mexico . .	28.1	2.8	195.3	226.2	2.4
New York. . . .	631.8	79.8	205.8	917.4	9.2
North Carolina .	6.4	0.7	14.9	22.0	0.4
North Dakota .	1.2	0.0	1.1	2.3	**
Ohio.	118.8	27.6	39.3	185.7	1.9
Oklahoma. . . .	6.2	1.0	5.9	13.1	0.1
Oregon	2.0	0.4	10.2	12.6	0.1
Pennsylvania . .	162.1	47.8	139.1	349.0	3.5
Rhode Island . .	5.6	0.3	3.8	9.7	**
South Carolina .	0.4	0.1	14.2	14.7	0.2
South Dakota .	10.3	0.4	1.5	12.2	0.1
Tennessee. . . .	42.0	1.8	89.8	133.6	1.4
Texas	168.2	136.4	26.3	330.9	3.3
Utah.	113.9	0.3	5.8	120.0	1.2

Table 2-1 (Continued)
Geographic Distribution of Federal R&D Funds, Fiscal Year, 1963

	Dept. of Defense	*NASA (Space Agency)*	*Other Agencies*	*Total*	*% of Total*
		(in millions of dollars)			
Vermont	2.1	**	2.8	4.9	**
Virginia	36.6	26.7	9.8	73.1	0.7
Washington . . .	277.5	2.0	41.8	321.3	3.2
West Virginia . .	1.1	0.2	4.1	5.4	**
Wisconsin	63.8	12.0	19.2	95.0	1.0
Wyoming	1.5	0.0	0.8	2.3	**
District of Columbia . . .	37.9	64.1	18.5	120.5	1.2
U.S. TOTAL	$5,647.1	$2,449.3	$1,767.7	$9,884.1	

Note: Other agencies include Atomic Energy Commission; U.S. Dept. of Health, Education and Welfare; National Science Foundation; Departments of Agriculture, Interior and Commerce. Totals do not include awards for research and development facilities and are for extramural performers only.

*Less than $50,000.

**Less than 0.1%.

Source: U.S. Congress, House, Committee on Science and Astronautics, *Obligations for Research and Development, and R&D Plant, by Geographic Divisions and States, by Selected Federal Agencies, Fiscal Years 1961-1964*, Committee Print, 88th Congress, 2nd Session, September, 1964, pp. 56-59.

Taking the totals from Table 2-1 by state and aggregating them by region leads to the following distribution:

Region	*% Federal R&D Obligation*
Pacific (Washington, Oregon, California)	41.8
Mountain (Montana, Idaho, Wyoming, Nevada, Utah, Colorado, Arizona, New Mexico)	8.7
West North Central (North Dakota, South Dakota, Minnesota, Iowa, Nebraska, Kansas, Missouri)	3.6
West South Central (Texas, Oklahoma, Arkansas, Louisiana)	5.5
East North Central (Wisconsin, Michigan, Illinois, Indiana, Ohio)	6.6
East South Central (Kentucky, Tennessee, Alabama, Mississippi)	2.8
New England (Maine, Vermont, New Hampshire, Massachusetts, Connecticut, Rhode Island)	6.1
Middle Atlantic (New York, New Jersey, Pennsylvania)	15.7
South Atlantic (Virginia, West Virginia, Maryland, Delaware, North Carolina, South Carolina, Georgia, Florida)	8.5

The NSF study went on to document the percentage distribution of federal R&D obligations for educational institutions in 1963. Once again, California led the way, capturing almost 31% of approximately 1.5 billion R&D dollars. The distribution to educational institutions of the major states was as follows:[18]

State	*% of Total R&D Outlay, Educational Institutions*	*State*	*% of Total R&D Outlay, Educational Institutions*
California	31	Maryland	5
Massachusetts	12	Pennsylvania	3
New York	9	Michigan	3
Illinois	7	Ohio	2
New Mexico	6	New Jersey	2

Ten states had about 80 percent of the total federal obligations to educational institutions for R&D.

The NSF report also provided data on the geographic distribution of federal R&D obligations for profit organizations, or what could be classified as private industry. Representing almost two-thirds of the total, these obligations rose from approximately $5.8 billion in 1961 to almost $9.4 billion in 1964. Once again, the largest recipient was California, with its share of the total around 40 percent. In order of funding magnitude for 1963, the leading states were:[19]

State	*% of Total R&D Outlay, Profit Organizations*	*State*	*% of Total R&D Outlay, Profit Organizations*
California	44	New Jersey	3
New York	9	Maryland	3
South Carolina	4	Florida	3
Texas	4	Missouri	3
Washington	4	Massachusetts	3
Pennsylvania	4		

The Daddario Report substantiated the allegations of a very uneven distribution of contract dollars among the states. It inferred that steps should be taken to assure, particularly in the long run, a more uniform geographic distribution of R&D funds.

The Harris Subcommittee

Interest in the distribution of federal R&D dollars had been expressed at the highest governmental levels. On September 13, 1965, President Johnson issued a memorandum to the departments and agencies of the government, titled "Strengthening Academic Capability for Science Throughout the Country." In that memorandum, he made specific recommendations concerning the distribution of research contracts. On the following day in comment to the Cabinet on the same subject, the President stated:

> Our policies and attitudes in regards to science cannot satisfactorily be relate solely to achievement of goals and ends we set for our research. Our vision in this regard is limited at best. We must, I believe, devote ourselves purposefully to developing and diffusing – throughout the nation – a strong and solid scientific capability, especially in the many centers of advanced education. *Our future must rest upon diversity of inquiry as well as the universality of capability.*[20] (Emphasis added.)

"Research funds," President Johnson concluded, "are still concentrated in too few institutions in too few places of the country."[21]

Partly in response to President Johnson's memorandum, partly as a Midwest protest, and partly to provide a platform for Senator Fred R. Harris who faced a difficult election in Oklahoma, the Senate created a Subcommittee on Government Research of the Government Operations Committee. Harris was appointed chairman of the Subcommittee whose membership included Republican Senators Karl E. Mundt of South Dakota and Milward Simpson of Wyoming and Democratic Senators John McClellan of Arkansas, Joseph Montoya of New Mexico, and Abraham Ribicoff of Connecticut. The hearings of the Harris Subcommittee are entitled "Equitable Distribution of R&D Funds by Government Agencies," which suggests the dimension of the issues encompassed.[22]

Senate Resolution 231 which created the Harris Subcommittee was introduced by Senator Curtis with the cosponsorship of Senators Hruska, Mundt, Thurmond and Dominick. It called for an examination of techniques for using improved geographic distribution of research and development funds made available by the departments and agencies of the federal government. In introducing the resolution, Senator Curtis stated:

> The imbalance which must be righted is not only the increasing maldistribution of $15 to $20 billions of Federal research and development funds but the reversal of trends which this maldistribution has set in motion. Mind you, were we to delay this reversal for another decade, the Members of this body might be faced, in a score of years, with the fact that wide areas of this great Nation may then fail to possess the skills and talents to perform useful research in the basic sciences. Our great institutions of higher learning, in vast areas, may be reduced in scope to schools of narrower pursuits. Our laboratories and kindred facilities will move away, and will not then our industry follow that pattern?[23]

Senator Curtis went on to say:

> The industry of tomorrow will be located where the scientific complexes are now being located today, and Congress has an obligation to do equity and justice in determining where this money is being spent, loaned or given.[24]

At the conclusion of his introduction of the resolution, Senator Curtis asked three questions:

> First. Is the current geographic distribution of Federal research and development grants and contracts in American universities meeting the major program objectives of federal agencies, especially as these program objectives may involve a strengthening diffusion of higher education capacity for research activity over a long run? Second. Is the current general support of American universities enabling additional universities to advance their capacities for research activity? Third. What are the social and economic effects of this concentration of research and development programs?[25]

The Subcommittee was authorized by the Senate to make studies of the research and development programs financed by the federal government. One of its primary goals was to arrive at policy-making criteria for equitable distribution of such research and development contracts.

The Harris Subcommittee provided several interesting tables of data showing the distribution pattern for 1964 by both state and functional areas.[26] Senator Mundt, while placing the tables in the *Congressional Record*, noted:

> It is interesting and distressing to note that more than half of all research and development expenditures from Federal sources were spent in four States during fiscal year 1964. One state receives more than the total of the lowest 43 states. It is not the intention of this Senator to suggest that funds be taken from the affluent and showered upon the undernourished. But I would here point out that some departments have worked out quite equitable systems of distribution of research and development funds. The Department of Agriculture is using a program of distribution that provides a climate in which all States can participate and share in the burdens and fruits of their joint labors. This has worked to the benefit of all, and it can hardly be said that the agricultural community of the Nation has shirked its responsibility in research. They have an enviable tradition of good, sound research programs, going back many years. Similarly, the Department of the Interior has worked out a plan in which all States participate in cooperative research programs. Thus, if some departments have a workable solution to this difficult problem it would seem to me that others might try to accomplish similar distribution patterns.[27]

Tables 2-2, 2-3, and 2-4, which are referred to by Senator Mundt, indicate both geographic distribution by state and a breakdown of each state's share by agency.

Table 2-2

Research and Development for All Performers, Fiscal Year 1964 (arranged in alphabetical order)

State	Amount	% of Total	State	Amount	% of Total
Alabama	$ 314,069	2.2	New Hampshire	$ 22,306	.2
Alaska	13,155	.1	New Jersey	440,571	3.1
Arizona	88,429	.6	New Mexico	422,791	3.0
Arkansas	5,030	(1)	New York	1,133,240	7.9
California	4,953,121	34.6	North Carolina	27,389	.2
Colorado	264,504	1.8	North Dakota	33,832	.2
Connecticut	130,288	.9	Ohio	307,924	2.2
Delaware	5,627	(1)	Oklahoma	19,753	.1
District of Columbia	478,425	3.3	Oregon	19,831	.1
Florida	489,628	3.4	Pennsylvania	513,951	3.6
Georgia	48,135	.3	Rhode Island	27,120	.2
Hawaii	7,358	.1	South Carolina	17,571	.1
Idaho	31,334	.2	South Dakota	12,133	.1
Illinois	208,632	1.5	Tennessee	155,514	1.1
Indiana	103,117	.7	Texas	628,313	4.4
Iowa	25,128	.2	Utah	65,180	.5
Kansas	13,949	.1	Vermont	4,904	(1)
Kentucky	11,936	.1	Virginia	210,399	1.5
Louisiana	290,092	2.0	Washington	262,565	1.8
Maine	6,423	(1)	West Virginia	8,935	.1
Maryland	748,178	5.2	Wisconsin	69,983	.5
Missouri	386,378	2.7	Wyoming	29,531	.2
Montana	8,832	.1	Territories	7,418	.1
Nebraska	5,463	(1)	Abroad	3,739	(1)
Nevada	173,299	1.2	Unallocated	237,883	1.7
			Total	$14,315,604	—

(1) Less than 1/10 of 1 percent.

Source: U.S. Congress, Senate, Committee on Governmental Operations, *Equitable Distribution of R&D Funds by Government Agencies*, Hearings, 90th Congress, 1st Session, July 11, 17 & 18, 1967, p. 752.

Table 2-4
R&D Expenditure, Percent, Per capita, Fiscal Year 1964

State	*Amount (thousands)*	*Percent of total*	*Per capita*
Total	$14,315,604		
California	4,953,121	34.6	$315.14
New York	1,133,240	7.9	67.52
Maryland	748,178	5.2	241.29
Texas	628,313	4.4	65.58
Massachusetts	570,303	4.0	110.76
Pennsylvania	513,951	3.6	45.40
Florida	489,628	3.4	98.88
District of Columbia	478,425	3.3	– –
New Jersey	440,571	3.1	72.62
New Mexico	422,791	3.0	444.56
Missouri	386,378	2.7	89.42
Alabama	314,069	2.2	96.17
Ohio	307,924	2.2	31.72
Louisiana	290,092	2.0	89.06
Colorado	264,504	1.8	150.80
Washington	262,565	1.8	92.02
Virginia	210,399	1.5	53.04
Illinois	208,632	1.5	20.69
Nevada	173,299	1.2	607.47
Michigan	163,827	1.1	20.94
Tennessee	155,514	1.1	43.59
Connecticut	130,288	.9	51.39
Indiana	103,117	.7	22.11
Arizona	88,429	.6	67.91
Minnesota	72,474	.5	20.93
Wisconsin	69,983	.5	17.66
Utah	65,180	.5	73.18
Georgia	48,135	.3	12.20
North Dakota	33,832	.2	53.49
Idaho	31,334	.2	46.96
Wyoming	29,531	.2	89.47
North Carolina	27,389	.2	6.01
Rhode Island	27,120	.2	31.67
Iowa	25,128	.2	9.04
New Hampshire	22,306	.2	36.75
Oregon	19,834	.1	11.21
Oklahoma	19,753	.1	8.48
South Carolina	17,571	.1	7.39
Mississippi	15,691	.1	.20
Kansas	13,949	.1	6.40
Alaska	13,155	.1	8.16
South Dakota	12,133	.1	17.82
Kentucky	11,936	.1	3.92
West Virginia	8,935	.1	4.80
Montana	8,832	.1	13.06
Territories	7,418	.1	– –
Hawaii	7,358	.1	11.62
Maine	6,423	(1)	6.62
Delaware	5,627	(1)	12.38
Nebraska	5,463	(1)	3.87
Arkansas	5,030	(1)	3.21
Vermont	4,904	(1)	12.57
Abroad	3,739	(1)	– –
Unallocated	237,883	1.7	– –
National per capita	– –	– –	79.84

(1) Less than 1/10 of 1 percent, 1960 census.

Source: U.S. Congress, Senate, Committee on Government Operations, *Equitable Distribution of R&D Funds by Government Agencies*, Hearings, 90th Congress, 1st Session, July 11, 17 & 18, 1967, p. 761.

The Harris Subcommittee additionally listed R&D expenditures for all departments and agencies, by order of magnitude for fiscal year 1964 (see Table 2-4).

Testimony before this Subcommittee brought other significant information to light. For example, testimony by Dr. Donald F. Hornig, the Director of the Office of Science and Technology, noted data concerning the geographic distribution of research funds to universities. Dr. Hornig suggested that one of the major factors affecting the distribution is the existence of university capability to conduct high-quality research.

The ratio of federal obligations for R&D to Ph.D. output for various regions in fiscal year 1964-65 was as follows:[28]

Table 2-5
Regional Ph.D. Output and R&D Obligations

Region	*Federal R&D Funds Divided by Ph.D. Production*
New England	1.20
Middle Atlantic	1.07
East North Central	.75
West North Central	.65
South Atlantic	1.17
East South Central	1.21
West South Central	.95
Mountain	1.35
Pacific	1.17

Source: U.S. Congress, Senate, Committee on Government Operations, *Equitable Distribution of R&D Funds by Government Agencies*, Hearings, 90th Congress, 1st Session, July 11, 17 and 18, 1967, p. 71.

Although wide-ranging, these funds per Ph.D. awarded were much more evenly distributed than were funds per capita. Figure 2-1 shows that the regional totals extended from a per capita low of $2.40 to a high of $12.30.

At the conclusion of the extensive testimony, the Subcommittee reached four conclusions. First, given particular elements in a particular region or state including the spirit of entrepreneurship, the availability of risk capital and the interaction between academic research and industry, the expenditure of federal research and development funds in an institution of higher learning or in a state would have direct and immediate impact on the economic development and growth of that state or of the area served by the institution of higher education. Second, the presence of high-quality research in an institution of higher learning has direct impact on the quality of education received by the students in that institution. Third, for two reasons – economic impact and the impact upon the quality of education – the distribution of federal research and development funds as a matter of national policy should be made on an equitable basis throughout the country and among institutions of higher learning. And four, there is no easy way to provide a more equitable distribution of funds around the country.[29]

The Nelson Subcommittee

Meanwhile, in the Senate, the Subcommittee on Employment and Manpower of the Committee on Labor and Public Welfare became active in reviewing the impact of federal R&D expenditures on manpower, growth of employment prosperity, and concentration of expenditures in large and established firms and institutions. In 1965, the Subcommittee held hearings on the "Impact of Federal Research and Development Policies on Scientific and Technical Manpower." The Subcommittee chairman, Senator Joseph Clark of Pennsylvania, turned the chair over to Senator Gaylord Nelson, a Wisconsin Democrat, to conduct hearings on this particular problem. The Subcommittee was comprised of Democratic Senators Jennings Randolph of West Virginia, Pat McNamara of Michigan, and Claiborne Pell of Rhode Island. The Republicans were Winston L. Prouty of Vermont, Jacob K. Javits of New York, and George Murphy of California.

Senator Nelson, presiding pro tempore, stated the primary purpose of the Subcommittee:

> We are holding these hearings not because this subcommittee has any desire, or indeed any capacity, to study the details of scientific and technical efforts in this country; rather, we are interested in the effect of research and development policies, of scientific and technical efforts, upon the development, training, utilization, and employment of scientific and technical manpower.[30]

Regarding the distribution of research and development funds, Senator Nelson suggested that it was the purpose of the Subcommittee to determine:

> . . . what it is, to what extent this is inevitable or useful, to what extent it promotes the development of various regions, or hinders the development of others, to what extent the pattern derives from established policies, to what extent it is the result of initiative or lack of initiative in given regions, to what extent existing policies are serving the national goal of wise utilization of our manpower and wise employment of all our resources, and to what extent new policies might be in order.
>
> Whenever the Geographic Distribution of funds is questioned or even examined, someone is certain to cry "pork-barrel politics." It will be claimed that we are only trying to see that one state gets its share of the federal money, even if this is not the wisest or most economical way to do a job. The Subcommittee has no interest in this kind of narrow approach to the problem.
>
> However, it often seems that those who cry "pork-barrel politics" are the "haves" who oppose the "have nots." If this is the case, it reflects the unfortunate shortsightedness which could have disastrous effects upon our national progress. For it is obvious that more and more states are worried about this issue, unless we devise national policies, we shall end up the victim of pork-barrel politics.[31]

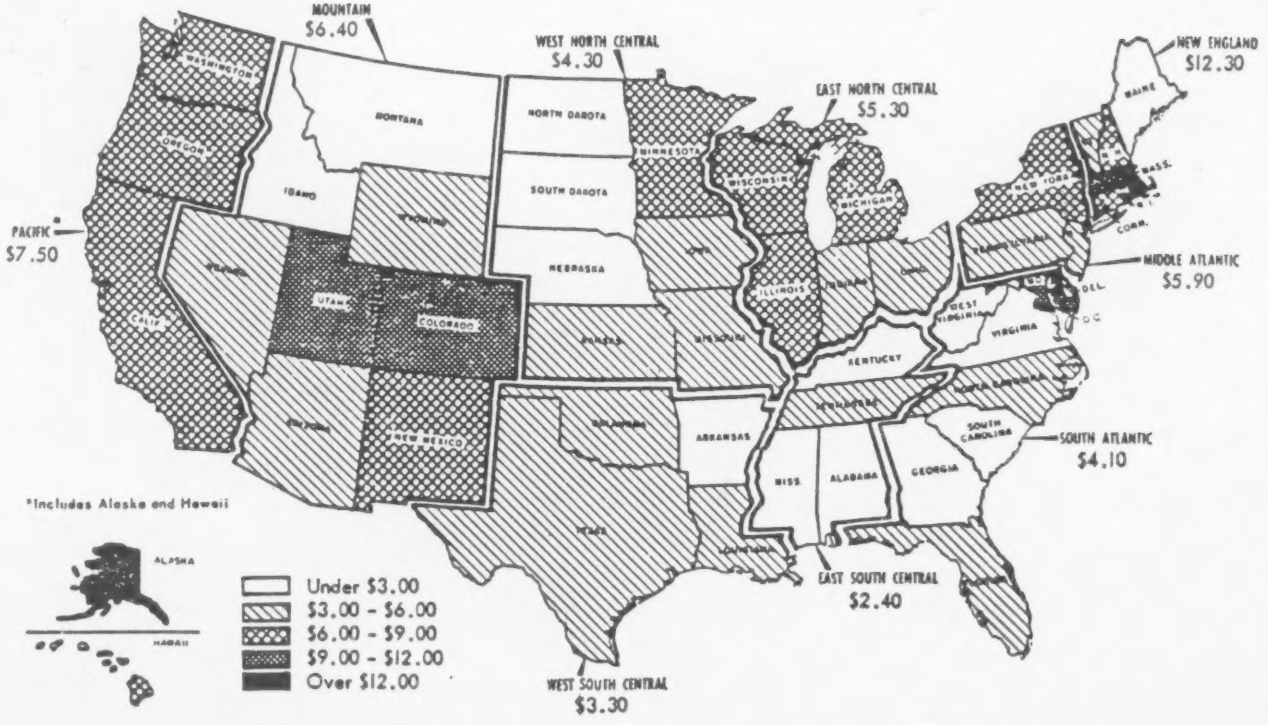

MOUNTAIN
$6.40
WEST NORTH CENTRAL
$4.30
EAST NORTH CENTRAL
$5.30
NEW ENGLAND
$12.30
PACIFIC*
$7.50
MIDDLE ATLANTIC
$5.90
SOUTH ATLANTIC
$4.10
EAST SOUTH CENTRAL
$2.40
WEST SOUTH CENTRAL
$3.30
WASHINGTON
OREGON
CALIF.
IDAHO
MONTANA
WYOMING
NEVADA
UTAH
COLORADO
ARIZONA
NEW MEXICO
NORTH DAKOTA
SOUTH DAKOTA
NEBRASKA
KANSAS
OKLAHOMA
TEXAS
MINNESOTA
IOWA
MISSOURI
ARKANSAS
LOUISIANA
WISCONSIN
ILLINOIS
MICHIGAN
INDIANA
OHIO
KENTUCKY
TENNESSEE
MISS.
ALABAMA
GEORGIA
FLORIDA
SOUTH CAROLINA
NORTH CAROLINA
VIRGINIA
WEST VIRGINIA
PENNSYLVANIA
NEW YORK
MAINE
N.H.
MASS.
CONN.
DEL.
D.C.
*Includes Alaska and Hawaii
ALASKA
HAWAII
Under $3.00
$3.00 - $6.00
$6.00 - $9.00
$9.00 - $12.00
Over $12.00

Figure 2-1. Federal R&D Obligations to Universities and Colleges Proper Per Capita, by Geographic Division and State, 1965 (Entire U.S. $5.50). Source: U.S. Congress, Senate, Committee on Government Operations, *Equitable Distribution of R&D Funds by Government Agencies*, Hearings, 90th Congress, 1st Session, July 11, 17 and 18,.1967, p. 32.

Again, testimony before the Subcommittee included data on the distribution of federal research and development funds. However, most of it was a reiteration of National Science Foundation material developed for the Daddario Committee. There was extensive testimony documenting the spread of scientific and technological manpower throughout the country. This distribution closely paralleled the national distribution of R&D. Of the 224,000 scientists in the 1964 National Register of Scientific and Technical Personnel, New York and California reported the greatest number of respondents, 27,000 and 25,000, respectively. Five states (Pennsylvania, New Jersey, Illinois, Texas and Ohio) reported over 10,000 scientists responding. Between 5,000 and 10,000 scientists were located in Massachusetts, Michigan, the District of Columbia, and Maryland.[32]

A highlight of the Nelson Hearings was a response to questions raised by Senator Nelson about the distribution of federally funded R&D activities of NASA and their relationship to manpower utilization by the Space Agency. In response to these questions, Mr. Earl Hilburn, NASA Deputy Associate Administrator, testified on June 9, 1965. His testimony was based on a lengthy document responding in detail to questions about NASA's policy toward geographic distribution of NASA procurement. The data in the response are especially significant in light of the fact that 49% of NASA procurement awards in 1964 went to California (47% in 1962; 50% in 1963).[33]

Senator Nelson stated, "The Daddario Subcommittee concluded that to a pronounced degree the geographical pattern of fund distribution is 'uneven.' We would welcome your comments on this conclusion."[34] In reply to the Senator's question, the prepared statement submitted by Mr. Hilburn noted that NASA policy with regard to contract awards on the basis of geographic distribution was covered in a letter from NASA Administrator James E. Webb to Mr. Frank R. Hammil, Jr., Counsel for the House Committee on Science and Astronautics. In that letter, dated April 5, 1965, Mr. Webb stated:

> To base the award of contracts on geographical consideration, rather than on competition for all companies regardless of location, would be inconsistent with the statutory procurement authority currently applicable to NASA. Moreover, limiting the competition to geographical areas might mean that the company with the best capability for a project of importance would not be awarded a contract because of its location.[35]

Mr. Webb went on to state that he thought it forseeable that the NASA contract program would attract companies which heretofore had not competed for NASA contracts. He expressed hope that these companies would be successful in bringing work to areas which did not have a substantial amount of NASA contracts.[36]

In December 1965, the Nelson Subcommittee issued its recommendations.[37] The Subcommittee identified some of the causes for the present distribution of R&D funds and made recommendations that sought to "increase the benefits from Federal research and development to the less developed regions of the Nation."[38] Senators Javits and Murphy found it difficult to support the findings and recommendations. That the Senators were from states receiving the largest shares of federal R&D dollars may have had something to do with their dissenting view.

Senator Murphy expressed his position quite clearly:

> I wish to disassociate myself from the findings and implications of this subcommittee report that the Federal research and development funds should be awarded with any objective in mind other than that of obtaining the best value for the dollar spent.[39]

Senator Javits pointed out that the "unique aptitudes of our (New York) population mix" as well as "the many advantages which New York State offers in the field of scientific research and technological development combined to offer unique capabilities in the fields of research and development." The Senator was not sure that the state was being adequately recognized for its R&D capabilities "even now as much as it deserves."[40]

The Roush Amendment

One additional significant event was the so-called Roush Amendment to the NASA Authorization Bill for fiscal year 1966. Congress in 1965 authorized $5,190,396,200 for the National Aeronautics and Space Administration for the following year. The bill, H. R. 7717, authorizing these funds included a provision in the form of an amendment submitted in Committee by Representative J. Edward Roush of Indiana stating the sense of the Congress was that "it is in the national interest to distribute funds as widely as possible across the nation" and that NASA "should explore ways and means of distributing its research and development funds whenever feasible."[41] The provisions reflected concern in Congress that NASA funds were being concentrated in the coastal states.

The NASA bill passed the House by a roll-call vote of 389-11. There was no specific vote on the Roush Amendment. Debate on the bill was conducted almost entirely by members of the House Science and Astronautics Committee which ha written the bill. Many members did not fully understand the implications. As Clarence J. Brown, a Republican of Ohio, put it in asking approval for an open rule for three hours of debate on the bill, "I want to be very frank, very sincere, and very honest with the House and admit that I did not know anything about this legislation and know very little about NASA and what it is doing. I do not think anyone else in the House knows too much about it either. . . . We are goin to have to take it on faith. . . ."[42]

All eleven votes cast against H.R. 7717 were by Republicans. Melvin R. Laird (R. Wis.), Chairman of the House Republican Conference at that time, explained his reasons for voting against the bill by noting that from 78 to 80% of NASA funds were being spent in coastal states. Laird pointed out, "this is the only method available to those of us in the great central sections of the United States in expressing our deep concern and sincere disappointment over the failure of a national space agency to follow through on their earlier commitments to recognize our section of the country."[43] In addition to Laird, eight other Midwesterners (Nelson of Minnesota, Clancey and Devine of Ohio, Michel, Reid and Collier of Illinois, Cross of Iowa, and Skubitz of Kansas) and two Californians (Utt and Younger) voted against the NASA authorization.

During the Senate debate, William Proxmire of Wisconsin echoed Laird's concern. Proxmire stated "as a Senator from the Midwest," he was troubled by the fact that "approximately 80% of the NASA budget is spent on the coastal states, in the well-known 'space crescent' extending from Florida around through Texas." Senator Proxmire went on to point out that the Senate Aeronautical and Space Sciences Committee had amended the House provision of H.R. 7717 to urge the distribution of funds on a geographical basis by dropping the geographical reference. He said the Committee had made "this innocuous but laudatory provision more innocuous and less laudatory," and that as the bill stood, NASA could distribute funds "perhaps upon some other criteria, like the size of the business."[44]

Chairman Clinton P. Anderson (D. N.M.) responded that earlier language in the provision referred specifically to geographic distribution, and that there was no question but that the Committee intended that the expenditure of R&D funds "should involve attention to geographic distributions." Furthermore, Anderson pointed out, "the desire for wide-spread distribution is limited by other, more important considerations," such as the question of "who can best and most economically do the job on the basis of past performance records and bids."[45]

The National Science Foundation Studies

In both 1967 and 1969, the National Science Foundation (NSF) published new data concerning geographic distribution of R&D funds for fiscal years 1965 and 1968, respectively.[46] (See Figure 2-2.) The purpose of these reports was to provide information both as to total dollars and as to the various R&D performers and the agencies that provided funds. While NSF's first major survey of the geographic distribution of federal R&D funds was at the request of the Daddario Subcommittee, the 1967 and 1969 reports were designed to provide more recent information regarding the geographic distribution of federal R&D funds for those who make national scientific policy. Although the full impact of R&D funds cannot be evaluated or described by statistical tables alone, such background data

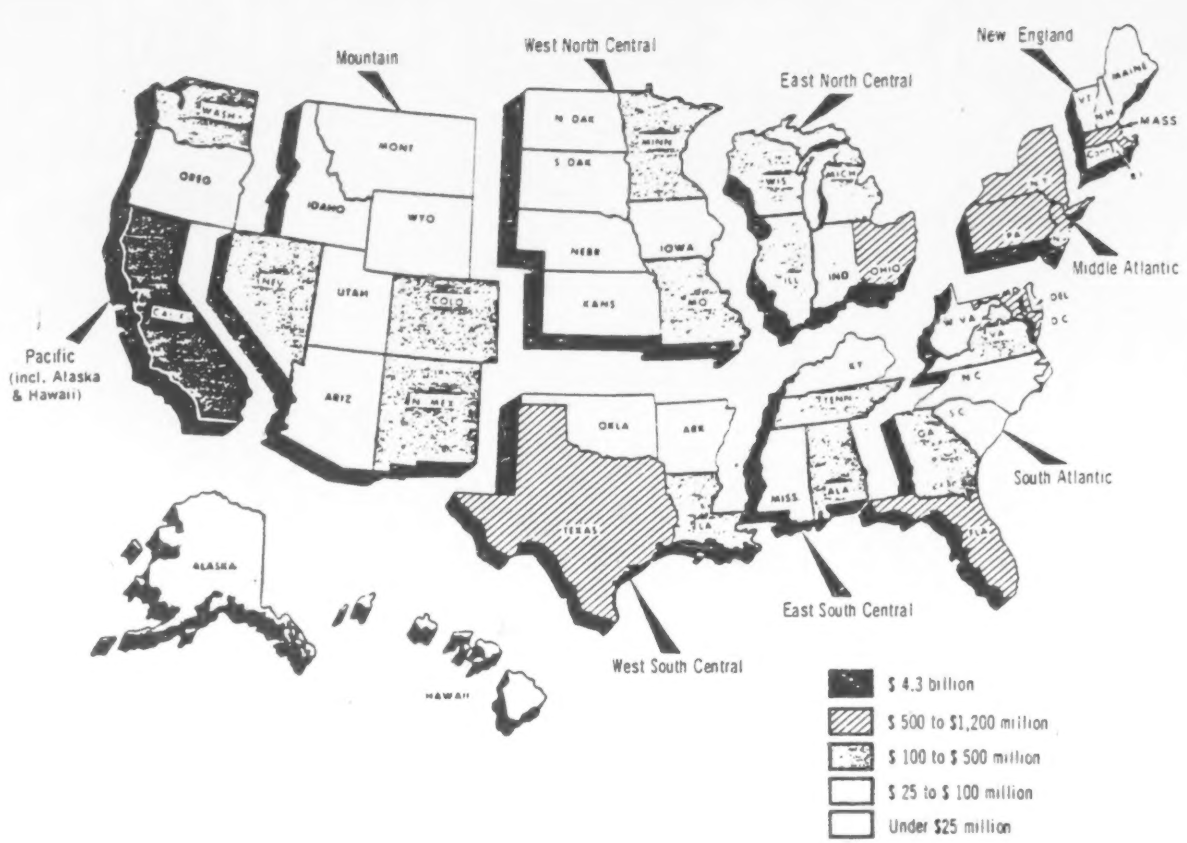

Mountain
West North Central
East North Central
New England
Middle Atlantic
South Atlantic
East South Central
West South Central
Pacific (incl. Alaska & Hawaii)
WASH
OREG
CALIF
IDAHO
MONT
WYO
NEV
UTAH
COLO
ARIZ
N MEX
N DAK
S DAK
NEBR
KANS
MINN
IOWA
MO
OKLA
ARK
TEXAS
LA
WIS
MICH
ILL
IND
OHIO
KY
TENN
MISS
ALA
GA
FLA
SC
NC
VA
W VA
DEL
DC
PA
NY
VT
NH
MAINE
MASS
CONN
RI
ALASKA
HAWAII
$ 4.3 billion
$ 500 to $1,200 million
$ 100 to $ 500 million
$ 25 to $ 100 million
Under $25 million

Figure 2-2. Distribution of total Federal R&D obligations by State, FY1968. Source: National Science Foundation, *Federal Funds for Research, Development and other Scientific Activities, FY 1968, 1969,*

are needed to evaluate the direct and indirect effects of federal R&D spending on any particular area

Table 2-6 shows the percentage of distribution of federal obligations for research and development by each agency and by each state for fiscal 1968, listed in order of total federal R&D obligations.

All NSF surveys were based on the reports of agencies whose combined obligations represented more than 90 percent of the federal total, and in all three years examined, there was a tendency for federal R&D funds to cluster within the ten leading support states.[47] In fact, the ten states which lead in federal R&D activity in 1963, 1965, and 1968 were from a list of eleven states. Eight states were always among the "leading ten," and three – Ohio, the District of Columbia (referred to as a State for the purpose of the studies), and New Mexico – moved on and off the list.

As indicated in Table 2-7, there was a tendency toward greater dispersion of funds among the top ten states during the period of the three studies; however, it is significant to note that the "other states" percentage remained almost constant (28-29 percent). The leading states during all three studies, California, New York, and Maryland, together declined from a combined total of 48 percent in 1963 to 41 percent in 1968. The "first five" group also saw a decline from 56 percent in 1963 to 51 percent in 1968. Although California continued to have the largest share during the period 1963-1968, its share dropped from 35 percent in fiscal 1963 to 32 percent in fiscal 1965, and to 27 percent in fiscal 1968.[48]

Since 1963, there have been some changes in the distribution of R&D funds by geographic region, which are noted in Table 2-8. The Pacific division has shown a continuing decline although it continues to receive the largest share. The change in California's position accounted for the greatest part of the Pacific division decline.

Sizable dollar gains in the South Atlantic division made this the division with the most rapid growth. The primary causes were NASA's activities at the Kennedy Space Center in Florida; the expansion at NASA's Goddard Space Flight Center at Greenbelt, Maryland; plus work for DOD by numerous industrial firms in Georgia, especially the Poseidon missile and the C-5A.

The Middle Atlantic division moved down from second place to third in 1968 although it showed a slight gain to 16 percent. Together, these three "coastal" divisions continued to receive almost two-thirds of all funds (64%).

Perhaps the best overview of the location of federal obligations for R&D by state and by region over time can be seen in Table 2-9. Only seven states (Alabama, South Carolina, Arizona, Montana, Connecticut, Kansas and South Dakota) saw a net decrease in federal R&D obligations from 1963 to 1968. None of these states was a major recipient of federal R&D dollars. The largest net increases occurred in Texas and Florida, primarily as a result of NASA outlays for the nation's space program. It is interesting to note that California, the largest recipient, witnessed a net increase in R&D obligations of only 1.9 percent from 1963 to

Table 2-6
Percent Distribution of Federal Obligations for Research and Development of Each Agency, By State, Fiscal Year 1968

State (in order of total federal R&D obligations)	*Total*	*Dept. of Agri- culture*	*Dept. of Commerce*	*Dept. of Defense*	*Dept. of Health, Education & Welfare*	*Dept. of Interior*	*Dept. of Trans- portation*	*Atomic Energy Commission*	*National Aeronautics & Space Admin.*	*National Science Foundation*
Total, all states	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
California	27.4	6.8	5.6	28.2	10.9	10.8	7.5	21.2	36.5	13.0
New York	7.5	3.3	3.8	4.6	12.7	2.4	29.6	9.0	10.0	10.5
Maryland	5.8	12.6	35.6	5.5	15.9	3.9	3.5	1.1	4.4	1.8
Texas	5.6	3.6	2.2	6.2	3.2	3.4	1.3	.3	7.6	2.2
Florida	5.1	2.5	4.0	4.5	1.3	1.9	1.9	.2	9.3	3.4
Massachusetts	5.1	.4	1.8	7.1	7.2	3.7	4.0	1.8	2.2	8.2
New Jersey	4.3	.6	.3	7.1	1.4	1.9	13.6	1.2	1.5	2.1
Ohio	4.2	1.6	1.0	6.3	3.4	2.6	1.8	1.6	1.9	4.0
Pennsylvania	3.9	3.4	.9	4.7	5.0	6.7	1.3	7.2	1.4	3.9
Dist. of Columbia	2.8	7.6	13.6	2.4	4.1	6.5	12.3	.2	2.9	6.7
New Mexico	2.6	.6	.4	2.4	.3	1.2	.1	14.8	.3	.4
Alabama	2.6	1.1	.1	1.2	.8	.7	.2	*	6.8	.1
Washington	2.2	2.0	.4	2.8	1.6	4.7	9.7	3.6	.4	3.0
Georgia	1.8	3.4	.9	3.1	1.6	1.3	.3	.1	.1	1.1
Virginia	1.8	1.3	1.6	1.8	1.1	1.3	3.6	.2	2.3	1.2
Louisiana	1.7	4.6	.3	.1	1.1	.5	.6	*	5.4	.6
Colorado	1.7	1.6	14.0	2.2	1.1	6.5	.2	.2	.8	5.8
Illinois	1.5	3.7	1.4	.8	4.0	1.3	.9	6.5	.2	5.6
Nevada	1.4	.3	*	.6	.1	2.2	*	11.7	.3	.2
Michigan	1.3	1.4	.7	1.5	2.7	1.8	.8	.5	.6	3.5
Tennessee	1.3	.9	.3	.8	1.2	1.2	.1	7.1	.3	1.2
Connecticut	.8	.5	1.1	1.0	1.4	.8	.7	.5	.5	1.4
Missouri	.8	1.3	.8	.7	2.2	1.3	.3	*	.6	1.1
Wisconsin	.7	3.0	1.0	.3	1.9	1.1	*	.3	1.1	1.9
Minnesota	.7	2.1	.6	.6	1.8	2.2	.2	.4	.5	1.3

Table 2-6 (Continued)
Percent Distribution of Federal Obligations for Research and Development of Each Agency, By State, Fiscal Year 1968

State (in order of total federal R&D obligations)	*Total*	*Dept. of Agri-culture*	*Dept. of Commerce*	*Dept. of Defense*	*Dept. of Health, Education & Welfare*	*Dept. of Interior*	*Dept. of Trans-portation*	*Atomic Energy Commission*	*National Aeronautics & Space Admin.*	*National Science Foundation*
Indiana	.5	1.2	.1	.7	1.1	.2	.3	.2	.1	2.5
Arizona	.5	1.9	.2	.6	.2	2.0	.1	.1	.2	2.2
Idaho	.4	1.2	–	*	*	.8	*	4.4	*	*
North Carolina	.4	2.1	.8	.2	2.5	1.5	.3	.1	.1	1.1
Alaska	.4	.6	.5	.1	.2	3.5	.1	2.9	.1	.5
Iowa	.3	2.8	*	.1	.7	.3	.1	.6	.1	1.2
Utah	.3	1.0	.3	.3	.6	1.9	.1	.1	*	.6
New Hampshire	.2	.4	.1	.3	.2	.3	*	*	.1	.5
Hawaii	.2	.5	.3	.2	.4	1.2	*	.2	.1	.7
Oregon	.2	1.8	*	*	1.4	3.3	*	.1	*	1.2
Rhode Island	.2	.2	.3	.2	.4	.6	1.0	.1	*	.8
Mississippi	.2	2.9	.3	.1	.3	.7	.3	*	.1	.1
Kentucky	.2	1.4	.7	.1	.6	1.0	.3	*	*	.5
Oklahoma	.2	1.0	1.6	.1	.4	1.3	1.2	*	*	.4
West Virginia	.1	.9	.2	.1	.3	2.1	.2	*	*	1.0
Kansas	.1	.8	.1	.1	.7	.3	.1	*	*	.5
South Carolina	.1	1.4	.4	*	.2	.2	.1	.7	*	.1
Nebraska	.1	1.0	*	.1	.3	.3	.3	.2	*	.2
Delaware	.1	.2	–	*	.2	.3	*	*	.2	.4
Montana	.1	1.2	.1	*	.2	.7	.1	*	*	.2
Arkansas	.1	.7	.3	*	.3	.4	.1	*	*	.1
Vermont	*	.4	.2	*	.2	.1	.1	*	*	.1
Wyoming	*	.4	.1	*	*	2.1	.1	*	*	.2
South Dakota	*	.7	.1	*	*	.8	.2	*	*	.2
North Dakota	*	1.4	*	*	.1	.6	.1	*	*	.1
Maine	*	.4	.3	*	.2	.8	.2	*	*	.1
U.S. Territories	.1	.9	.8	*	.4	.4	*	.3	*	*
Offices Abroad	.3	.3	–	.2	*	–	–	.3	.7	.3

Source: National Science Foundation, *Federal Funds for Research, Development and Other Scientific Activities, FY 1968, 1969, and 1970,* Volume XVIII, NSF 69-31, August 1969, p. 227.

Table 2-7
Distribution of Federal R&D Obligations for Fiscal Years 1963, 1965, and 1968 to the 10 States Leading in Such Support in 1968

	1963	*1965*	*1968*
Total, all States (millions of dollars)	$12,250.5	$14,356.8	$15,690.2
Percent Distribution			
California	35.1	31.7	27.4
New York	7.7	9.0	7.5
Maryland	5.5	6.1	5.8
Texas	3.2	5.1	5.6
Florida	2.8	3.2	5.1
Massachusetts	4.2	5.1	5.1
New Jersey	3.3	2.9	4.3
Ohio	2.5	2.6	4.2
Pennsylvania	3.6	3.1	3.9
District of Columbia	3.3	2.6	2.8
Other States*	28.8	28.6	28.3

*Includes territories and offices abroad.

Source: National Science Foundation, *Federal Funds for Research, Development and Other Scientific Activities, FY 1968, 1969, 1970,* Volume XVIII, NSF 69-31, August, 1969, p. 60.

Table 2-8
Distribution of Federal R&D Obligations by Geographic Division, Fiscal Years 1963, 1965 and 1968

Division	*1963*	*1965*	*1968*
Total (millions of dollars)	$12,250.5	$14,356.8	$15,690.2
Percent Distribution			
Pacific	38.1	33.8	30.4
South Atlantic	13.8	15.0	18.0
Middle Atlantic	14.6	15.5	15.7
East North Central	6.7	6.4	8.2
West South Central	5.0	8.0	7.6
Mountain	8.7	6.9	7.0
New England	5.7	6.9	6.4
East South Central	3.4	4.4	4.2
West North Central	3.1	2.8	2.0

Note: Percents do not add to 100 because territories have been excluded.

Source: National Science Foundation, *Federal Funds for Research, Development and Other Scientific Activities, FY 1968, 1969, 1970,* Volume XVIII, NSF 69-31, August, 1969, p. 58.

Table 2-9
R&D Obligations by Geographic Division and State, Fiscal Years 1963, 1965 and 1968
(Millions of dollars)

Division and State	*1963*	*1965*	*1968*	*Net Increase/ decrease, 1963-68*
United States, total	$12,250.5	$14,356.8	$15,690.2	+$3,439.7
Pacific Division	4,665.2	4,849.0	4,774.3	+109.0
Alaska	12.1	14.4	61.8	+49.7
California	4,296.7	4,553.3	4,298.6	+1.9
Hawaii	7.1	41.5	35.0	+27.9
Oregon	18.2	25.6	35.0	+16.8
Washington	331.1	214.3	343.9	+12.8
South Atlantic	1,685.3	2,154.9	2,819.7	+1,134.4
Delaware	24.6	7.1	13.9	−10.7
District of Columbia	403.9	374.3	443.2	+39.3
Florida	340.9	459.8	799.1	+458.2
Georgia	26.7	58.4	280.7	+254.0
Maryland	675.7	876.6	905.9	+230.2
North Carolina	27.0	57.8	62.4	+35.4
South Carolina	19.8	17.1	18.7	−1.1
Virginia	158.8	284.2	275.2	+116.4
West Virginia	7.9	19.6	20.6	+12.7
Middle Atlantic	1,789.2	2,228.6	2,473.4	+684.2
New Jersey	410.1	410.7	681.8	+271.7
New York	944.2	1,289.3	1,174.9	+230.7
Pennsylvania	434.9	528.7	616.7	+181.8
East North Central	819.5	923.7	1,294.0	+474.5
Illinois	203.9	191.7	239.3	+35.4
Indiana	57.3	71.9	82.9	+25.6
Michigan	154.4	155.2	203.4	+49.0
Ohio	304.3	379.1	653.6	+349.3
Wisconsin	99.7	125.8	114.8	+15.1
West South Central	608.2	1,143.1	1,188.9	+580.7
Arkansas	4.2	6.6	8.1	+3.9
Louisiana	189.2	377.1	272.4	+83.2
Oklahoma	16.9	28.4	24.0	+7.1
Texas	397.9	731.0	884.4	+486.5
Mountain	1,068.6	990.1	1,093.6	+25.0
Arizona	174.6	76.6	71.4	−103.2
Colorado	240.8	212.3	265.3	+24.5
Idaho	29.7	63.6	65.6	+35.9
Montana	8.5	8.6	8.3	−0.2
Nevada	128.9	154.5	223.6	+94.7
New Mexico	345.9	425.3	413.3	+67.4
Utah	135.8	45.0	39.7	−96.1
Wyoming	4.5	4.2	6.4	+1.9
New England	702.2	992.7	1,009.4	+307.2
Connecticut	139.3	184.5	133.2	−6.1
Maine	5.3	4.3	6.0	+.7
Massachusetts	515.1	733.7	795.7	+280.6
New Hampshire	16.9	28.8	36.6	+19.7
Rhode Island	20.5	37.5	31.1	+10.6
Vermont	5.2	4.0	6.8	+1.6

Table 2-9 (Continued)
R&D Obligations by Geographic Division and State, Fiscal Years 1963, 1965 and 1968 (Millions of dollars)

Division and State	*1963*	*1965*	*1968*	*Net Increase/ decrease, 1963-68*
East South Central	420.1	628.3	655.3	+235.2
Alabama	246.8	370.7	405.7	+158.9
Kentucky	11.0	17.1	24.1	+13.1
Mississippi	16.5	36.7	26.5	+10.0
Tennessee	145.8	203.7	199.1	+53.3
West North Central	376.7	408.7	321.1	-55.6
Iowa	23.3	28.8	41.7	+18.4
Kansas	21.5	25.7	19.8	-1.7
Minnesota	73.6	106.3	111.1	+37.5
Missouri	236.6	231.7	121.1	-115.5
Nebraska	4.8	7.7	15.4	+10.6
North Dakota	3.4	5.0	6.0	+2.6
South Dakota	13.5	3.5	6.0	-7.5

Note: U.S. territories and offices abroad included in the totals but not shown separately.

Source: National Science Foundation, *Federal Funds for Research, Development and Other Scientific Activities, FY 1968, 1969, 1970,* Volume XVIII, NSF 69-31, August, 1969. p. 54.

1968, less than 40 other states. The largest net decrease from 1963 to 1968 (–115.5 percent) occurred in Missouri largely as a result of the completion of NASA projects Mercury and Gemini at McDonnell Aircraft in St. Louis.[49]

Table 2-10 indicates the relationship among total federal R&D obligations, population, income, and federal tax collections. A number of interesting patterns emerge. For example, four states (California, Maryland, Massachusetts, and New Mexico) together accounted for almost 46 percent of the total federal R&D outlay in both FY 1965 and FY 1968, a proportion almost three times greater than their share of the population, personal income, or federal tax collections.

Extensive consideration in the NSF studies was given to the distribution of funds by agency. The studies pointed out that "in the final analysis the individual federal agencies had determined the pattern of R&D distribution by their selection of performers."[50] It is each agency which selects all R&D performers according to the prescribed requirements of the job. The studies note that the Department of Defense (DOD) has had more impact on a greater number of states than any other federal agency, and a greater dollar impact. The DOD was followed by the National Aeronautics and Space Administration (NASA). Together, these two agencies accounted for 82 percent of federal R&D obligations in 1965 and 77 percent in 1968.[51]

Table 2-10
Distribution of Federal R&D Obligations by State Compared With Other Measures of National Activity by State, 1968

	Total Federal R&D obligations		*Population*		*Total personal income*		*Total Federal taxes*[a]	
State	*Rank*	*Percent*	*Rank*	*Percent*	*Rank*	*Percent*	*Rank*	*Percent*
United States, total (in millions)	$15,690		200[b]		$683,702		$157,622	
California	1	27.40	1	9.66	1	11.20	2	8.77
New York	2	7.49	2	9.05	2	10.98	1	18.43
Maryland	3	5.77	18	1.88	15	2.05	11	2.41
Texas	4	5.64	5	5.49	6	4.86	7	3.96
Florida	5	5.09	9	3.07	10	2.87	16	1.72
Massachusetts	6	5.07	10	2.74	9	3.07	8	2.81
New Jersey	7	4.35	8	3.55	8	4.10	10	2.46
Ohio	8	4.17	6	5.30	5	5.43	6	6.05
Pennsylvania	9	3.93	3	5.87	4	5.87	5	6.25
District of Columbia	10	2.82	40	.40	36	.52	(c)	(c)
New Mexico	11	2.63	37	.50	41	.39	42	.19
Alabama	12	2.59	21	1.78	25	1.22	29	.64
Washington	13	2.19	22	1.64	20	1.77	21	1.34
Georgia	14	1.79	15	2.29	17	1.86	19	1.47
Virginia	15	1.75	14	2.30	14	2.06	20	1.46
Louisiana	16	1.74	19	1.86	22	1.44	25	.88
Colorado	17	1.69	30	1.02	28	1.00	22	1.16
Illinois	18	1.53	4	5.50	3	6.40	3	8.00
Nevada	19	1.43	48	.22	47	.26	44	.17
Michigan	20	1.30	7	4.37	7	4.70	4	7.48
Tennessee	21	1.27	17	1.99	21	1.50	23	.96
Connecticut	22	.85	24	1.48	18	1.84	14	1.82
Missouri	23	.77	13	2.31	12	2.20	37	.30
Wisconsin	24	.73	16	2.11	13	2.08	15	1.82
Minnesota	25	.71	20	1.82	19	1.78	17	1.72
Indiana	26	.53	12	2.53	11	2.53	12	2.28
Arizona	27	.45	34	.83	31	.74	34	.34
Idaho	28	.42	42	.35	46	.27	43	.19
North Carolina	29	.40	11	2.56	16	2.00	13	2.16
Alaska	30	.39	51	.14	50	.17	50	.06
Iowa	31	.27	25	1.39	23	1.32	26	.76
Utah	32	.25	36	.52	38	.42	41	.22
New Hampshire	33	.23	43	.35	42	.33	40	.23
Hawaii	34	.22	41	.39	40	.40	38	.25
Oregon	35	.22	31	1.00	29	.97	28	.66
Rhode Island	36	.20	39	.46	37	.47	33	.46
Mississippi	37	.17	28	1.17	32	.71	37	.30
Kentucky	38	.15	23	1.61	24	1.25	18	1.51
Oklahoma	39	.15	27	1.26	27	1.06	24	.93
West Virginia	40	.13	33	.90	35	.65	35	.31
Kansas	41	.13	29	1.15	26	1.11	30	.62
South Carolina	42	.12	26	1.33	30	.93	32	.47

Table 2-10 (Continued)
Distribution of Federal R&D Obligations by State Compared with Other Measures of National Activity by State, 1968

State	*Total Federal R&D obligations*		*Population*		*Total personal income*		*Total Federal taxes*[a]	
	Rank	*Percent*	*Rank*	*Percent*	*Rank*	*Percent*	*Rank*	*Percent*
Nebraska	43	.10	35	.72	33	.68	31	.56
Delaware	44	.09	47	.27	44	.30	27	.69
Montana	45	.05	44	.35	43	.30	45	.14
Arkansas	46	.05	32	.99	34	.67	36	.31
Vermont	47	.04	49	.21	49	.19	47	.11
Wyoming	48	.04	50	.16	51	.15	49	.07
South Dakota	49	.04	45	.33	45	.28	46	.11
North Dakota	50	.04	46	.31	48	.25	48	.11
Maine	51	.04	38	.49	39	.40	39	.24
U.S. territories and offices abroad	–	.38	–	[d]1.36	–	–	–	[e].37

a. Includes individual income and employment taxes, corporation, excise, estate, and gift taxes (minus refunds).
b. Includes persons stationed in the armed forces in each area.
c. Included in Maryland tax figures (not reported separately).
d. Puerto Rico.
e. Collections from and refunds to U.S. taxpayers in Puerto Rico, Canal Zone, and in foreign countries.

Source: National Science Foundation, *Federal Funds for Research, Development and Other Scientific Activities*, FY 1968, 1969, 1970, Volume XVIII, NSF 69-31, August, 1969, p. 56. U.S. Department of Commerce, *Current Population Reports*, Series P.-25, No. 414, "Population Estimates," January 28, 1969; U.S. Department of Commerce, Office of Business Economics, *Survey of Current Business*, Volume 49, November 8, 1969; U.S. Treasury Department, *Annual Report of the Secretary of the Treasury on the State of the Finances for the Fiscal Year Ended June 30, 1968* (Statistical Appendix).

In terms of dispersion, only one agency saw an increasing share of its R&D dollars located in that agency's "leading" ten states in receipt of R&D funds. The NASA share increased from 85 percent concentration in the top ten states in 1965 to 87 percent in 1968. The Department of Commerce concentrated 94 percent total R&D obligation in ten states in 1965 and 84 percent in 1968, while AEC concentrated 88 percent in ten states for both fiscal years studied. DOD allotted 79 percent in 1965 and 78 percent in 1968. The remaining agencies showed a more diffused pattern, with HEW focusing 70 percent of its obligation in ten states in 1965 and 69 percent in 1968; NSF 66 percent in 1965 and 62 percent in 1968; Agriculture 53 percent in 1965 and 52 percent in 1968; and Interior 53 percent in both years.[52]

The states receiving the major funds from DOD are shown in Figure 2-3. The dollar volume of DODs total R&D program was $7,695 million in 1968 and

$6,769 million in 1965. California was the first ranking state with 29 percent of DODs total obligation for research and development in 1965 and 28 percent in 1968. The Defense Department's obligation of $1.9 billion in California was the second largest amount directed to one state by any agency in 1965. (NASAs obligation in California was even greater.)

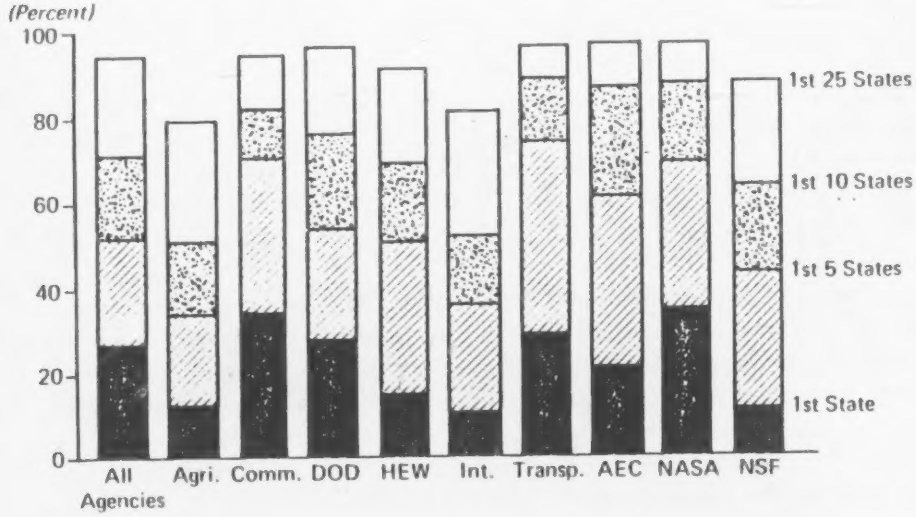


Figure 2-3. Each agency's R&D obligations to the 25 States leading in such support, FY 1968.

However, by 1968 the Department of Defense's obligation for research and development in California mounted to $2.17 billion, the largest amount directed to one state. NASAs obligation to California was the second largest amount. There were nine states in which DOD obligated more than $200 million in 1965 and eleven states in 1968.[53]

In 1965, NASA placed almost 45 percent of all its R&D obligations in California. By 1968, 36.5 percent of all NASA R&D obligations were placed in California. It is significant to note that in Figure 2-3, nine of the top leading states in NASA R&D support were coastal states.[54]

The bar graph in Figure 2-3 indicates that the R&D obligations of the Department of Commerce (totaling $61 million in 1965 and $83 million in 1968) were the most concentrated of any of the agencies. Commerce was the only agency for which the District of Columbia led the performance of R&D work in 1965. By 1968, the District of Columbia was replaced by Maryland as the leading state. Four of the ten subdivisions of the department reporting R&D

activities did over three-quarters of all R&D work. These were the National Bureau of Standards, the Weather Bureau, the Maritime Administration, and the Bureau of Public Works and Roads, all of which conducted their work in close proximity to the nation's capitol.[55]

Conclusion

Thus, the brief history of congressional interest in geographic distribution for federal R&D dollars is highlighted by a deep concern for "to whom the dollars should go," with both the "haves" and "have nots" keenly interested in participating. It should be noted that the great volume of official reports, hearings and documents has only been sampled to provide a little of the flavor of congressional interest in the dispersion of R&D funds. The data demonstrate that allocation of funds for research and development are not evenly distributed. Implicit in that distribution is the question of the social, economic, and political impact of uneven distribution.

The federal government must determine not only how much it should spend on the support of basic scientific research, but also how these expenditures should be divided among the various fields of science and among the various functional fields over which it has responsibility. In the sixties, new questions were added, such as to whom these research dollars should go, on the basis of what criteria, and in what amounts. When the Executive Branch has made its determinations on these points, it must then request of Congress the authority and money to implement the decisions. Subsequent chapters will deal with the geopolitical dynamics of the Executive-Legislative decision process.

Notes

1. Hatch Act (24 Stat. 440, Mar. 2, 1887); Bankhead-Jones Act (49 Stat. 436, June 29, 1935), and Morrell Act.
2. U.S., Congress, *Congressional Record,* Mar. 2, 1966, p. 4688.
3. Ralph E. Lapp, "Where The Brains Are," *Fortune,* March 1966, pp. 154-6.
4. U.S., Congress, *Congressional Record,* Mar. 15, 1965, p. 5011.
5. *Ibid.,* May 6, 1965, p. 9659.
6. Dr. Phillip H. Abelson, "Editorial," *Science,* July 2, 1965, p. 11.
7. Hearings before the Senate Subcommittee on Government Research of the Committee on Government Operations on S. Res. 110, *Distribution Among the States of Research and Development Funds Made Available by Government Agencies,* 90th Cong., 1st Sess. (1967), pp. 750-1.
8. *Ibid.,* p. 738.
9. For a thorough analysis of the impact of R&D on regional development, see U.S. National Academy of Sciences, *The Impact of Science and Technology on Regional Economic Development* (Washington, 1969).

10. Hearings, Senate Subcommittee on Government Research of the Committee on Government Operations, *op. cit.*, p. 543.

11. *Ibid.*

12. *Ibid.*

13. U.S. National Academy of Sciences, *Research and Development in Industry*, 1966, NSF 68-20, p. 1.

14. Hearings, Senate Subcommittee on Government Research of the Committee on Government Operations, *op. cit.*, p. 489.

15. H.R. 504, 88th Congress, 1st Session (1963).

16. Letter of Transmittal Report No. 4, "Geographic Distribution of Federal Research and Development Funds," Subcommittee on Finance, Research, and Development, Committee on Science and Astronautics, U.S. House of Representatives, 88th Cong., 2nd Session (1964).

17. Report on the Subcommittee on Science, Research, and Development of the Committee on Science and Astronautics, U.S. House of Representatives, *Obligation for Research and Development and R&D Plant by Geographic Divisions and States, by Selected Federal Agencies, Fiscal Years 1961-1964*, 88th Cong., 2nd Sess. (1964), p. 5.

18. *Ibid.*, pp. 13-14.

19. *Ibid.*

20. *Public Papers of the Presidents, Lyndon B. Johnson, 1965*, Vol. 2, Item 514, "Statement by the President to the Cabinet and Memorandum on Strengthening Academic Capability for Science," p. 996.

21. *Ibid.*

22. Hearings before the Senate Subcommittee on Government Research of the Committee on Government Operations on S. Res. 231, *Equitable Distribution of R&D Funds by Government Agencies*, 89th Cong., 2nd Sess., and 90th Cong., 1st Sess., in three parts (1966-1967).

23. U.S., Congress, *Congressional Record*, Mar. 2, 1966, p. 4688.

24. *Ibid.*, p. 4689.

25. *Ibid.*, p. 4690.

26. The following tables appear in U.S., Congress, *Congressional Record*, May 16, 1966, pp. 10708-10710; and in Hearings before the Senate Subcommittee on Government Research of the Committee on Government Operations, *Equitable Distribution of R&D Funds by Government Agencies*, 89th Cong., 2nd Sess., pt. 1, pp. 11-20.

27. U.S., Congress, *Congressional Record*, May 16, 1966, p. 10708.

28. Senate Subcommittee on Government Research of the Committee on Government Operations, *Equitable Distribution of R&D Funds by Government Agencies*, 89th Cong., 2nd Sess., p. 1, p. 71.

29. *Ibid.*, pt. 3 (from testimony of Dr. Donald F. Hornig, then Special Assistant to the President for Science and Technology), pp. 540-556.

30. *Hearings* before the Senate Subcommittee on Employment and Manpower of the Committee of Labor and Public Welfare, "Impact of Federal Research

and development Policies on Scientific and Technical Manpower," 89th Cong., 1st Sess. (1965), p. 3.

31. *Ibid.*, p. 4.

32. *Ibid.*, p. 29. This material was compiled by the National Science Foundation, National Register of Scientific and Technical Personnel, 1964.

33. U.S. National Aeronautics and Space Administration, *Annual Procurement Reports, Fiscal Years 1962, 1963, 1964,* Washington, D.C.

34. *Hearings* before the Senate Subcommittee on Employment and Manpower of the Committee of Labor and Public Welfare, "Impact of Federal Research and Development Policies on Scientific and Technical Manpower," prepared statement of Earl D. Hilburn, Deputy Associate Administrator, National Aeronautics and Space Administration, 89th Cong., 1st Sess. (1965), p. 766.

35. *Ibid.*, pp. 809-810.

36. *Ibid.*, p. 810.

37. Report and Recommendations of the Subcommittee on Employment, Manpower, and Poverty of the Committee of Labor and Public Welfare, "The Impact of Federal Research and Development Policies on Scientific and Technical Manpower," 89th Cong., 2nd Sess. (1966).

38. *Ibid.*, Letter of Transmittal.

39. Report and Recommendations of the Subcommittee on Employment, Manpower, and Poverty of the Committee of Labor and Public Welfare, p. 63.

40. *Ibid.*, pp. 61-62.

41. H.R. 7717, 89th Cong., 1st Session (1965).

42. U.S., Congress, *Congressional Record,* May 6, 1965, p. 9629.

43. *Ibid.*, p. 9659.

44. *Ibid.*, p. 12251.

45. *Ibid.*, p. 12252.

46. U.S. National Science Foundation, *Geographic Distribution of Federal Funds for R&D, Fiscal Year 1965,* NSF 67-8, Washington, D.C., 1967; and U.S. National Science Founcation, *Federal Funds for Research and Development and other Scientific Activities, Fiscal Years 1968, 1969, 1970,* NSF 69-31, Washington, D.C., 1969.

47. NSF, Federal Funds . . . , *Ibid.*, p. 51.

48. *Ibid.*, p. 60.

49. *Ibid.*, p. 54.

50. NSF, Geographic Distribution . . . , *op. cit.*, p. 51.

51. NSF, Federal Funds . . . , *op. cit.*, pp. 61-62.

52. *Ibid.*, pp. 61-66.

53. *Ibid.*, p. 229.

54. *Ibid.*, p. 230.

55. NSF, Geographic Distribution . . . , *op. cit.*, p. 33.

SCIENCE POLICY MAKING IN THE UNITED STATES

and the Batavia Accelerator

Anton G. Jachim

SOUTHERN ILLINOIS UNIVERSITY PRESS
Carbondale and Edwardsville

Feffer & Simons, Inc.
London and Amsterdam

5

Decisional Criteria Challenged

In the mid-60s the competition for R & D resources reached new levels. Some extrascientific groups began to focus on the process of R & D distribution itself. Although Elliot's select committee received more attention, it was Hubert Humphrey's subcommittee that began pushing for answers to recurrent questions. Humphrey's reasons for his inquiry followed an increasingly familiar premise: "We have been thrust into the 1960's on the crest of a wave of scientific knowledge. Out of this one startling fact becomes clear: our national well-being is becoming more dependent upon human resources than upon natural resources. This shift has accelerated. The problems and opportunities implicit in the trend are crucial to our welfare." But the specific focus of his attention became clear with his first witness, an R & D assistant secretary of Defense. Humphrey told of how the University of Minnesota regularly lost faculty members to research centers that had accumulated more federally subsidized resources, especially in applied work. The second- and third-level state universities in Minnesota suffered even more seriously. Humphrey made the thrust early.

> SENATOR HUMPHREY: Are you giving any further consideration or any consideration at all to a little better geographic distribution of the vast sums of money that are at your disposal for research and development?

> MR. RUBEL: I guess that depends on what you mean by better.
>
> SENATOR HUMPHREY: May I just start out by saying that the present situation is deplorable.
>
> MR. RUBEL: Well, as you know we have—are committed . . . , to making the selection of people to do work for the Defense Department on the basis of their capability to do it and our conviction that we are getting the best deal . . .
>
> We have no mechanism that I know of, Mr. Chairman, for introducing considerations other than that as a part of our decision making procedure and we haven't any way of saying that because a particular region of the country does not enjoy or does not participate very fully or actively now . . . , that, . . . we should direct our work toward that area at the expense of an area where the competence and excellence is to be found.
>
> So competence and excellence and performance are the bases for making these selections at the present time.

Excerpts of some of Humphrey's other comments to Rubel are more illustrative:

> SENATOR HUMPHREY: All I am asking is that, because of the huge appropriation that the Department of Defense has, it doesn't take the easiest route—which is what I think it is now doing . . .
>
> When they say this company has the competence, I come back to the procurement officer and say, "How did he get the competence; you gave him the contract."
>
> What do you plan on doing about it, Mr. Rubel?
>
> MR. RUBEL: My only comment, Mr. Chairman is that first you used the words selected areas.
>
> SENATOR HUMPHREY: Yes.
>
> MR. RUBEL: And I would like to emphasize again; there is no such thing as selected areas. We don't select by area.

When discussing the issue of concentration at a later date:

> SENATOR HUMPHREY: The Government of the United States is the No. 1 conspirator in this business, because it has the money, the Department of Defense most of it. We are going to get them up here. They need to be looked over in many ways.

At this point he was engaged in discussions with J. Herbert Holloman, assistant secretary of Commerce for Science and Technology. They discussed the problem of the small industrial firm trying to generate or use R & D. Humphrey's feeling on the possibility of such a firm getting an R & D contract was that it "has no more chance than a snowball in Hell." Holloman then discussed something that he had been proposing, a new program, that would not touch the current system of R & D contract letting, but which would develop the ability of small firms along this line based on the model of the early Agricultural Extension Service. Holloman worked vigorously for the new program, meeting with groups around the country to stimulate interest. In the Midwest he had meetings with the Economic Development Committee of the Midwest Governors' Conference. He received unanimous support from both the Development Committee and the Governors' Conference. There was practically no opposition, either from administrative units at the various levels of government or from scientific leadership, to a program such as Holloman's which did not affect current ones. It was approved and became an administrative responsibility of the Department of Commerce in 1965. The States Technical Services Act (PL 89182) was a program that consisted essentially of federal matching grants to states that set up a science advisory apparatus for the purpose of providing scientific and technical advice to small industrial firms that could not themselves afford an R & D unit. Consultants were typically secured from the state university or other university or research institutes to advise these small firms of technological innovations in their field, or in other fields that might be of use to them.

A later exchange between Humphrey and Jerome Weisner, presidential science advisor and director of OST took place.

> SENATOR HUMPHREY: Do you think—well, are you concerned, first about the distribution of R & D contracts and, if you are concerned about it, if you have any specific concerns, what do you think can or should be done to modify or improve the situation?
>
> DR. WEISNER: I would not say that I am concerned about the

> distribution of R & D contracts because we award R & D contracts to get specific jobs done in Government. We should have them done where they can be done best.[1]

Weisner went on to suggest, as had Holloman, that new programs were needed, development programs, to build up weak research-universities and regions, and supported Holloman's suggestion for small firms.

Hubert Humphrey was the harbinger of an intensifying conflict. Premises that are clear throughout his questioning had in earlier interactions been implicit—perhaps even ignored. They now became explicit. In the Senate he had said in 1962, "With all due respect to that great institution, the Massachusetts Institute of Technology, they are just not that good. And the Institute of Technology at the University of Minnesota is just not that relatively unable to do the job."[2] Moreover, he sometimes hinted at the emerging conflict in priorities that was to haunt the scientific community on basic and applied research. He discussed the impact of low priority on applied work in universities. "They [researchers] liked to lock themselves up in cozy rooms, some of them not so cozy either, and just write and write. Meanwhile, there are students out there reading those old obsolete, and outdated books, while the scholar is still writing and researching, but never getting the research out to the students." These "government researchers" then talk to each other, says Humphrey, participating in "intellectual incest."[3]

On August 22, 1963, George P. Miller of California, chairman of the House Committee on Science and Astronautics, appointed a new subcommittee on Science, Research and Development which came to be headed by Emilio Daddario. In late 1963, the special Daddario Committee hearings began which were to form the basis for another comprehensive study of the entire spectrum of government-science relationships. The list of expert witnesses was a familiar one and included: Jerome Weisner (Office of Science and Technology-President's Science Advisory Committee); Edward Teller (the Livermore Laboratory near the Lawrence Radiation Laboratory); Paul Gross (past president of the American Association for the Advancement of Science); Frederick Seitz (National Academy of Sciences); Werner von Braun (NASA); Leland J. Haworth (NSF), among others. They responded to the committee in the usual style for such meetings but participated in a "Government and Science Seminar" as well. The subcommittee was new and it was part of a standing committee which in many ways shared the interests of the witnesses. Since it was new its members were often deferential in their interactions with witnesses, but not always. For instance, during a discussion that took place when the seminar was in session and the topic of research in the universities came up, one member charged the guests:

> You gentlemen could solve the problems in your areas if you could do it your way, but we are dealing with something more complex than that . . .
>
> I am telling you gentlemen that you are responsible for the situation which exists in education today, because you have been selfish from your point of view in trying to solve your problems without regard to the problems you are creating for somebody else.

It seems fair to say that the few challenges made during the Daddario hearings seemed to be anticipated by witnesses. Thus they made some suggestions for change of sorts but there again was remarkable agreement on the nature of this change. Paul Gross (AAAS) can be pinpointed as the spokesman here: "The change of policy would recognize that there is now an overemphasis on research at the expense of teaching, and an overemphasis upon short-run research goals at the expense of broadened research competence." And further: "I propose, therefore, that the Government's total objective in supporting science would be better served if immediate research competence were not the only criterion for the distribution of funds and if some grants for research were to be made either on a formula basis or by selection of especially promising institutions in parts of the country in which they do not now exist."[4] Although Gross was perhaps not as definite as the others on this issue of new programs—he did, however, make the strongest statement on the criterion of competence—the official consensus of the witnesses began to emerge. Redistribution yes, but with separate development programs administered by each agency. By all means, the regular program and the development program must be kept separate and the funding for the new programs should not affect, should be an addition to, the regular program. But private discussions at the Cosmos Club and elsewhere may have shown something different. One scientific leader declared to a reporter, regarding possible regional redistribution, "It is foolish to take a regional view of this. We are one country, with an excellent transportation system. If the midwest is lacking in first class research facilities this is more than compensated for by the strength of the east and west coasts."[5] By implication a related concern was revealed frequently, that even maintaining the status quo for programs that tended to support centers of excellence was not enough since research becomes ever more expensive. Teller's comments are illustrative: "I think that the few percent of the national income which we spend on research can be increased and should be increased. . . . I am convinced that our whole future welfare is involved in precisely these efforts, and I am unequivocally behind spending more money, with the only restriction that I am fully aware of the fact that to spend more money is not enough—you also have to know how to spend it." But institutional integrity is further protected when it comes to planning, when Weisner notes "it is not possible always to plan what is the best way of doing something, and allowing a lot of freedom for experimental interactions, and for things to evolve . . ." and Teller, "To plan it [research] in detail is to emasculate it."

How new development programs with new budgets could be added to existing expenditures, and the expenditures level for regular R & D programs be increased as research became more expensive, troubled the congressional inquirers, since one of their major concerns was to gain some control over the growth curve (in expenditures) in research. Moreover, they wanted expert advice on how this control might be arrived at—what kind of planning would be possible. To be told that planning in R & D is not to be taken too seriously was disillusioning at best.

In early 1964 the Daddario Committee contracted with the National Academy of Sciences to do studies on planning for R & D, while in May and June of 1964 hearings continued. (This was the first contract entered into by the NAS and a congressional committee, the past contracts being largely with federal executive units.) Presumably the subcommittee members had done some homework and this showed up in their lines of inquiry that covered the distribution of R & D resources. The first witness representing DOD noted, "In summary, we recognize that defense R & D is concentrated geographically, and that this concentration will continue for the foreseeable future," and "we tend to follow competence where we find it and therefore consider that greater uniformity in the geographical distribution of our R & D contracts and grants can only be achieved to the extent that more uniform competence is developed." He listed the elements of the evaluation of competence as: the experience of a contractor with similar work, management capacity, technical talent, and the hardware that he already has on hand that may obviate the need to procure it anew. Moreover, he continued, some university administrators would like to see a more even distribution of funds, but they realize that these funds must go where the "talent is located." In response to a query on what DOD intended to do about concentration in R & D, "I am sure the Department of Defense would probably like to see a more uniform distribution, but our business is not that kind of business." Moreover, he stipulated that DOD does try to spread funds around to small firms in procurement operations (not R & D) and so the problem is not so bad. Some were not convinced.

> MR. ROUSH: Do you see any danger of what science calls "inbreeding" and concentration of research and development in areas?
>
> GENERAL ELY: I would say that the values of inbreeding probably overcome the dangers.

The NASA spokesman discussed the core R & D program there, ". . . in fulfilling its mission and carrying out its R & D program, it is essential that NASA locate and fully utilize the best technical competence available wherever it may be." In response to the query on whether NASA tried to support research in small as well as large universities the response was indirect. The process was described as having the universities "evolve" proposals that are evaluated by advisory committees of experts first on quality, second on the congruency of the proposed research with NASA's goal.

At this point we will simply list some characteristic responses to these recurring questions. According to Leland Haworth (NSF),

> . . . to achieve high quality results requires going where the best capability exists. That capability is now quite concentrated geographically. Hence the maintenance of high quality results will result in concentration unless and until we build up a broader geographic base of capability. *And this will require additional expenditures* [emphasis added]. . . . Pushing back the frontiers of knowledge is no easy task, and in many fields today basic research requires a large investment in equipment and fairly sizable research groups in order to make real progress. Over the years, largely because of the initiative of individual scientists, there have sprung up in various parts of the country centers for research in each of the major disciplines. By and large, these centers of research have had a tendency to cluster on a given campus . . .

He discussed the NSF's stand:

> In walking this very narrow line between "undue concentration" on the one hand, and unacceptably low standards, on the other, the Foundation has been assiduous in its efforts to seek out and support individuals and groups in organizations throughout the United States who can carry out research . . . in a manner which is consistent with advancing the scientific potential of the Nation.

Further:

> I think our regular programs must continue to use quality as the primary criterion. For one thing we get better results that way. For another thing we must not allow the places that are already good to deteriorate. That would be disastrous.

And regarding the persistence of peer-group selection in regular programs he offered no alternative:

> In any given situation the best appraisal is by the man's peers, so to speak, in the laboratory, in a department of the university or of other laboratories or universities. To have a high reputation among scientists in the fields in which an individual works is, to me the best measure of all. . . . The physics department at Columbia University has a high reputation among physicists. I think this is the best measure you could possibly get. I don't really know any other good measure.

The spokesman for HEW-National Institutes of Health provided data on the distribution of its R & D funds and added "This distribution was not planned but reflects the accumulation of tens of thousands of judgments with respect to the *scientific merit* and *program relevance* of research proposals submitted by institutions since World War II" (emphasis added). In terms of developing new competence he urged a new program providing for institutional grants (as opposed to peer-group selected project grants) but new competence will "not . . . happen overnight." The AEC spokesman noted, "The research dollars tend to flow to those places where the best researchers are located. By and large, I believe this is as it should be. The very nature of research dictates that the best people be supported wherever they may be."[6]

In their turn, further witnesses such as Lee DuBridge (then president of the California Institute of Technology), Donald F. Hornig (OST-PSAC), and George B. Kistiakowsky (COSPUP) informed the committee that competence to do quality research is paramount, that the peer-evaluation system is the best, that researchers do not follow the dollar, but follow stimulating research leaders and tend to cluster around them, and the R & D dollars tend to go to these groups of people, not to particular universities or laboratories. What was to be done, was to leave the regular allocational system alone and provide money for additional development programs.

The NASA Authorization Act of 1966 was passed on June 28, 1965, containing an unusual amendment. The amendment's author, Walter F. Mondale of Minnesota, was later to call it in the summer of 1967 "one of the least used paragraphs in the U.S. statute books."[7] It stipulated: "It is the sense of Congress that it is in the national interest that consideration be given to geographic distribution of Federal research funds whenever possible, and that the National Aeronautics and Space Administration should explore ways and means of distributing its research and development funds whenever feasible."[8] During the period of deliberation on this authorization, Weston E. Vivian of Michigan attempted to insert a section into the authorization bill that would require NASA to award 50 percent of the Voyager Phase I research contracts to companies operating in states then receiving few NASA R & D funds. He was informed by James Webb, the NASA administrator, that there was no way to do this. This drew a response from Gaylord Nelson of Wisconsin, "As you are well aware, the position of those placing contracts asserts that their policy is to put the money where the competence is. I think there are some fallacies in that position."

The Clark hearings of 1965 took a somewhat different flavor. The usual list of scientific leaders appeared, but the witness list also included spokesmen from "have-not" areas and organizations. After Donald F. Hornig (Office of Science and Technology) attempted to show that California universities, the major ones being the University of California, Stanford, and the California Institute of Technology, received large doses of R & D funds because of the competence found in them, Gaylord Nelson of Wisconsin responded: "One of the things that concerns the Congress is that if that theory is correct and competence is the sole test then the disproportion simply grows greater and greater and there is no way to reverse it." Leland Haworth (NSF) explained the regular project grant system. "The decision is not made with respect to the institution. It is made with respect to the man and the work he proposes to do. The only way the institution comes into it in any sense is that one must take into account whether the equipment is available that needs to be used. It is only in that sense that it is done."

It also appears that questioners were beginning to understand, correctly or incorrectly, some of the aspects of the allocation system. Nelson had earlier remarked to Haworth: "The opinion has been expressed from time to time that there is a kind of inbred relationship between the people who are determinative in making research and development grants and the recipients. It would be perfectly natural for a grantor to have a preference for a particular grantee with whom he has had a successful relationship." Haworth thought this would not be the case with NSF. Responding to a similar remark he stood firm behind the "two programs" approach, the regular one based on project grants to individuals and groups, and the development programs of various kinds. Hornig had held the same line. It was, however, the allocation system of the regular programs that was being challenged.

But others who were supposed to represent the "have-nots" had some points to make. In discussing the present allocational system Fred H. Harrington, president of the University of Wisconsin, thought that it "results in a tendency to lose faith in the democratic process." Representative Robert B. Duncan, paraphrasing the Speaker of the House, said, "Whenever one group gets too strong and begins to oppress some other group or to treat some other group unfairly, then it is the function of government to step in and balance things," and the clincher, ". . . here is a potential weapon [redistribution of R & D] against poverty." The core of the implicit conflict of values begins to stand out, that is, meritocracy vs. equality—the wider distribution of educational and economic opportunity in this case. Weston E. Vivian of Michigan chastised agency contract negotiators for being afraid to take risks by going back again and again to large firms that have experience with R & D contracts. Robert C. Edwards, president of Clemson University, was blunt: "Returning to the basic question before this committee, we submit that in our judgement it would be appropriate for the Congress to consider the possibility of distributing at least some of the research and development funds on a population basis." And near the end of the hearings Nelson repeated his sentiments:

> I am concerned about the rigidity of attitude and apparent lack of concern about the effect of . . . expenditures upon the concentration of scientists, engineers, and technical people, and the multiplier effect of this critical mass of scientists and technicians in certain areas.

> If all R & D grants are to be placed strictly where the greatest competence is . . . under that theory all the competence would become pretty well concentrated creating greater and greater imbalance in the Nation.[9]

Along this line the final subcommittee report went on to say: "It [the subcommittee] was disappointed by the paucity of material presented by the agencies asked to comment on this. The meagerness of the agency responses tended to confirm the subcommittee's view that the agencies are little concerned about the long-term effects of the programs they administer."[10] Moreover, the final report repeated over and over again, the big corporations, large, prestigious universities, and sometimes the geographic areas in which they tended to cluster, were far too well-off vis-à-vis other organizations and areas.

Lyndon Johnson was, by all accounts, disturbed by the amount of time and attention he had had to give to the Midwest Universities Research Association and Stanford Linear Accelerator Center struggle in high energy physics. He was also aware of these interactions between scientific leadership and congressmen. Weisner made it clear that there was trouble ahead for the scientific community that was only delicately held together, if proponents of per capita or geographic distribution of R & D resources became too strong. Probably in order to avoid that and to get the jump on Congress, on September 13, 1965, Johnson, after consultations with Weisner and others, issued a memorandum to heads of departments and agencies. It was entitled "Strengthening Academic Capabilities for Science Throughout the Country." It asked for some new efforts on the part of agencies supporting R & D universities. It stipulated in part: "Research supported to further agency missions should be administered not only with a view to producing specific results, but also with a view to strengthening academic institutions and increasing the number of institutions capable of performing research of high quality." Further:

> To the fullest extent compatible with their primary interest in specific fields of science, their basic statutes, and their needs for research results in high quality, all Federal agencies should act so as to:
>
> a. Encourage the maintenance of outstanding quality in science and science education in those universities where it exists;
> b. Provide research funds to academic institutions under conditions

> affording them the opportunity to improve and extend their programs for research and science education and to develop the potentialities for high quality research of groups and individuals, including capable younger faculty members.
>
> c. Contribute to the improvement of potentially strong universities through measures such as:
>   —Giving consideration, where research capability of comparable quality exists, to awarding grants and contracts to institutions not now heavily engaged in Federal research programs;
>   —Assisting such institutions or parts of institutions in strengthening themselves while performing research relevant to agency missions, by such means as establishing university administered programs in specialized areas relevant to the missions of the agencies.
>
> Departments and agencies should carefully assess the degree to which and the manner in which their existing programs support this policy, and, when indicated, should use a larger proportion of their research funds in accordance with the intent of the policy. The means for attaining this objective will be determined by each department and agency. In carrying out the policy, the various Federal agencies supporting research at a university should act in concert to a greater degree in making decisions, so as to make the university better able to meet the collective needs of the agencies and to make the Federal support most effective in strengthening the university.[11]

The salient points of the memorandum were that although some kind of redistribution was called for, there was no clear indication of how it was to be done except as "determined by each department and agency." Agencies, it was suggested, might take redistribution into account, all other things being equal, where research capability of comparable quality exists. Moreover, the memorandum was restricted to universities which would not receive anything like a bulk of the R & D funds. Some kind of block grant approach was suggested in the call for more "university administered" programs. Thus the plea for the two differentiated programs emerged anew, with implementation of the new university program to be decentralized in part to university adminstrators.

With the agencies providing their own remedies the two-programs approach continued to be favored. The NSF spokesmen, for instance, would often point to its new Science Development Program when asked what the NSF was doing. And in hearings before the Reuss Subcommittee on Research and Technical Programs of the House Committee on Government Operations, Henry Riecken, NSF associate director described the NSF's purposes: "We think of a center of excellence as an institution that is as much as possible uniformly excellent. A major part of our effort in the science development program is to raise the level of excellence everywhere in the institution." The criterion of competence (quality) thus had been incorporated into the NSF's development program. The subcommittee members clearly did not see this as the development program they had in mind; certain members would have liked a redistribution of some of the regular R & D funds as also might be implied from the presidential directive. In the subcommittee's report it was observed that the four initial recipients of development grants, Washington University-St. Louis, Western Reserve, Case Institute of Technology, and the University of Oregon, could not be thought of as "research deprived; three of the four, in fact, rank with the top 40 institutions in terms of Federal research money received" and the committee concluded, "emphasis should be placed on improving developing institutions rather than lagging departments within already important research institutions."[12] The NSF also began an Institutional Grants Program (not grants to specific researchers or groups for project proposals but grants to universities). This program maintained the quality criterion as well. The formula by which these grants were allotted was based largely on the number of grants the researchers at the university have been able to attract through the regular project grant program. Levels of conflict seem to have been reduced somewhat with the two-programs approach, at least for the time being.

Every once in a while views exchanged within the scientific community told a somewhat different story from what took place officially. For instance, when discussing block grant programs during a seminar in 1966, Maurice J. Zucrow declared that he did not like the idea. He used the well-recognized chemist, Roger Revelle, as an example, and tried to show what might occur at a university with which Revelle was associated that would receive a block grant, these funds to be distributed by the university administration. The university administrator then feels he should give the money to his mediocre departments where there are mediocre researchers "instead of making Roger Revelle's job easier, to improve his excellence." A little earlier in the discussion Lee DuBridge (then president of Caltech) offered the observation that it is foolish to think that all research organizations can be excellent, this will never be because "there aren't enough good people to go around."[13]

In early January 1966, the Daddario committee issued another report based on a comprehensive review of one R & D administrative unit, the National Science Foundation. The theme of the report was that the NSF must cease to be a service agency for researchers and should exert leadership rather than follow its past practice, as the committee saw it, of "once granted its annual budget, the NSF has to a large extent followed a practice of waiting for talented outsiders to suggest appropriate projects on which to spend it."[14] The NSF was told not only to direct the focus of research but to become more involved in applied programs.

In the spring of 1966, the Department of Defense announced a new $18 million program (later named Project Themis and raised to $20 million) to meet three objectives:

1) Build up new graduate research centers where they are now sparse.
2) Amalgamate some of the existing centers into small compact units.
3) Offer DOD research facilities to more schools.

Robert McNamara said, "We plan to take the initiative and systematically visit universities that have not as yet had the opportunity to bid for defense work. In the course of these visits, we hope to help these institutions determine their capabilities and inform them on how to prepare proposals." A *Physics Today* editorial disclosed that the NSF was soon to announce similar programs and that the AEC would "expand our national laboratories and contract research programs."[15] But the underlying theme of this editorial was that researchers who had reached their present position of excellence by merit would suffer. Budgets for research agencies could not possibly expand to include all of these programs and keep the present programs from deteriorating. At the same time articles were run in relevant journals that told of the shift in emphasis to applied programs of all kinds.

In July of 1966, a series of hearings was held by another new oversight committee, the Harris Subcommittee on Government Research of the Senate Committee on Government Operations (chaired by Fred R. Harris of Oklahoma). The vehicle for setting up the hearings was Senate Resolution 231, sponsored primarily by Senator Carl T. Curtis of Nebraska, directing the NSF to formulate and transmit recommendations as soon as possible on what changes should be made in law and administration to provide for a more equitable distribution of R & D funds. The witness list included a number of research administrators for "have-not" areas and organizations who told of their woes in trying to attract R & D resources. Robert Kamm, president of Oklahoma State University, provided an example: "One member of our faculty has been unable to obtain federal support for 10 successive proposals since his coming to our campus, but he previously obtained support for all three proposals he made while he was a member of the staff of a well-known eastern university." These examples were put forward in connection with the usual underlying premise. Senator James B. Pearson of Kansas provides this premise: "In other words, it is literally the case that the awarding of large research grants can make or break a college or university and can also have a decisive influence on the character and vitality of the surrounding local community." Further, "It [concentration] has occurred because there has been little or no conscious planning in this area." By now this premise was rigidly held by opponents of the allocational system with the not unexpected call for "planning" emerging. Of course, the kind of planning wanted was any kind that would result in a redistribution, while planning as a symbol has its own special appeal, with connotations of rationality, efficiency, and economy. That these things might actually be best furthered by concentration was an easily forgotten point on the part of the critics.

Since S.R. 231 singled out the National Science Foundation, spokesman Director Leland Haworth and Director Donald F. Hornig of the Office of Science and Technology, which was supposed to be the "coordinator" of all science programs, bore the brunt of the attack. Haworth again repeated the two-programs solution, with the warning that the new development programs required much greater expenditures on all R & D. In turn, Harris responded:

> SENATOR HARRIS: . . . in essence you are asking for funds in addition to your regular budget to distribute around the country on a geographic basis. Is that a fair statement?
>
> DR. HAWORTH: Well, it is not a complete statement, I think.
>
> SENATOR HARRIS: Well, now what is Defense doing to get more equitable distribution? They say, "If you give us $20 million in addition to our regular operating budget, we will be sure to distribute it around the country."

When Hornig followed essentially the same line as Haworth, these exchanges took place:

> SENATOR HARRIS: . . . it seems to me that you and Dr. Haworth, from whom we heard earlier, have been a little bit patronizing and condescending in your treatment of this committee by coming here and saying some things that are fairly obvious. . . . Now both of you have about the same kind of statement. You have spent half your time saying that these things are not as important as I think, but that you are doing a whole lot about it anyway. . . . Now, I think if we would recognize this as of great concern . . . and quit talking down to members of Congress, as you have done . . . then we would come a lot nearer getting down to some cases.

Later Hornig tried to show that the present system was not so bad:

> DR. HORNIG: . . . I don't think one can find any evidence of this rigidifying of existing structures. The really rapid rates of growth are, . . . in the Mountain States and in the South.
>
> SENATOR HARRIS: That is an amazing statement. There are a lot of people in the country who disagree with you. Either they are badly informed or you are mistaken . . . it is a self-feeding cycle. That seems obvious.

In the course of the discussion Hornig stated that the OST and others such as the NSF were gathering a good deal of statistical data on which they might proceed and drew this response:

> SENATOR HARRIS: I agree with that. I agree that we must have the facts, but I think what you do with those facts very much depends on your viewpoint. If you are part of the established order, and your general feeling is that things are not so bad as our critics think they are, and that things are really in pretty good shape—concerning distribution—then I think you will tend to do nothing at all. If on the other hand, you are outside of the established order, and you don't think things are going too well, it seems to me that you would tend to look for ways in which things might be done to improve the situation.

> I think, except for reporting, you really haven't done anything in the year since the President gave you that special responsibility [the White House memorandum of September 13, 1965] and has given the agencies and departments to understand that some changes must be made in distribution.[18]

The discussion so far has been an attempt to show what was going on, what were the issues, in science policy making in general. The same issues were being raised in more specific areas. On that point we return to high energy physics.

5. DECISIONAL CRITERIA CHALLENGED

1. Statements from the hearings are found in U. S. Congress, Senate, *The Role and Effect of Technology in the Nation's Economy*, vol. 1, pp. 1–116; vol. 2, pp. 117–230; vol. 3, pp. 231–375.

2. Quoted in Daniel S. Greenberg, *The Politics of Pure Science*, p. 248.

3. *The Role and Effect of Technology in the Nation's Economy*, vol. 1, pp. 1–116; vol. 2, pp. 117–230; vol. 3, pp. 231–375.

4. Statements from the hearings are found in U. S. Congress, House, *Government and Science*, pp. 269 and 145.

5. Daniel S. Greenberg, "News and Comment," *Science*, 8 November 1963, p. 650.

6. *Government and Science*, 1963, pp. 111–34, 57, 114; 1964, pp. 2–33, 16, 34, 65–106, 108, 144.

7. U. S. Congress, Senate, *Equitable Distribution of R & D Funds by Government Agencies*, p. 329. vol. 1, pp. 1–301; vol. 2, pp. 302–538; vol. 3, pp. 539–762.

8. Public Law 89–53, 89th Cong. 1st sess. 1965, H. Rept. 7717.

9. Statements from the hearings are found in U. S. Congress, Senate, *Impact of Federal Research and Development Policies on Scientific and Technical Manpower*, pp. 109, 59, 662, 661, 76, 100, 115–16, 226, 83.

10. *Report and Recommendations*, p. 37.

11. "Strengthening Academic Capability for Science Throughout the Country," The White House, 13 September 1965, 2 pp. A similar memorandum was issued to cabinet members on September 14, 1965, entitled "Statement of the President to the Cabinet on Strengthening the Academic Capability for Science Throughout the Nation," 2 pp.

12. "R & D Boom: House Report Sees Harm to Higher Education," *Science*, 22 October 1965, p. 466.

13. U. S. Congress, House, *Science and Public Policy, Panel on Science and Technology*, Seventh Meeting Proceedings before the Subcommittee on Science, Research and Development of House Committee on Science and Astronautics, 89th Cong., 2d sess., 1966, pp. 155, 154.

14. Quoted in Daniel S. Greenberg, "News and Comment," *Science*, 14 January 1966, p. 178.

15. "Physics and Government," *Physics Today*, June 1966, p. 75.

16. Statements from the hearings are found in *Equitable Distribution of R & D Funds by Government Agencies*, 89th Cong., 2d sess., 1966, pp. 254, 218, 53, 77, 107, 110.

TOWARD THE ENDLESS FRONTIER

History of the Committee on Science and Technology, 1959–79

By KEN HECHLER

COMMITTEE PRINT

U.S. HOUSE OF REPRESENTATIVES

U.S. GOVERNMENT PRINTING OFFICE
35-120 O WASHINGTON : 1980

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, D.C. 20402

GEOGRAPHIC DISTRIBUTION OF NSF FUNDS

During the 1974 debate, Flowers renewed an argument he had frequently made both in committee and on the floor:

> Certain institutions in certain states continue to receive the lion's share of the National Science Foundation's grants. It seems to me that NSF has a definite obligation to see that these awards are distributed more equitably across our Nation. I am not persuaded that quality research people and institutions deserving of grants and contracts are located only in those few areas now receiving special attention.

The efforts which Flowers made, as with prior efforts directed at wider geographic distribution of NASA expenditures, furnished the subject of much serious debate and conferences within NSF. Aside from some token exceptions, however, the net results continued to show that those institutions and geographical areas which were already benefiting continued to do so. It was difficult to argue with quality, experience, and availability of good proposal-writing personnel who knew how to hone the keys to the strongbox. There was a great deal of agonizing, along with lip service given by NSF officials to support the general principle that NSF funds generated greater ability in certain areas to obtain more funds. But in the case of larger contracts, NSF did little better than NASA in effecting a wider distribution geographically. It was always easy within the privacy of the executive branch to defend the "national interest" against the "parochial" assaults of "pork barrel politicians."

GEOGRAPHIC DISTRIBUTION IN 1975

The subcommittee also tackled an issue which, year after year, was raised by Flowers—the overconcentration of NSF research funds in research-rich States like California and Massachusetts, with consistently low results for many States in the Deep South:

> Maybe I am batting my head against a stone.wall. I have been doing this thing for 3 or 4 years, and I keep thinking maybe I might be getting somebody's attention. I want to be good-natured about this thing, but I want to see some results too.

In 1975, Symington insisted that the subcommittee recommend strong language in the report emphasizing the need for wider geographic distribution, in accordance with the terms of the amended NSF act of 1968, admonishing NSF "to avoid undue concentration." The committee views in the authorization bill stressed this point, and faulted the NSF for failing to take the legislative language into account. But despite reiteration of the firm congressional intent, there was very little affirmative action demonstrated by NSF in carrying out the intent of Congress in this respect.

FLOWERS RENEWS FIGHT FOR WIDER DISTRIBUTION

In 1977, Flowers renewed the long effort he had been making to attempt to redress the balance in geographic concentration of research grants and contracts. Lightheartedly, Flowers told his colleagues:

> This is the annual exercise in futility that I go through with NSF. For the benefit of some of the new Members, we have heretofore in years gone by been furnished with a geographical breakdown of NSF branches. It shows exactly in what parts of this great land of ours very nice grants and contracts are parceled out. Lo and behold, this year they did not even have the audacity to furnish us with the information. I have to conclude, friends and neighbors, that it is worse than it was before.* * * It will be easier to defend these grants if maybe one or two of them went to your district.

The solution which Flowers proposed was to make a slight change in the law passed in 1968, which read:

> It shall be one of the objectives of the Foundation to strengthen research and education in the sciences, including independent research by individuals, throughout the United States, and to avoid undue concentration of such research and education.

Flowers suggested that in the first line the phrase "one of the objectives" be changed to "the objective." He explained:

> Now they have generally been looking beyond that and saying generally oh, yes, that is one of the objectives but you know we have these other objectives, too, and that just has to take a back seat. I am merely trying to upgrade that and let them give a little bit better attention to it.

In a horrified tone, Ottinger protested:

> Obviously, these ought to be given out first on the basis of merit. I think there ought to be an attempt to concentrate. You are not trying to force them to put geography ahead of all other considerations by this, are you?

Quickly and decisively, Flowers responded: "Absolutely not."

At Ottinger's suggestion, Flowers weakened his amendment by making it read "an objective" rather than "the objective" and it sailed through without opposition. The effect on NSF remained nebulous. About the only concrete result was that, working through Flowers, NSF did manage to pass along advice to Alabama educational institutions on how to sharpen up their proposals and applications for grants. Whether or not by coincidence, there appeared to be some increase in Alabama grants after Flowers had made his many protests. The whole exercise, however, did not result in any substantial changes toward a wider distribution of NSF funds nationwide.

The State Of Academic Science

The Universities
in the Nation's
Research Effort

Bruce L. R. Smith and Joseph J. Karlesky

Change Magazine Press
New York

Institutional Concentration of Federal Funds for Academic Research

Most of the federal funds for academic research are concentrated in universities as opposed to other kinds of educational institutions, and within that group the major research universities receive the largest share. Each year, 100 universities account for more than 85 percent of all academic research expenditures (see Table 3). This heavy concentration in a relatively small number of universities can be attributed to

Table 3
Proportional Concentration of R&D Funds Among Universities, Selected FY 1963-1975

Year	Top 10	Top 20	Top 50	Top 100
1963	32.8%	49.5%	71.9%	89.5%
1967	29.1	44.9	69.6	88.1
1968	27.7	43.1	67.4	86.7
1969	27.4	43.2	67.3	86.1
1970	27.8	43.3	67.6	86.1
1971	27.1	42.7	67.2	85.8
1972	27.4	42.8	67.0	85.3
1973	27.1	42.4	67.1	85.1
1974	27.3*	42.8	67.2	85.7
1975	25.8*	41.4	66.2	85.0

* Includes funding for Draper Laboratories. Funding for the Draper Laboratories—because of a change in its charter to an independent nonprofit corporation—was reported separately from MIT funding for the first time in 1974 and 1975. Another change in reporting procedures should also be noted. At multicampus systems, the reporting unit has sometimes changed. In some cases the research statistics of the entire system have for a time been attributed to a single campus. Thus changes in reporting procedures can alter an institution's relative standing. Such reporting changes can complicate time series analysis of research statistics, and reported data should therefore be viewed with care.

Source: National Science Foundation. *Federal Support to Universities, Colleges, and Selected Nonprofit Institutions.* Annual Publications (1975 data in press).

Table 4
Universities Gaining as Recipients of Federal Reseach Funds, 1964 and 1974

Institutions	Ranking 1964	1974
University of California, San Diego	37	5
Washington University	28	18
University of California, San Francisco	36	19
University of Alabama, Birmingham	80	40
University of Hawaii	71	41
City University of New York—Mt. Sinai School of Medicine	*	44
Colorado State University	68	45
Boston University	83	50

*Not in top 100 in 1964.

Source: General Accounting Office. *Geographical Distribution of Federal Science Funds to Colleges and Universities.* Washington, D.C.: GAO Report B-117219, 1976, p. 11.

Table 5

R&D Expenditures of 20 Largest Universities as a Percent of Total R&D Expenditures by Field, With Three Definitions of "the 20 largest," 1970 and 1974

	Engineering		Physical Sciences		Mathematics		Life Sciences		Social Sciences		Environmental Sciences		Psychology	
	'70	'74	'70	'74	'70	'74	'70	'74	'70	'74	'70	'74	'70	'74
Largest 20 universities in '70; same 20 in '74	41%	45%	45%	41%	39%	36%	31%	33%	46%	38%	30%	45%	36%	37%
Largest 20 in each field in '70; same 20 in '74	46	52	50	41	49	46	32	35	52	35	38	32	35	37
Largest 20 in each field in '70; largest 20 in '74	46	44	50	47	49	56	32	37	46	43	38	63	35	51

Source: National Science Foundation. *Expenditures for Scientific and Engineering Activities at Universities and Colleges.* Annual Publications.

several factors: Four-year colleges, and to a greater extent community colleges, are primarily concerned with teaching. Universities, on the other hand, consider research an essential function, and they recruit faculty committed to research. NSF and other government agencies and private foundations, despite the criticisms of the peer review system and proposals for geographic distribution formulas, still base their grant decisions on merit and the desire to use their funds as productively as possible. This has led them to the universities where the best scientists and research facilities are to be found.

In the past decade there has been a slight decline in the concentration of federal research funds in the universities. Further, there have been substantial shifts, as some institutions have dramatically increased their research funds while others have fallen back. Early in the 1960s, as Table 3 shows, a full third of all federal funds allocated to academic research went to only 10 universities. Concentration at the top is still the case, but the share has gradually declined. The top 20 universities, which received half the total a dozen years ago, now get about 40 percent. The top 100, which had 90 percent of the total, now get 85 percent. Some of the universities in the top 100 recipients that have made dramatic gains in funds are listed in Table 4.

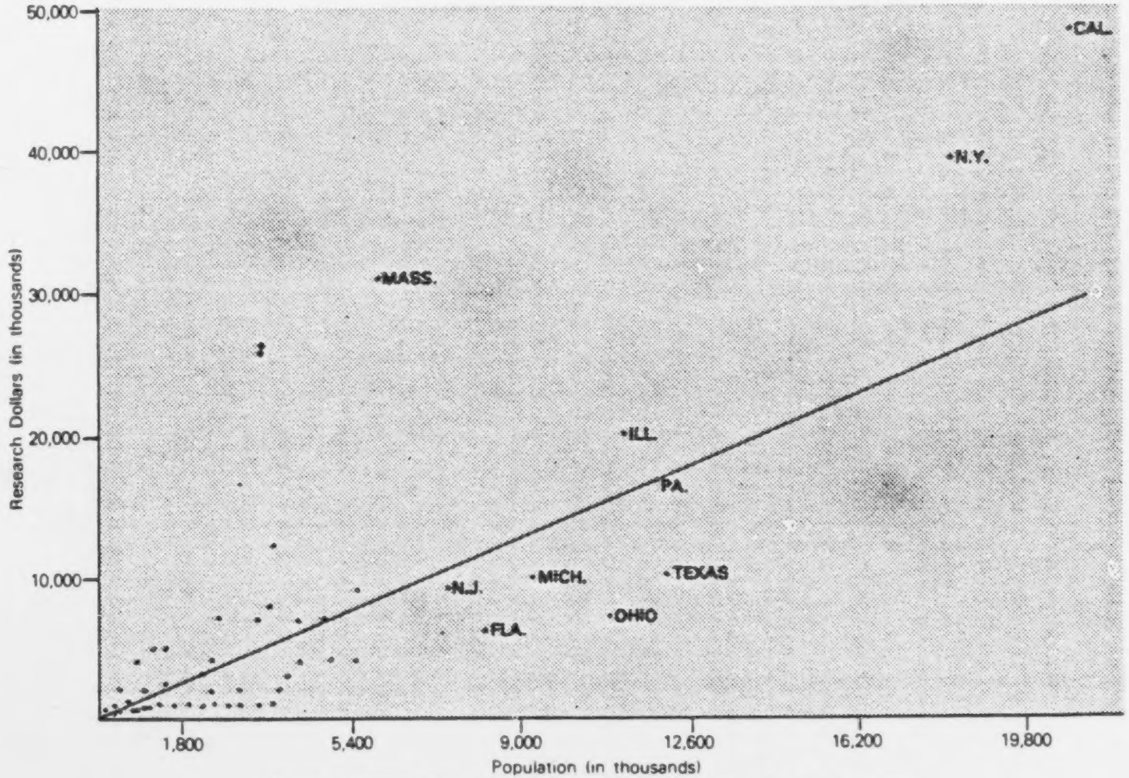
Dael Wolfle, in his background paper for this project, identified 17 universities whose shares of federal R&D funds increased by 25 percent or more from 1963-65 (a three-year average) to 1973-75, and an additional three universities whose shares increased between 15 and 24 percent over the same period.[51] Of course, the matter becomes more complicated when an attempt is made to chart changes by broad fields of science within the overall institutional totals. Table 5, which includes three definitions of the "20 largest," illustrates the complexities in interpreting the results of such an exercise. The data are affected by changes in funding within fields as well as overall changes in an institution's place on the list of recipients of federal funds. They confirm the picture of relative institutional concentration of funding but with considerable movement in relative positions on the list.

The geographical distribution of federal research funds remains concentrated as well. Yet in the past decade those regions with smaller shares of federal funds generally showed the largest rate of growth: the East South Central increasing 129 percent between 1963 and 1974; the West South Central, 105 percent; the South Atlantic, 102 percent; and the Mountain region, 101 percent.[52]

When research funds are plotted against state populations, as in Figure 11, it is apparent that most states receive proportions roughly corresponding to their populations, while only a few states receive much larger amounts. A rather different picture emerges if regional funding is plotted against rough measures of scientific quality. For example, in the distribution of research funds to states based on the number of members of the National Academy of Sciences and the National Acad-

Figure 11

Scientific Research Project Support Dollars for Each State
Plotted Against State Populations, 1974



Source: National Science Foundation, Atkinson, p. 271. See footnote 53.

emy of Engineering, some of the more populous states appear to be relatively disadvantaged. The states that seemingly do well in terms of population alone do less well than might be expected if judged by various criteria of merit. Results similar to the latter are obtained if one measures dollars to each state against publications in leading scientific journals. There are many ways in which to argue the issue, but the fundamental conclusion is the absence of severe geographical imbalances in the distribution of funds by most accepted measures.[53]

Site visits reveal considerable parochialism within universities about such questions as geographical distribution of research funds. Complaints at the elite universities ("It's an outrage; because of politics they are sending all that money to awful places where it will be wasted") complement the litany occasionally heard at less prestigious universities ("You can be sure the big boys won't leave much—even a superb proposal from our department doesn't have much of a chance"). Despite such complaints, the evidence does not suggest any striking regional imbalances.

[51] Dael Wolfle, "Forces Affecting the Research Role of Universities," *The State of Academic Science*, Vol. II (New Rochelle, N.Y.: Change Magazine Press, 1977). The institutions also invariably advanced in their place on the ranking of the list of the 100 largest recipients of federal R&D funds. Wolfle identified 10 universities whose shares decreased by 25 percent or more and 11 whose shares decreased from 15 percent to 24 percent over the same period.

[52] *Geographical Distribution of Federal Science Funds to Colleges and Universities*, GAO Report No. B-117219, Appendix I (Washington, D.C.: General Accounting Office), p. 6.

[53] For a thorough discussion, see the testimony of Dr. Richard C. Atkinson, Deputy Director, National Science Foundation, in *Peer Review Special Oversight Hearings* before the Subcommittee on Science, Research and Technology of the Committee on Science and Technology, U.S. House of Representatives, Ninety-fourth Congress, First Session (Washington, D.C.: Government Printing Office, July 23, 1975), pp. 269-291.

Stratification

One of the major findings of this study is the negative impact of recent trends on the less distinguished science departments and less distinguished scientific investigators. By most measures—research funding, graduate student enrollments, or other indicators of the vitality of the research effort—the most serious impact of recent trends has been felt in second-rank departments and institutions. In contrast, the leading science departments have tended to retain their scientific eminence and their relative strength. They have even strengthened their positions somewhat given the difficult circumstances for contemporary higher education. The downward trend has not appeared uniformly throughout all second-rank institutions, and the indicators of trouble have not always reached an advanced state. But the signs are sufficiently clear to warrant the conclusion that a very rapid deterioration in the relative position of many weaker departments could well occur in the near future. To the extent that a somewhat austere funding environment for academic research continues, these tendencies toward stratification may accelerate. The realities of modern science—the cost and increasing complexity of experimental research at the frontiers of knowdeledge—also create strong pressures on the system to have fewer departments fully prepared to conduct research of the highest quality.

Is this a development the nation should view with alarm, or should we accept and encourage it? No doubt some of the adjustments that have taken place have been healthy for the overall system. PhD programs in the 1960s perhaps overexpanded, and some of the research undertaken a decade ago was of less than first-rank quality. It is of course extremely important that the highest quality science continue to be nurtured and protected in a period of relative austerity if the United States is to continue its scientific leadership. It is in one sense highly reassuring that our system of awarding research grants and contracts, based on competition according to scientific merit, appears to have achieved its aim in making certain that preeminent investigators are able to continue their work, and in most cases with adequate support.

But there is a legitimate concern that the erosion may have proceeded too far and may indeed have left the nation with too few centers of scientific excellence. Since the nineteenth century the country has encouraged a broad base of scientific competence in many universities where research and teaching would be relatively closely related.[1] If the nation were to move too far in the direction of fewer and fewer outstanding university research departments, this national breadth and diversity would be endangered. It is possible that the competitive elements in the system could also be undermined if there were too few universities fully equipped for preeminent research. The nation might then be forced to rely on a narrow base of dominant centers. The competitive pressures in the system can, however, operate quite effectively with, say, 50 strong departments competing for research funds in a certain field in contrast to twice that number. But could such competitive pressures work equally well if there were only 15 serious producers of quality research in a given scientific field?

There are a variety of pressures pushing in the direction of increased stratification; these are related to the internal logic of a field, the practice of funding agencies to focus research energies on a specific target, the scale and cost of modern instrumentation and facilities, or other causes. They all pose these important issues: What kinds of research must be preserved as a matter of policy, and in which universities? Is it enough for the nation to have a relatively small number of clearly elite centers of scientific excellence?

There is little in our national experience to provide a ready basis for answering such questions. A return to a somewhat more stratified system of academic science would mean a partial return to the system prevailing before World War II, when relatively fewer universities were serious producers of research. But that was before the United States had become a leader in world science; American scientific preeminence occurred at a time when academic science was developing in a large number of new or revitalized universities all over the country.

Although the strongest departments and universities have remained in a relatively more secure position, signs of trouble are also appearing there. The weakening of other parts of the research network might gradually ripple through the entire system. There could be fewer opportunities for the most creative scientists to synthesize in their own theories the empirical work of others, less scientific activity at the periphery that feeds the mainstream of a discipline's development, and potentially fewer well trained and highly motivated students attracted to the main centers of scientific activity. Other undesirable side effects might result if too much divergence were allowed to develop among the science capacities of various universities.

Besides serving its unique role as an incubator for the next generation of scientists, the university is also engaged in the transmission of knowledge to the public. It could be highly undesirable if students in a certain region of the country were cut off from exposure to the most creative thinking available. No doubt the cultural life of a community could suffer if its higher education institutions lacked a research capability. There are also questions of balance in the internal development of a discipline. A broad base of inquiry, with many investigators spread across the entire spectrum of a field, has traditionally been one of the strengths of American academic science. If the total effort were focused exclusively on the most exciting "cutting edges" of a field, some important links among disciplinary subfields or across disciplinary lines could very well be missed.

We call attention to these issues not because there is one clear and obvious path that the nation should choose; in fact, we see no such single path. We believe it important for public officials, university spokesmen, and scientists to confront these issues. They must analyze the consequences of choices for people's lives and for the long-term benefit of the nation as a whole. The decade ahead will be one of continuing and most likely accelerating change as universities begin to consider their future courses of development. On the basis of present trends, it seems likely that some smaller universities will essentially confine their science efforts to high quality teaching at the undergraduate and first professional degree levels. Others will concentrate on specialized research efforts that take advantage of their special strengths.

Among all universities there will probably be considerable experimentation with distinctive PhD programs and research efforts. The National Board on Graduate Education recently issued a strong call for such university efforts.[2] Based on our site visits, we believe that many universities are beginning to think seriously along the lines of specialized and differentiated graduate programs. A number of universities may decide that they cannot maintain coverage in depth across the entire range of modern science or staff every specialty within a discipline; they may instead concentrate their scientific research efforts in several departments or focus on certain subfields within a department. (This is somewhat analogous to the situation, as discussed by Gilpin, of a small country that must select only certain areas of scientific endeavor for special attention.[3]) Gross and Grambsch have summarized the beginnings of this trend toward a more diverse set of academic institutions and science departments:

> Universities have become distinctly more stratified. The sets of goals exhibiting congruence among universities show distinct differences and clusterings. Goals and goal preferences are found to divide sharply in private as compared to public universities, in highly productive as compared to less productive universities, and in top prestige as compared to bottom prestige universities. The stratification has reached the point where one might more appropriately speak of it as fragmentation, for stratification does imply that the universities are part of the same system. If anything, some universities (most strikingly those of

> low prestige) appear to be seeking wholly new objectives which do not compete with those of the more prestigious universities. This tendency suggests a breakup of the system itself, with still further options for students of different interests and talents. The university system may then become less of a monolith, better adapted to the enormously diversified student body that now finds attendance at an institution of higher learning not simply an advantage but an obligation.[4]

Despite the likelihood of significant changes in the next decade—some universities dropping research programs, fewer universities managing comprehensive coverage of all major science fields, more specialization in research and training programs, and the like—there are probably limits to how far these developments will extend. It does not seem likely to us that the country will encourage or permit a basic shift in the nature of its higher education system—one that would result in a dozen or so major research universities, the remainder acting essentially as teaching institutions. And it is equally unlikely that some two hundred strong, research-intensive, doctoral granting institutions will be preserved in the future. Something between the broadly diversified system of the present and an extremely narrowly based system will probably emerge for academic science.

What we see as the most appropriate alternative is a somewhat slimmed down but still broadly based and competitive system, with vigorous ongoing scientific activity in numerous universities across the country. Without entering into extended analyses, we are skeptical about any revival of the NSF Science Development Grants Programs tailored to "needy" or "emergent" departments or universities.[5] Similarly, proposals for special grants to selected "outstanding" departments or universities seem of doubtful feasibility and wisdom. Programs designed to aid special constituencies inevitably tend to introduce political considerations into the process of awarding research grants. These are at odds with the principle of competition on the basis of scientific merit—a principle that has served the country well. The dominant pattern of federal support for university research is, and should remain, the competitive award of project grants and contracts, as urged by the Bell Report over a decade ago.[6]

The preservation of competition based on merit does not necessarily lead to the conclusion, however, that project support alone—and no other form of assistance to the university research effort—can be justified. Forms of institutional support exist that do not conflict with the merit principle—indeed, they can be geared to a merit formula—and they could help keep financially troubled universities engaged in creative research. The NIH biomedical research formula grant is one such example: It assists universities, up to a certain ceiling, with flexible funds to meet needs not covered by direct project grants. These discretionary funds are based on the amount of total competitive project funds received by the university. In the 1960s NSF had a formula grant similar to the NIH basic biomedical research grant, but it was recently phased out. The NSF formula was inversely related to the total amount of competitive grant funds received by the institution, which gave a slight proportional advantage to the smaller universities.

A revival of the NSF program, a strengthened NIH program, and the possible extension of this grant approach to other major federal R&D agencies could significantly assist universities in their efforts to retain a high-quality research capacity. Although this support formula would not persuade a university with little or no research capacity to compete for project funds, it would provide for those already committed to research a small amount of funds for equipment needs, start-up grants for young faculty, research assistants, or other special research purposes. The Bell Report, while endorsing the continuing framework of the project grant system, also recommended that a modest level of general institutional support be made part of the total federal effort.[7]

The readjustments that will take place over the next decade in the research activities and graduate programs of various universities will probably be determined by the intense competition among universities, and within universities, rather than by a central plan or design. Here again we express our view that these marketplace decisions are apt to be more rational than any vision of the role of academic science held by central planners. The nation will benefit from continued research competition; most of the changes in the position of universities will be associated with changes in the perceived quality of an institution's research programs. The decade ahead is likely to see even greater fluidity than in the recent past, with a number of institutions receiving more federal research funds while others drop back. Most of these adjustments will reflect some degree of logic inherent in the situation of a particular university, even though the decision to phase back an effort may be extremely painful and may foreclose the opportunity for a university to recover scientific strength in a field for years to come.

One uncertainty that complicates this competitive approach is the weakening financial base of many universities. The general financial difficulties faced by universities may present a situation where research competition between universities cannot operate effectively. A sharp deterioration in the total scientific capabilities of a wide range of universities could occur that would go beyond a healthy competitive adjustment in the system. Most universities now live in a state of manageable discomfort. None can fully do what it would like for its students, in research or in public service. Many suffer deterioration in their basic financial situations. All are apprehensive that restricted income and rising costs will further undermine their capacities to do adequately what society asks of them. A major factor influencing research in both public and private universities is the virtual disappearance of an institution's own funds for direct support of research. Over the long run, research cannot continue to flourish in universities without institutional strength or organizational integrity.

Relative declines in faculty salaries, deterioration of physical plant, sharp restrictions on services and travel, inability to undertake risky innovations with university funds, starvation of library budgets—all could have powerfully depressing effects on academic science. Clearly, the significance of unrestricted research funds will be enhanced. Even small discretionary gifts and grants could become highly important. Any university with access to even moderate amounts of discretionary funds will now have a distinct advantage in attracting outstanding faculty, in entering new fields of investigation, in setting its own priorities independent of swings in federal priorities, in starting younger faculty on research careers, and in dealing with discontinuities inherent in the project system of support.

The federal government continues to view universities as it does other suppliers of research services, principally as sites where research of interest to federal agencies can be carried on. It does not recognize (except in the case of the health professional schools through the relatively small NIH formula grant linked to biomedical research) a national responsibility for the continuing health of the universities themselves. We believe that the current federal view is inadequate and that measures need to be devised to strengthen the institutional base of universities within the continuing framework of the competitive project grant system.

Because of their declining financial position and the need to retain a reasonable degree of flexibility, the universities will no doubt continue to press for federal support not tied to specific projects. This seems likely even though general institutional support, if achieved, would make universities even more dependent on the federal government and more vulnerable to changes in federal policy. There is no easy answer to the dependency problem: The "federalization" of academic research has brought with it financial burdens for the universities that can only be alleviated by new infusions of federal support—and in turn are made worse by them. The more general—i.e., flexible—the funds, the more valued they will be, and the more leverage over university affairs even small amounts of federal money will enable the government to have. For the foreseeable future, the fundamental difference between universities and the federal government on the issue of institutional support will probably remain unresolved.

Institutional funds may not, of course, basically alter the tendencies toward stratification in the university research system, but some formula-type funds could at least provide a modest amount of flexible support to those institutions that traditionally have lacked endowment income, fees, gifts, and other flexible monies. Unless an institution can mount some degree of research effort, it will not be eligible for support under any formula pegged to the level of competitive grants received. The stratification issue clearly has roots in many developments of the past decade, and there is no ready answer to how the universities themselves, federal and state government, corporate and philanthropic leaders, and the public should react.

The trend toward fewer universities and departments able to mount first-class science efforts, especially in the experimental research areas, will continue so long as federal research support remains essentially level, which is likely to be the case. The most likely prospect for federal research support over the foreseeable future is that it will increase modestly, just matching the rate of economic growth. A return to the spectacular growth rates of the 1960s seems highly unlikely. To some extent universities may be able to preserve their research missions by achieving a greater functional division of labor among themselves and by developing highly specialized research programs. But there is a certain minimum coverage of basic science fields that a university must undertake in order to retain its research vitality. An overlap of function and competition for scarce research funds seems guaranteed by the realities of the academic science system. The task of analyzing the implications of these trends, and establishing the links between them and possible corrective action, should be high on the agenda of government officials and educational planners. The possibility of very rapid changes in the university science system in the years ahead makes a more comprehensive survey of academic science highly advisable. The new White House Office of Science and Technology Policy, as well as appropriate congressional committees and state legislative committees, would be logical sponsors of such analyses.

---

2. National Board on Graduate Education, *Final Report, 1975* (Washington, D.C.: National Academy of Sciences, 1975).

3. Robert G. Gilpin, Jr., *France and the Age of the Scientific State* (Princeton, N.J.: Princeton University Press, 1968).

4. Edward Gross and Paul V. Grambsch, *Changes in University Organization, 1964-71* (New York: McGraw-Hill, 1974), p. 205.

5. See David E. Drew, *Science Development: An Evaluation Study* (Washington, D.C.: National Board on Graduate Education, June 1975).

6. See the Bell Report, quoted in U.S. Congress, House Committee on Government Operations, *Hearings Before the Subcommittee on Government Operations*, Part I, Eighty-seventh Congress, Second Session 1962, pp. 192, 233.

7. Ibid.

8. "Meeting the Needs of Doctoral Education: Statement of Policy and Proposed Action by the Regents of the State of New York" (Albany: New York State Education Department, 1973), p. 25 (adapted from Princeton University Priorities Committee Guidelines for 1972).

federal funds for research and development fiscal years 1982, 1983, and 1984

volume xxxii

surveys of science resources series
national science foundation

detailed statistical tables

NSF 83-319

TABLE C-103. FEDERAL OBLIGATIONS FOR RESEARCH. DEVELOPMENT, AND R&D PLANT, BY GEOGRAPHIC DIVISION AND STATE: FISCAL YEAR 1982

(THOUSANDS OF DOLLARS)

GEOGRAPHIC DIVISION AND STATE	RESEARCH, DEVELOPMENT AND R&D PLANT	RESEARCH AND DEVELOPMENT	R&D PLANT
TOTAL	36,741,688	35,361,579	1,380,109
NEW ENGLAND	3,617,889	3,567,323	50,566
CONNECTICUT	464,636	439,554	25,082
MAINE	23,916	23,900	16
MASSACHUSETTS	2,798,490	2,775,192	23,298
NEW HAMPSHIRE	50,906	50,816	90
RHODE ISLAND	244,210	242,130	2,080
VERMONT	35,731	35,731	-
MIDDLE ATLANTIC	3,884,998	3,711,935	173,063
NEW JERSEY	944,916	887,944	56,972
NEW YORK	1,757,451	1,692,498	64,953
PENNSYLVANIA	1,182,631	1,131,493	51,138
EAST NORTH CENTRAL	2,367,584	2,260,149	107,435
ILLINOIS	617,292	543,023	74,269
INDIANA	186,797	186,172	625
MICHIGAN	384,503	376,701	7,802
OHIO	1,048,552	1,024,807	23,745
WISCONSIN	130,440	129,446	994
WEST NORTH CENTRAL	2,162,522	2,151,274	11,248
IOWA	138,509	136,270	2,239
KANSAS	637,782	637,664	118
MINNESOTA	379,028	378,616	412
MISSOURI	916,984	911,311	5,673
NEBRASKA	32,691	31,988	703
NORTH DAKOTA	37,703	36,460	1,243
SOUTH DAKOTA	19,825	18,965	860
SOUTH ATLANTIC	7,801,489	7,563,561	237,928
DELAWARE	34,688	34,582	106
DISTRICT OF COLUMBIA	988,242	954,649	33,593
FLORIDA	1,808,218	1,776,426	31,792
GEORGIA	218,727	217,263	1,464
MARYLAND	2,932,401	2,850,160	82,241
NORTH CAROLINA	274,061	272,671	1,390
SOUTH CAROLINA	163,892	98,732	65,160
VIRGINIA	1,289,194	1,268,063	21,131
WEST VIRGINIA	92,066	91,015	1,051
EAST SOUTH CENTRAL	1,565,034	1,486,683	78,351
ALABAMA	567,374	561,543	5,831
KENTUCKY	98,604	97,163	1,441
MISSISSIPPI	131,592	125,701	5,891
TENNESSEE	767,464	702,276	65,188
WEST SOUTH CENTRAL	1,434,771	1,400,110	34,661
ARKANSAS	44,511	43,485	1,026
LOUISIANA	258,974	236,791	22,183
OKLAHOMA	66,760	65,554	1,206
TEXAS	1,064,526	1,054,280	10,246
MOUNTAIN	3,272,253	3,002,829	269,424
ARIZONA	264,877	264,470	407
COLORADO	573,167	556,596	16,571
IDAHO	148,393	126,959	21,434
MONTANA	42,797	42,683	114
NEVADA	406,271	376,280	29,991
NEW MEXICO	1,519,195	1,322,595	196,600
UTAH	291,014	287,866	3,148
WYOMING	26,539	25,380	1,159
PACIFIC	10,533,501	10,118,023	415,478
ALASKA	40,016	40,003	13
CALIFORNIA	9,222,855	8,888,300	334,555
HAWAII	44,603	44,544	59
OREGON	106,399	106,319	80
WASHINGTON	1,119,628	1,038,857	80,771
OUTLYING AREAS	41,244	40,726	518
OFFICES ABROAD	60,403	58,966	1,437

NOTE: THE OBLIGATIONS OF THE TEN MAJOR R&D SUPPORTING AGENCIES INCLUDED IN THIS TABLE REPRESENT MORE THAN 97 PERCENT OF TOTAL FEDERAL R&D AND R&D PLANT OBLIGATIONS IN FY 1982.

SOURCE: NATIONAL SCIENCE FOUNDATION

TABLE C-104. FEDERAL OBLIGATIONS FOR RESEARCH AND DEVELOPMENT, BY STATE AND PERFORMER: FISCAL YEAR 1982

(THOUSANDS OF DOLLARS)

STATE (IN ORDER OF TOTAL FEDERAL R&D OBLIGATIONS)	TOTAL	FEDERAL INTRAMURAL 1/	EXTRAMURAL						
			INDUSTRIAL FIRMS	FFRDCS ADMIN BY INDUSTRIAL FIRMS	UNIVERSITIES AND COLLEGES	FFRDCS ADMIN BY UNIVS AND COLLEGES	OTHER NONPROFIT INSTITUTIONS	FFRDCS ADMIN BY NONPROFIT INSTITUTIONS	STATE AND LOCAL GOVTS
TOTAL	35,361,579	8,784,276	17,135,806	1,373,871	4,444,001	1,943,395	1,000,613	500,156	179,461
CALIFORNIA	8,888,300	1,181,326	5,722,621	75,001	627,520	799,278	163,567	296,934	22,053
MARYLAND	2,850,160	1,970,714	613,756	26,435	214,600	-	20,219	204	4,232
MASSACHUSETTS	2,775,192	239,069	1,544,034	-	514,832	153,977	243,096	72,416	7,768
FLORIDA	1,776,426	769,446	912,553	-	81,354	-	7,302	-	5,771
NEW YORK	1,692,498	128,801	689,475	145,047	464,443	127,671	104,645	-	32,416
NEW MEXICO	1,322,595	257,217	207,784	428,502	27,956	383,123	16,243	-	1,770
VIRGINIA	1,268,063	503,562	603,495	-	67,044	17,707	42,085	30,250	3,920
PENNSYLVANIA	1,131,493	234,405	347,448	254,437	237,383	-	53,423	-	4,397
TEXAS	1,054,280	232,771	570,668	-	218,509	-	25,686	-	6,646
WASHINGTON	1,038,857	66,264	662,062	115,621	116,127	-	22,513	49,523	6,737
OHIO	1,024,807	481,204	380,970	7,427	115,400	-	35,466	-	4,340
DISTRICT OF COLUMBIA	954,649	716,812	122,162	-	42,380	-	72,123	-	1,172
MISSOURI	911,311	30,131	783,377	-	82,335	-	13,385	-	2,083
NEW JERSEY	887,944	282,111	450,569	-	57,931	88,316	5,226	-	3,791
TENNESSEE	702,276	76,569	343,425	212,557	58,681	1,299	8,246	-	1,499
KANSAS	637,664	7,062	603,757	-	25,031	-	974	-	840
ALABAMA	561,543	322,751	176,409	-	47,144	-	13,788	-	1,451
COLORADO	556,596	83,391	301,006	317	76,308	32,534	8,366	50,819	3,855
ILLINOIS	543,023	53,489	47,014	-	181,456	230,380	26,579	-	4,105
CONNECTICUT	439,554	14,501	312,364	-	104,241	-	4,249	-	4,199
MINNESOTA	378,616	24,232	248,754	-	78,751	-	25,053	-	1,826
MICHIGAN	376,701	44,767	187,042	-	121,206	-	17,148	-	6,538
NEVADA	376,280	63,035	303,907	-	7,243	-	636	-	1,459
UTAH	287,866	50,299	179,824	-	53,245	-	2,014	-	2,484
NORTH CAROLINA	272,671	114,012	21,952	-	124,559	-	11,031	-	1,117
ARIZONA	264,470	67,759	137,274	-	63,444	11,220	2,177	-	2,596
RHODE ISLAND	242,130	177,806	23,722	-	32,371	-	7,593	-	638
LOUISIANA	236,791	37,692	160,618	-	32,613	-	3,914	-	1,954
GEORGIA	217,263	70,338	64,938	-	74,994	-	4,732	-	2,261
INDIANA	186,172	44,343	66,597	-	72,958	-	1,604	-	670
IOWA	136,270	13,339	60,837	-	47,165	13,800	114	-	1,015
WISCONSIN	129,446	18,844	6,468	1,643	99,271	-	1,983	-	1,237
IDAHO	126,959	10,125	4,800	52,675	5,197	50,030	1,672	-	2,460
MISSISSIPPI	125,701	87,509	18,908	-	17,559	-	870	-	855
OREGON	106,319	33,484	12,762	-	46,880	-	9,317	-	3,876
SOUTH CAROLINA	98,732	9,529	31,309	32,559	22,501	-	1,588	-	1,246
KENTUCKY	97,163	16,149	58,253	-	21,312	-	404	-	1,045
WEST VIRGINIA	91,015	38,801	25,516	-	10,450	14,840	82	-	1,326
OKLAHOMA	65,554	26,213	11,633	-	22,188	-	4,365	-	1,155
NEW HAMPSHIRE	50,816	15,720	16,246	-	17,289	-	902	-	659
HAWAII	44,544	10,703	7,760	-	22,132	-	3,212	-	737
ARKANSAS	43,485	25,741	5,682	-	10,656	-	58	-	1,348
MONTANA	42,683	17,944	10,785	-	7,458	-	479	-	6,017
ALASKA	40,003	21,215	1,412	-	10,713	-	1,975	-	4,688
NORTH DAKOTA	36,460	16,755	11,554	-	6,757	-	-	-	1,394
VERMONT	35,731	993	20,227	-	13,637	-	199	-	675
DELAWARE	34,582	966	3,108	21,650	8,247	-	556	-	55
NEBRASKA	31,988	10,013	6,197	-	12,827	-	1,917	-	1,034
WYOMING	25,380	18,143	215	-	6,786	-	-	-	236
MAINE	23,900	3,622	5,208	-	5,415	-	7,528	-	2,127
SOUTH DAKOTA	18,965	5,204	22	-	4,706	7,843	10	-	1,180
OUTLYING AREAS	40,726	22,516	128	-	12,150	5,320	104	-	508
OFFICES ABROAD	58,966	14,869	27,199	-	10,646	6,057	195	-	-

1/ FEDERAL INTRAMURAL ACTIVITIES COVER COSTS ASSOCIATED WITH THE ADMINISTRATION OF INTRAMURAL AND EXTRAMURAL PROGRAMS BY FEDERAL PERSONNEL AS WELL AS ACTUAL INTRAMURAL PERFORMANCE. SEE TECHNICAL NOTES FOR FURTHER DESCRIPTION.

NOTE: THE OBLIGATIONS OF THE TEN MAJOR R&D SUPPORTING AGENCIES INCLUDED IN THIS TABLE REPRESENT MORE THAN 97 PERCENT OF TOTAL FEDERAL R&D OBLIGATIONS IN FY 1982.

SOURCE: NATIONAL SCIENCE FOUNDATION

TABLE C-104A. PERCENT DISTRIBUTION OF FEDERAL OBLIGATIONS FOR RESEARCH AND DEVELOPMENT TO EACH PERFORMER, BY STATE: FISCAL YEAR 1982

STATE (IN ORDER OF TOTAL FEDERAL R&D OBLIGATIONS)	TOTAL	FEDERAL INTRAMURAL 1/	EXTRAMURAL						
			INDUSTRIAL FIRMS	FFRDCS ADMIN BY INDUSTRIAL FIRMS	UNIVERSITIES AND COLLEGES	FFRDCS ADMIN BY UNIVS AND COLLEGES	OTHER NONPROFIT INSTITUTIONS	FFRDCS ADMIN BY NONPROFIT INSTITUTIONS	STATE AND LOCAL GOVTS
TOTAL	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
CALIFORNIA	25.1	13.4	33.4	5.5	14.1	41.1	16.3	59.4	12.3
MARYLAND	8.1	22.4	3.6	1.9	4.8	-	2.0	*	2.4
MASSACHUSETTS	7.8	2.7	9.0	-	11.6	7.9	24.3	14.5	4.3
FLORIDA	5.0	8.8	5.3	-	1.8	-	.7	-	3.2
NEW YORK	4.8	1.5	4.0	10.6	10.5	6.6	10.5	-	18.1
NEW MEXICO	3.7	2.9	1.2	31.2	.6	19.7	1.6	-	1.0
VIRGINIA	3.6	5.7	3.5	-	1.5	.9	4.2	6.0	2.2
PENNSYLVANIA	3.2	2.7	2.0	18.5	5.3	-	5.3	-	2.5
TEXAS	3.0	2.6	3.1	-	4.9	-	2.6	-	3.7
WASHINGTON	2.9	.8	3.9	8.4	2.6	-	2.2	9.9	3.8
OHIO	2.9	5.5	2.2	.5	2.6	-	3.5	-	2.4
DISTRICT OF COLUMBIA	2.7	8.2	.7	-	1.0	-	7.2	-	.7
MISSOURI	2.6	.3	4.6	-	1.9	-	1.3	-	1.2
NEW JERSEY	2.5	3.2	2.6	-	1.3	4.5	.5	-	2.1
TENNESSEE	2.0	.9	2.0	15.5	1.3	.1	.8	-	.8
KANSAS	1.8	.1	3.5	-	.6	-	.1	-	.5
ALABAMA	1.6	3.7	1.0	-	1.1	-	1.4	-	.8
COLORADO	1.6	.9	1.8	*	1.7	1.7	.8	10.2	2.1
ILLINOIS	1.5	.6	.3	-	4.1	11.9	2.7	-	2.3
CONNECTICUT	1.2	.2	1.8	-	2.3	-	.4	-	2.3
MINNESOTA	1.1	.3	1.5	-	1.8	-	2.5	-	1.0
MICHIGAN	1.1	.5	1.1	-	2.7	-	1.7	-	3.6
NEVADA	1.1	.7	1.8	-	.2	-	.1	-	.8
UTAH	.8	.6	1.0	-	1.2	-	.2	-	1.4
NORTH CAROLINA	.8	1.3	.1	-	2.8	-	1.1	-	.6
ARIZONA	.7	.8	.8	-	1.0	.6	.2	-	1.4
RHODE ISLAND	.7	2.0	.1	-	.7	-	.8	-	.4
LOUISIANA	.7	.4	.9	-	.7	-	.4	-	1.1
GEORGIA	.6	.8	.4	-	1.7	-	.5	-	1.3
INDIANA	.5	.5	.4	-	1.6	-	.2	-	.4
IOWA	.4	.2	.4	-	1.1	.7	*	-	.6
WISCONSIN	.4	.2	*	.1	2.2	-	.2	-	.7
IDAHO	.4	.1	*	3.8	.1	2.6	.2	-	1.4
MISSISSIPPI	.4	1.0	.1	-	.4	-	.1	-	.5
OREGON	.3	.4	.1	-	1.1	-	.9	-	2.2
SOUTH CAROLINA	.3	.1	.2	2.4	.5	-	.2	-	.7
KENTUCKY	.3	.2	.3	-	.5	-	*	-	.6
WEST VIRGINIA	.3	.4	.1	-	.2	.8	*	-	.7
OKLAHOMA	.2	.3	.1	-	.5	-	.4	-	.6
NEW HAMPSHIRE	.1	.2	.1	-	.4	-	.1	-	.4
HAWAII	.1	.1	*	-	.5	-	.3	-	.4
ARKANSAS	.1	.3	*	-	.2	-	*	-	.8
MONTANA	.1	.2	.1	-	.2	-	*	-	3.4
ALASKA	.1	.2	*	-	.2	-	.2	-	2.6
NORTH DAKOTA	.1	.2	.1	-	.2	-	-	-	.8
VERMONT	.1	*	.1	-	.3	-	*	-	.4
DELAWARE	.1	*	*	1.6	.2	-	.1	-	*
NEBRASKA	.1	.1	*	-	.3	-	.2	-	.6
WYOMING	.1	.2	*	-	.2	-	-	-	.1
MAINE	.1	*	*	-	.1	-	.8	-	1.2
SOUTH DAKOTA	.1	.1	*	-	.1	.4	*	-	.7
OUTLYING AREAS	.1	.3	*	-	.3	.3	*	-	.3
OFFICES ABROAD	.2	.2	.2	-	.2	.3	*	-	-

1/ FEDERAL INTRAMURAL ACTIVITIES COVER COSTS ASSOCIATED WITH THE ADMINISTRATION OF INTRAMURAL AND EXTRAMURAL PROGRAMS BY FEDERAL PERSONNEL AS WELL AS ACTUAL INTRAMURAL PERFORMANCE. SEE TECHNICAL NOTES FOR FURTHER DESCRIPTION.

* PERCENTAGE LESS THAN 0.05%.

SOURCE: NATIONAL SCIENCE FOUNDATION

TABLE C-104B. PERCENT DISTRIBUTION OF FEDERAL OBLIGATIONS FOR RESEARCH AND DEVELOPMENT TO EACH STATE, BY PERFORMER: FISCAL YEAR 1982

STATE (IN ORDER OF TOTAL FEDERAL R&D OBLIGATIONS)	TOTAL	FEDERAL INTRAMURAL 1/	EXTRAMURAL						
			INDUSTRIAL FIRMS	FFRDCS ADMIN BY INDUSTRIAL FIRMS	UNIVERSITIES AND COLLEGES	FFRDCS ADMIN BY UNIVS AND COLLEGES	OTHER NON-PROFIT INSTITUTIONS	FFRDCS ADMIN BY NONPROFIT INSTITUTIONS	STATE AND LOCAL GOVTS
TOTAL	100.0%	24.8%	48.5%	3.9%	12.6%	5.5%	2.8%	1.4%	0.5%
CALIFORNIA	100.0	13.3	64.4	.8	7.1	9.0	1.8	3.3	.2
MARYLAND	100.0	69.1	21.5	.9	7.5	-	.7	*	.1
MASSACHUSETTS	100.0	8.6	55.6	-	18.6	5.5	8.6	2.6	.3
FLORIDA	100.0	43.3	51.4	-	4.6	-	.4	-	.3
NEW YORK	100.0	7.6	40.7	8.6	27.4	7.5	6.2	-	1.9
NEW MEXICO	100.0	19.4	15.7	32.4	2.1	29.0	1.2	-	.1
VIRGINIA	100.0	39.7	47.6	-	5.3	1.4	3.3	2.4	.3
PENNSYLVANIA	100.0	20.7	30.7	22.5	21.0	-	4.7	-	.4
TEXAS	100.0	22.1	54.1	-	20.7	-	2.4	-	.6
WASHINGTON	100.0	6.4	63.7	11.1	11.2	-	2.2	4.8	.6
OHIO	100.0	47.0	37.2	.7	11.3	-	3.5	-	.4
DISTRICT OF COLUMBIA	100.0	75.1	12.8	-	4.4	-	7.6	-	.1
MISSOURI	100.0	3.3	86.0	-	9.0	-	1.5	-	.2
NEW JERSEY	100.0	31.8	50.7	-	6.5	9.9	.6	-	.4
TENNESSEE	100.0	10.9	48.9	30.3	8.4	.2	1.2	-	.2
KANSAS	100.0	1.1	94.7	-	3.9	-	.2	-	.1
ALABAMA	100.0	57.5	31.4	-	8.4	-	2.5	-	.3
COLORADO	100.0	15.0	54.1	.1	13.7	5.8	1.5	9.1	.7
ILLINOIS	100.0	9.9	8.7	-	33.4	42.4	4.9	-	.8
CONNECTICUT	100.0	3.3	71.1	-	23.7	-	1.0	-	1.0
MINNESOTA	100.0	6.4	65.7	-	20.8	-	6.6	-	.5
MICHIGAN	100.0	11.9	49.7	-	32.2	-	4.6	-	1.7
NEVADA	100.0	16.8	80.8	-	1.9	-	.2	-	.4
UTAH	100.0	17.5	62.5	-	18.5	-	.7	-	.9
NORTH CAROLINA	100.0	41.8	8.1	-	45.7	-	4.0	-	.4
ARIZONA	100.0	25.6	51.9	-	16.4	4.2	.8	-	1.0
RHODE ISLAND	100.0	73.4	9.8	-	13.4	-	3.1	-	.3
LOUISIANA	100.0	15.9	67.8	-	13.8	-	1.7	-	.8
GEORGIA	100.0	32.4	29.9	-	34.5	-	2.2	-	1.0
INDIANA	100.0	23.8	35.8	-	39.2	-	.9	-	.4
IOWA	100.0	9.8	44.6	-	34.6	10.1	.1	-	.7
WISCONSIN	100.0	14.6	5.0	1.3	76.7	-	1.5	-	1.0
IDAHO	100.0	8.0	3.8	41.5	4.1	39.4	1.3	-	1.9
MISSISSIPPI	100.0	69.6	15.0	-	14.0	-	.7	-	.7
OREGON	100.0	31.5	12.0	-	44.1	-	8.8	-	3.6
SOUTH CAROLINA	100.0	9.7	31.7	33.0	22.8	-	1.6	-	1.3
KENTUCKY	100.0	16.6	60.0	-	21.9	-	.4	-	1.1
WEST VIRGINIA	100.0	42.6	28.0	-	11.5	16.3	.1	-	1.5
OKLAHOMA	100.0	40.0	17.7	-	33.8	-	6.7	-	1.8
NEW HAMPSHIRE	100.0	30.9	32.0	-	34.0	-	1.8	-	1.3
HAWAII	100.0	24.0	17.4	-	49.7	-	7.2	-	1.7
ARKANSAS	100.0	59.2	13.1	-	24.5	-	.1	-	3.1
MONTANA	100.0	42.0	25.3	-	17.5	-	1.1	-	14.1
ALASKA	100.0	53.0	3.5	-	26.8	-	4.9	-	11.7
NORTH DAKOTA	100.0	46.0	31.7	-	18.5	-	-	-	3.8
VERMONT	100.0	2.8	56.6	-	38.2	-	.6	-	1.9
DELAWARE	100.0	2.8	9.0	62.6	23.8	-	1.6	-	.2
NEBRASKA	100.0	31.3	19.4	-	40.1	-	6.0	-	3.2
WYOMING	100.0	71.5	.8	-	26.7	-	-	-	.9
MAINE	100.0	15.2	21.8	-	22.7	-	31.5	-	8.9
SOUTH DAKOTA	100.0	27.4	.1	-	24.8	41.4	.1	-	6.2
OUTLYING AREAS	100.0	55.3	.3	-	29.8	13.1	.3	-	1.2
OFFICES ABROAD	100.0	25.2	46.1	-	18.1	10.3	.3	-	-

1/ FEDERAL INTRAMURAL ACTIVITIES COVER COSTS ASSOCIATED WITH THE ADMINISTRATION OF INTRAMURAL AND EXTRAMURAL PROGRAMS BY FEDERAL PERSONNEL AS WELL AS ACTUAL INTRAMURAL PERFORMANCE. SEE TECHNICAL NOTES FOR FURTHER DESCRIPTION.

* PERCENTAGE LESS THAN 0.05%.

SOURCE: NATIONAL SCIENCE FOUNDATION

TABLE C-105. FEDERAL OBLIGATIONS FOR RESEARCH AND DEVELOPMENT, BY STATE AND AGENCY: FISCAL YEAR 1982

(THOUSANDS OF DOLLARS)

STATE (IN ORDER OF TOTAL FEDERAL R&D OBLIGATIONS)	TOTAL	DEPT OF AGRI-CULTURE	DEPT OF COMMERCE	DEPT OF DEFENSE	DEPT OF ENERGY	DEPT OF HEALTH & HUMAN SERVICES	DEPT OF THE INTERI-OR	DEPT OF TRANS-POR-TATION	ENVIRON-MENTAL PROTEC-TION AGENCY	NATIONAL AERO-NAUTICS & SPACE ADMIN	NATIONAL SCIENCE FOUNDA-TION
TOTAL....................	35,361,579	795,198	335,984	20,545,540	4,696,719	3,914,111	380,638	310,083	335,089	3,073,667	974,550
CALIFORNIA..............	8,885,300	49,298	14,292	6,141,172	923,106	431,276	38,942	29,123	36,075	1,071,856	153,160
MARYLAND................	2,850,160	71,272	107,860	1,361,326	40,871	919,929	12,600	30,971	10,416	278,738	16,177
MASSACHUSETTS...........	2,775,192	6,426	14,259	2,134,654	108,290	310,409	7,654	35,850	17,781	58,418	21,451
FLORIDA.................	1,776,426	19,188	26,050	1,292,802	3,530	41,588	4,570	5,386	8,700	360,601	14,011
NEW YORK................	1,692,498	23,690	5,116	836,415	264,230	410,936	6,566	11,904	7,469	29,184	96,988
NEW MEXICO..............	1,322,595	3,579	646	449,771	831,478	9,509	6,159	1,223	544	15,101	4,585
VIRGINIA................	1,268,063	6,579	5,897	923,727	40,847	52,917	26,870	23,521	14,291	162,647	10,767
PENNSYLVANIA............	1,131,493	24,620	407	434,953	380,590	178,347	20,549	9,770	6,319	39,342	36,596
TEXAS...................	1,054,280	38,484	4,401	447,898	48,770	141,882	8,963	25,985	15,194	299,725	22,978
WASHINGTON..............	1,038,857	14,819	32,773	637,558	221,027	81,844	13,048	5,900	4,737	5,636	21,515
OHIO....................	1,024,807	11,043	810	619,490	113,328	94,413	4,115	10,725	38,851	117,067	14,965
DISTRICT OF COLUMBIA...	954,649	91,277	16,267	350,335	89,072	83,851	20,955	42,554	18,675	113,751	127,912
MISSOURI................	911,311	11,118	612	800,411	2,292	70,158	10,937	2,353	2,092	3,896	7,442
NEW JERSEY..............	887,944	3,288	13,479	668,068	120,570	27,706	3,418	11,601	2,046	12,188	25,580
TENNESSEE...............	702,276	7,185	499	281,428	339,600	50,671	2,336	2,306	7,983	4,038	6,230
KANSAS..................	637,664	6,588	34	605,579	2,220	13,350	1,043	537	154	4,579	3,580
ALABAMA.................	561,543	9,748	1,336	306,457	11,219	41,810	5,756	2,092	1,744	180,254	1,127
COLORADO................	556,596	14,590	43,460	253,498	78,759	36,744	33,608	12,400	4,930	32,101	46,506
ILLINOIS................	543,023	30,394	875	68,860	260,377	110,762	5,583	4,625	7,060	9,180	45,307
CONNECTICUT.............	439,554	3,107	1,710	278,979	11,512	84,906	1,455	5,831	2,849	36,772	12,433
MINNESOTA...............	378,616	13,225	345	235,205	17,507	76,173	11,698	368	8,944	4,353	10,798
MICHIGAN................	376,701	11,286	5,383	210,822	17,273	81,750	8,805	6,837	3,331	9,134	22,080
NEVADA..................	376,280	1,931	87	46,487	299,376	553	9,796	154	15,788	990	1,118
UTAH....................	287,866	7,964	70	180,559	6,671	24,855	11,787	304	786	46,642	8,228
NORTH CAROLINA..........	272,671	17,788	3,229	37,501	3,610	133,947	2,961	838	55,843	5,462	11,492
ARIZONA.................	264,470	13,430	169	172,639	13,383	20,852	5,462	2,126	886	13,879	21,644
RHODE ISLAND............	242,130	1,129	2,112	199,193	2,618	13,062	850	416	6,481	948	15,321
LOUISIANA...............	236,791	34,926	2,287	30,196	40,622	24,257	2,628	2,043	1,390	96,118	2,324
GEORGIA.................	217,263	30,596	942	72,869	24,529	60,441	3,586	1,325	6,566	8,060	8,349
INDIANA.................	186,172	7,785	494	106,013	8,126	28,884	1,909	2,141	653	6,425	23,742
IOWA....................	136,270	18,718	265	62,919	13,355	30,552	1,906	298	397	3,745	4,115
WISCONSIN...............	129,446	22,300	2,654	9,209	8,510	58,817	3,313	497	1,788	4,333	18,025
IDAHO...................	126,959	9,460	126	20	109,191	525	6,846	126	219	130	316
MISSISSIPPI.............	125,701	23,561	5,992	71,349	1,981	7,709	1,324	877	480	11,010	1,418
OREGON..................	106,319	17,826	3,209	4,620	15,510	30,051	12,717	718	8,319	1,899	11,450
SOUTH CAROLINA..........	98,732	10,684	2,150	3,906	63,018	11,717	2,133	462	370	158	4,134
KENTUCKY................	97,163	8,006	73	10,358	54,897	15,582	2,105	735	2,646	941	1,820
WEST VIRGINIA...........	91,015	7,759	29	25,554	24,394	11,166	5,583	388	390	3	15,749
OKLAHOMA................	65,554	9,215	2,303	11,025	8,403	9,619	1,701	9,663	7,098	3,918	2,609
NEW HAMPSHIRE...........	50,816	3,037	354	25,984	4,272	8,732	1,777	209	553	3,169	2,729
HAWAII..................	44,544	3,668	5,107	13,729	2,347	7,332	2,893	338	162	4,313	4,655
ARKANSAS................	43,485	6,780	132	904	4,822	25,318	1,879	1,305	770	290	1,285
MONTANA.................	42,683	10,138	146	1,187	10,656	5,744	11,367	198	1,368	107	1,772
ALASKA..................	40,003	4,853	4,554	7,932	759	329	13,401	360	156	1,570	6,089
NORTH DAKOTA............	36,460	14,449	117	11,580	3,082	1,386	5,221	253	-	10	362
VERMONT.................	35,731	2,278	81	20,589	27	10,624	872	59	114	270	817
DELAWARE................	34,582	2,419	565	2,657	22,014	1,927	579	248	148	842	3,183
NEBRASKA................	31,988	13,263	36	6,586	1,473	6,029	1,159	333	174	244	2,691
WYOMING.................	25,380	3,553	61	527	12,917	611	4,879	278	474	327	1,753
MAINE...................	23,900	3,462	914	4,303	834	7,159	1,992	1,118	815	476	2,827
SOUTH DAKOTA............	18,965	4,546	111	2,623	8,031	787	1,904	108	70	255	530
OUTLYING AREAS..........	40,726	7,044	1,057	20,529	823	4,613	478	294	-	130	5,758
OFFICES ABROAD..........	58,966	1,826	47	42,585	-	-	-	9	-	8,442	6,057

NOTE: THE OBLIGATIONS OF THE TEN MAJOR R&D SUPPORTING AGENCIES INCLUDED IN THIS TABLE REPRESENT MORE THAN 97 PERCENT OF TOTAL FEDERAL R&D OBLIGATIONS IN FY 1982.

SOURCE: NATIONAL SCIENCE FOUNDATION

TABLE C-105A. PERCENT DISTRIBUTION OF FEDERAL OBLIGATIONS FOR RESEARCH AND DEVELOPMENT OF EACH AGENCY, BY STATE: FISCAL YEAR 1982

STATE (IN ORDER OF TOTAL FEDERAL R&D OBLIGATIONS)	TOTAL	DEPT OF AGRICULTURE	DEPT OF COMMERCE	DEPT OF DEFENSE	DEPT OF ENERGY	DEPT OF HEALTH & HUMAN SERVICES	DEPT OF THE INTERIOR	DEPT OF TRANSPORTATION	ENVIRONMENTAL PROTECTION AGENCY	NATIONAL AERONAUTICS & SPACE ADMIN	NATIONAL SCIENCE FOUNDATION
TOTAL	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
CALIFORNIA	25.1	6.2	4.3	29.9	19.7	11.0	10.2	9.4	10.8	34.9	15.7
MARYLAND	8.1	9.0	32.1	6.6	.9	23.5	3.3	10.0	3.1	9.1	1.7
MASSACHUSETTS	7.8	.8	4.2	10.4	2.3	7.9	2.0	11.6	5.3	1.9	8.4
FLORIDA	5.0	2.4	7.8	6.3	.1	1.1	1.2	1.7	2.6	11.7	1.4
NEW YORK	4.8	3.0	1.5	4.1	5.6	10.5	1.7	3.8	2.2	.9	10.0
NEW MEXICO	3.7	.5	.2	2.2	17.7	.2	1.6	.4	.2	.5	.5
VIRGINIA	3.6	.8	1.8	4.5	.9	1.4	7.1	7.6	4.3	5.3	1.1
PENNSYLVANIA	3.2	3.1	.1	2.1	8.1	4.6	5.4	3.2	1.9	1.3	3.8
TEXAS	3.0	4.8	1.3	2.2	1.0	3.6	2.4	8.4	4.5	9.8	2.4
WASHINGTON	2.9	1.9	9.8	3.1	4.7	2.1	3.4	1.9	1.4	.2	2.2
OHIO	2.9	1.4	.2	3.0	2.4	2.4	1.1	3.5	11.6	3.8	1.5
DISTRICT OF COLUMBIA	2.7	11.5	4.8	1.7	1.9	2.1	5.5	13.7	5.6	3.7	13.1
MISSOURI	2.6	1.4	.2	3.9	*	1.8	2.9	.8	.6	.1	.8
NEW JERSEY	2.5	.4	4.0	3.3	2.6	.7	.9	3.7	.6	.4	2.6
TENNESSEE	2.0	.9	.1	1.4	7.2	1.3	.6	.7	2.4	.1	.6
KANSAS	1.8	.8	*	2.9	*	.3	.3	.2	*	.1	.4
ALABAMA	1.6	1.2	.4	1.5	.2	1.1	1.5	.7	.5	5.9	.1
COLORADO	1.6	1.8	12.9	1.2	1.7	.9	8.8	4.0	1.5	1.0	4.8
ILLINOIS	1.5	3.8	.3	.3	5.5	2.8	1.5	1.5	2.1	.3	4.6
CONNECTICUT	1.2	.4	.5	1.4	.2	2.2	.4	1.9	.9	1.2	1.3
MINNESOTA	1.1	1.7	.1	1.1	.4	1.9	3.1	.1	2.7	.1	1.1
MICHIGAN	1.1	1.4	1.6	1.0	.4	2.1	2.3	2.2	1.0	.3	2.3
NEVADA	1.1	.2	*	.2	6.4	*	2.6	*	4.7	*	.1
UTAH	.8	1.0	*	.9	.1	.6	3.1	.1	.2	1.5	.8
NORTH CAROLINA	.8	2.2	1.0	.2	.1	3.4	.8	.3	16.7	.2	1.2
ARIZONA	.7	1.7	.1	.8	.3	.5	1.4	.7	.3	.5	2.2
RHODE ISLAND	.7	.1	.6	1.0	.1	.3	.2	.1	1.9	*	1.6
LOUISIANA	.7	4.4	.7	.1	.9	.6	.7	.7	.4	3.1	.2
GEORGIA	.6	3.8	.3	.4	.5	1.5	.9	.4	2.0	.3	.9
INDIANA	.5	1.0	.1	.5	.2	.7	.5	.7	.2	.2	2.4
IOWA	.4	2.4	.1	.3	.3	.8	.8	.1	.1	.1	.4
WISCONSIN	.4	2.8	.8	*	.2	1.5	.9	.2	.5	.1	1.8
IDAHO	.4	1.2	*	*	2.3	*	1.8	*	.1	*	*
MISSISSIPPI	.4	3.0	1.8	.3	*	.2	.3	.3	.1	.4	.1
OREGON	.3	2.2	1.0	*	.3	.8	3.1	.2	2.5	.1	1.2
SOUTH CAROLINA	.3	1.3	.6	*	1.3	.3	.6	.1	.1	*	.4
KENTUCKY	.3	1.0	*	.1	1.2	.4	.6	.2	.8	*	.2
WEST VIRGINIA	.3	1.0	*	.1	.5	.3	1.5	.1	.1	*	1.6
OKLAHOMA	.2	1.2	.7	.1	.2	.2	.4	3.1	2.1	.1	.3
NEW HAMPSHIRE	.1	.4	.1	.1	.1	.2	.5	.1	.2	.1	.3
HAWAII	.1	.5	1.5	.1	*	.2	.8	.1	*	.1	.5
ARKANSAS	.1	.9	*	*	.1	.6	.5	.4	.2	*	.1
MONTANA	.1	1.3	*	*	.2	.1	3.0	.1	.4	*	.2
ALASKA	.1	.6	1.4	*	*	*	3.5	.1	*	.1	.6
NORTH DAKOTA	.1	1.8	*	.1	.1	*	1.4	.1	-	*	*
VERMONT	.1	.3	*	.1	*	.3	.2	*	*	*	.1
DELAWARE	.1	.3	.2	*	.5	*	.2	.1	*	*	.3
NEBRASKA	.1	1.7	*	*	*	.2	.3	.1	.1	*	.3
WYOMING	.1	.4	*	*	.3	*	1.3	.1	.1	*	.2
MAINE	.1	.4	.3	*	*	.2	.5	.4	.2	*	.3
SOUTH DAKOTA	.1	.6	*	*	.2	*	.5	*	*	*	.1
OUTLYING AREAS	.1	.9	.3	.1	*	.1	.1	.1	-	*	.6
OFFICES ABROAD	.2	.2	*	.2	-	-	-	*	-	.3	.6

* PERCENTAGE LESS THAN 0.05%.

SOURCE: NATIONAL SCIENCE FOUNDATION

B. THE 98TH CONGRESS: THE DISPUTE INTENSIFIES

UNIVERSITY RESEARCH FACILITIES

AND PORK BARREL POLITICS

Philip M. Smith

and

Albert H. Teich

UNIVERSITY RESEARCH FACILITIES AND PORK BARREL POLITICS

Philip M. Smith
and
Albert H. Teich

SUMMARY

During the past year, a number of universities have obtained funding for research facilities by direct intervention in the congressional budget process. This "pork barrel" approach contrasts sharply with the traditional process of organized consultation and peer review which science has used for allocating resources, and also differs significantly from previous instances of university lobbying. The phenomenon poses serious threats to the political autonomy of science, to the basis for its support by society, and to the effectiveness of the nation's research. A broad-based discussion in the scientific community, including consideration of alternative courses of action, is called for.

UNIVERSITY RESEARCH FACILITIES AND PORK BARREL POLITICS

Philip M. Smith
and
Albert H. Teich

The first session of the 98th Congress was marked by an unusual degree of interest and activity in science and technology. Not since Sputnik, it would seem, have so many diverse elements of the Congress--from all parts of the political spectrum--focused their attention on the nurturing and application of science and technology. Amidst the rush to increase funding for basic research, to respond to the crisis in science education, and to bring the benefits of "high tech" industry to every corner of the nation, a rather disturbing phenomenon arose. This phenomenon, which the press promptly labeled "the academic pork barrel," involved attempts by several universities to bypass conventional budgetary and appropriations procedures and gain funding for campus research facilities through direct intervention in the Congressional appropriations process.

The flurry of legislative activity took place in the spring and summer of 1983. Several universities, some retaining the advice of Washington-based consulting and lobbying organizations, sought appropriations for facilities which had not been part of any executive agency budget request, and which had not even been reviewed by the relevant Congressional committees. In several instances, the funds were diverted from other programs rather than added on to the requests.

Philip M. Smith is Executive Officer, National Research Council, Washington, D.C., 20418. Albert H. Teich is Manager, Science Policy Studies, American Association for the Advancement of Science, Washington, D.C. 20036.

Some efforts were successful: Catholic University, Columbia University, the University of Oregon, and the University of New Hampshire, respectively, obtained appropriations for a vitreous state laboratory, a chemical research facility, a medical school library, and a space and marine sciences building. In addition, Boston College acquired funds for a new library. Other attempts which followed in the wake of this first effort involved science construction projects at Boston University, the University of Pennsylvania, and the University of New Mexico. These were not successful. Later, following widespread negative reaction by scientific and higher education organizations, informal agreement was reached among science and appropriations committee leaders in the House and Senate that forestalled additional academic pork barrel activity during the remainder of the session.

Following the end of the Congressional session, scientific and academic leaders as well as some journalists, expressed the belief--or at least the hope--that the lobbying activity had been a transitory phenomenon, and that it would not recur. (1) Judging from similar activities taking place in 1984, this does not seem to be the case. In fact, the academic pork barrel may mark the emergence of a new, more contentious phase in the politics of science, altering the long established peer review process for the allocation of research funds.

The President's FY 1985 budget includes proposals for interdisciplinary engineering centers, an expanded biotechnology grants program, and increased access for academic researchers to supercomputers. These new initiatives present themselves as attractive candidates for pork barrel activity. Individual campuses and consortia of universities are actively using hired consultants to help them with their proposals to acquire these prizes. And, in a pattern similar to the lobbying in the first session of the 98th Congress, a number of universities are using political connections to seek funds for research facilities that have neither been requested by the President nor developed as legislative proposals by the appropriation and authorization committees of the Congress. Georgetown University, for example, is seeking diversion of $160 million of Army research funds to build an experimental coal-gasification fuel cell cogeneration facility. (2,3)

We believe this new phenomenon deserves wide discussion by scientists, since it poses some serious dilemmas and dangers for the scientific community. If it continues, the pork barrel phenomenon represents an important change in the process through which the federal government allocates resources for the support of research facilities. Carried to extremes, it could constitute a serious threat to the political autonomy which science has historically enjoyed in this country. In the Administration's FY 1985 budget, nearly $7 billion is identified for support of basic research in civilian agencies, of which by far the largest share is directed toward colleges and universities. This is roughly 8 percent of all nondefense discretionary spending in FY 1985. (4) The image of universities blatantly lobbying for their self-interests will raise questions about the management of this substantial sum and the degree to which it is being used in the national interest rather than in private interests. This, in turn, would surely erode the political support base for federal funding of basic research.

The New Lobbying Efforts

Lobbying, of course, is nothing new in Washington, even in academia and the halls of science. And political jockeying over the location of research facilities has a long history as well. We are convinced, however, that the current academic pork barrel is different in some significant ways from past instances of academic competition for research facilities.

The surge of pork barrel politics first came to public attention in May 1983, when Rep. Norman Mineta (D-Calif.) proposed a floor amendment to the Department of Energy (DOE) authorization bill which reduced the budget of the proposed new National Center for Advanced Materials (NCAM) by $5 million and allocated the funds instead to the construction of a vitreous state research laboratory at Catholic University. (5) At the same time, Representative Charles Rangel (D-N.Y.) sponsored an amendment--in this case with the reluctant support of Science and Technology Committee Chairman Don Fuqua (D-Fla.)--to reallocate $5 million from various Department of Energy basic research programs to begin construction of a new chemistry research building at Columbia University. (6) The amendments were apparently well-greased, passed easily, and similar provisions were inserted into the DOE appropriations bill at the subcommittee level.

The Columbia and Catholic University maneuvers, which succeeded in the Senate as well, were followed by a number of similar cases. In June, during Senate debate on a supplemental appropriations bill for the FY 1983 budget, an amendment was offered from the floor providing $7.5

million to complete a library at Boston College, $15 million for a space and marine sciences center at the University of New Hampshire, and $20.4 million for a biomedical library at the University of Oregon. (7) The measure was the product of a deal struck among several powerful senators who represent the states in which the institutions are located. It passed without debate.

Subsequently, the Senate voted multi-million dollar amendments to an appropriations bill providing funds for a new dental school at the University of Pennsylvania, a new engineering laboratory at the University of New Mexico, and an engineering building at Boston University. (8) These last three amendments failed in conference, but the others gained final passage.

In a number of these cases, the institutions were represented by Schlossberg-Cassidy and Associates, a Washington lobbying firm whose name has become closely interwoven with the academic pork barrel saga. In other cases, representatives of the universities dealt directly with senators, representatives and their staffs. (9)

Some observers lay this outbreak of institution-oriented lobbying at the doorstep of the Administration itself. As noted above, the President's FY 1984 budget contained $26 million for initiating construction of a National Center for Advanced Materials at the Lawrence Berkeley Laboratory, a project intended to cost $266 million over six years. On the scale usually discussed in the materials sciences community, this was a large sum. NCAM had apparently had little prior discussion in the scientific community or among officials of the Department of Energy and its research advisory board. Questions were immediately raised concerning the project in many quarters, including the materials science community. (10) These questions, as well as an outpouring of angry letters from materials researchers, eventually led Congress to express concern, ironically, about the lack of peer review for the proposed project and to allocate only $3 million for NCAM construction in the FY 1984 budget. (11,12) Subsequently, following review by an ad hoc advisory panel, the DOE decided to go ahead with a scaled-down version of the project, dropping the "National" from its title and, at least for FY 1985, dropping the advanced synchrotron light source that was to be its centerpiece. (13)

The NCAM example--regarded by cynics on Capitol Hill as Executive Branch "pork"--may have served as a trigger for the events of 1983, but the "high tech bandwagon" that developed in the Congress certainly facilitated its spread. "High tech" has acquired enormous political appeal as the public has come to associate it with regional economic development and, especially, jobs. Academic research facilities--regardless of their actual connections with high technology industry--can be important symbols to a region or locality and the temptation is strong for a representative or senator to snag one for his or her constituents.

How does the current pork barrel competition differ from what has come before, from the point of view of the scientific community? The major difference is that past competitions and lobbying have generally been for announced facilities that have been subject to considerable discussion and planning by the scientific community and by public officials charged with the management of research and research policymaking in the Executive Branch and Congress. Past competitions have included university research facilities for high energy physics and astronomy, as well as university development grants funding a variety of science and engineering centers at universities. These competitions followed the more conventional budgetary procedures that have characterized the way in which federal research support has been provided over the last several decades.

In the case of Columbia University, Catholic University, and the other instances, the lobbying was an effort to insert funds for science facilities into the appropriations process without prior consideration anywhere in the scientific community, through scientific advisory bodies to government research agencies, in the Office of Management and Budget in its normal review of agency proposals for the President's budget, or by Congressional appropriation and authorization committees in their reviews of the President's budget. In the case of NCAM, the proposal became part of the Executive budget but apparently in the absence of scientific community input.

The Columbia and Catholic University projects, furthermore, were not "add-ons" as is frequently the case with pork barrel items. They were, instead, diversions of funds from other items in appropriations requests--specifically from the NCAM proposal. The Congressional sponsors of these measures apparently felt some justification for their actions, since NCAM had not been subject to conventional peer review, but the precedent was significant. In the Georgetown University case which has recently come to light, funds for the proposed effort would be diverted from the Army's regular research activities.

The reaction of scientific organizations to this new approach to obtaining funds for academic research facilities was predictable and swift. The Association of American Universities, the American Physical Society, the American Association for the Advancement of Science, and the National Academy of Sciences all issued statements supporting the continued use of peer review in allocating resources for science and its facilities and decrying the effort to distort the budgetary process. (14,15) The admonitions of these organizations appear to have had little effect on universities motivated to lobby in their own self-interest.

Political Autonomy and Self-Regulation in University Research

In raising this issue for discussion, we would make this fundamental observation: one of the remarkable features of the United States' system of support for basic research is the fact that the political forces of our society have granted a considerable amount of autonomy to science for the planning of research and its support through public funds. The scientific community itself has been presumed to have the best collective capacity to judge its self-interest and the national interest. The management of basic research has--compared to many other elements of government support--been self-regulating. (16-18)

The framework of political autonomy that was put in place immediately after World War II, and that has continued to guide basic research over the last several decades, was based on a deliberate commitment by government to excellence, to merit and to peer review by scientists of proposals for research and the facilities needed for research. Those political decisions followed much debate over excellence and equity and over whether the scientific community should play the primary role in guiding basic research or whether research should be controlled from Washington. The science program of the agencies established after World War II--the expanded National Institutes of Health, the National Science Foundation, the Atomic Energy Commission, and a host of programs in other departments--all followed these tenets:

o Program stability, so that longer-term work could be undertaken.

- Program guidance by outsiders, not government officials alone.
- Promotion of research outside the government through grants and contracts to non-governmental institutions.
- Internal control of policies in colleges and universities by the institutions themselves.
- Public accountability: While assuring independence in research and its methodology and discretion in allocation, responsibility to the President and the Congress to maintain the proper relationship between science and other aspects of a democratic system. (19)

To be certain, over the years there has been much debate about excellence versus equity and about the quality of the peer review process and possible biases. Nonetheless, the above tenets have stood well against many tests and examinations.

The principal method of evaluating facility needs has been by advisory committee: either through interagency panels of government scientists; committees directly advisory to the government, such as the National Science Board; or panels of outside scientific experts assembled through organizations such as the National Academy of Sciences. Such panels have established frameworks wherein budgetary decisions could be made by public leaders and the Executive and Legislative Branches have utilized them consistently.

We contend that this compact between the political and scientific communities has evolved into what could be called the "traditional process" for setting priorities within disciplines and for allocating funds for research facilities. The acceptance and use of this process by the political and scientific communities has extended now over three decades. Seven steps characteristic of this traditional process can be identified:

1. The scientific community, within a discipline, consults among members and is consulted.
2. The research funding agencies consider programs and priorities recommended by the science community.

3. The President's budget office reviews agency proposals, preparing a consolidation of requests in research as in other budgetary functions.

4. The Congress, in its appropriation and authorization committees, approves funding levels and programs, including "new starts."

5. Specific competition within the scientific community for funds, including facility funds, begins.

6. Individual proposals are peer reviewed.

7. Funds are awarded.

This general framework of self-regulation and public management has been used for at least three different but important types of research facility support programs: individual university facilities associated with specific areas of research; institutional development grants to upgrade academic research capacity; and national research facilities, particularly in physics and astronomy.

Earlier Facility Support and Lobbying

To see in starker relief the differences between the current research facility lobbying efforts and previous research facility decisions, and the way they proceeded, it is useful to consider a few examples of the latter. Federal funding of the materials research centers is one long-standing disciplinary support program. The university materials research centers program arose in 1960 in the Department of Defense (DOD) Advanced Research Projects Agency (ARPA). ARPA initiated an Interdisciplinary Laboratory (IDL) concept, in response to both scientific community and governmental beliefs that materials sciences were at new research frontiers and could benefit from expanded support. Forty-five universities submitted proposals to establish IDL's and 12 universities were selected. A key feature allowed the universities to make capital investments for buildings, to be paid back over 10 years. The program was transferred to the NSF in 1972, but continues, with NSF providing both core funding and individual investigator support at the materials centers. Four new centers were later added by NSF. The programs have been peer reviewed regularly since inception, sometimes causing concern on the part of grantees who received critical reports.

Other disciplines have also benefitted from the process of self-assessment and subsequent appropriations for research and facilities. The fields of chemistry and physics have relied heavily on directions provided by reports of the National Academy of Sciences--the Westheimer report in chemistry in 1965 and the Pake report, Physics: Survey and Outlook of the following year. (20,21) There was a successor report in physics, the Bromley report, (22) and in both chemistry and physics new National Research Council surveys are in progress. A reexamination of any one of these reports in light of the recent pork barrel phenomenon is instructive. Whatever their shortcomings, the surveys were, at the moment of their completion, comprehensive assessments of opportunities and needs within their disciplines, including facilities and instrumentation. In very real ways they helped to guide decisionmaking on resource allocation in those disciplines.

The government's largest facility support programs, the institutional development programs of the 1960s, also followed established advisory and review procedures. The President's Science Advisory Committee (PSAC), in its 1960 report, presaged the centers of excellence program that followed several years later by calling for additional university research centers but noting that excellence and reliance on the advisory judgment of respected scientists must guide choices. President Johnson's 1965 policy directive "Strengthening Academic Capability for Science Throughout the Country" derived from this PSAC report and from subsequent discussions by the National Science Board and the Federal Council for Science and Technology. (23,24) The 1966 program that followed provided funds through several agencies and transformed many campuses and disciplines. In all instances the competition followed scientific and governmental deliberation, proposals were reviewed, and many were declined in favor of those selected.

Two fields of science--high energy physics and astronomy--have had unusual facility requirements: the need for costly large-scale facilities serving large numbers of investigators. The notion of national facilities--scientific time-sharing on large instruments owned by the government and operated by consortia of institutions--was a novel idea thirty years ago. The scientific community edged itself toward collective utilization of such facilities by way of deliberative processes. The Energy Department's High Energy Physics Advisory Panel and the Pake and the Bromley Committees of the Academy argued out national facility needs in physics. The reports of these groups guided appropriations processes. A similar pattern has prevailed in astronomy with the Greenstein report and more recently the Field report of the National Academy of Sciences and National Research Council providing a framework for subsequent budget actions. The current budget request for the Very Long Baseline Array was the highest priority recommendation of the Field report. (25)

Of course, there have also been instances in which distributive political considerations--local or regional economic benefits, construction funds, jobs and the like--have influenced location decisions for research facilities. Among the examples that come to mind are the seven Federal Water Quality Administration laboratories established in the 1960s and later transferred to EPA, whose locations (Ada, Oklahoma; Duluth, Minnesota; Corvallis, Oregon, etc.) were widely felt to correspond more to Congressional districts of key members of the relevant committees than to aspects of the water problem. The Earth Resources Observation Satellite (EROS) data center, a U.S. Geological Survey facility, sought by a number of states once the decision to fund it had been made, was located in South Dakota at the behest of then Senator Karl Mundt.

Some oceanographic facilities in the Sea Grant Program and oceanographic ship awards appear to have been linked to congressional backing. Political disputes have surrounded decisions about where to locate many other research facilities including the Solar Energy Research Institute (Golden, Colorado), the Regional Solar Energy Centers, Fermilab (Batavia, Illinois) and, more recently, the Continuous Electron Beam Accelerator Facility (Newport News, Virginia). (26,27)

Congress, furthermore, has earmarked funds for research and facilities at specific institutions from time to time. The DOE national laboratories, notably Oak Ridge, have been adept at manipulating the system to their advantage for such purposes. (28) And Schlossberg-Cassidy and Associates, the same firm that was involved in last year's pork barrel incidents, is reported to have steered a variety of multi-million dollar contracts to Tufts University using Congressional agency contacts. (9)

All of these instances--and the several formula-based research programs, such as those of the Department of Agriculture, which distribute funds widely across the country--share some features in common with the pork barrel incidents under discussion. The pork barrel incidents, however, are different in some important ways:

- In most prior instances, the decision to build a facility or conduct a program had already been made and the politics involved choosing among alternative locations.
- Seldom, if ever, did the previous proposals emerge through floor amendments, without even committee discussion in Congress.
- Never before, to the best of our recollection, had such a spate of similar incidents involving universities occurred during a single Congress.

There is a tendency among members of the scientific community to regard this pork barrel phenomenon as a temporary aberration of a process which is basically sound. Although, as noted earlier, organizations such as the NAS and the AAAS have taken stands opposing the academic pork barrel, few concrete actions have been proposed and the general posture seems to be one of waiting and hoping the whole thing will blow over. In addition, one hears murmurs suggesting that the scientific community should avoid unnecessary discussion of the phenomenon in order not to give credence to external critics of science. This approach has been advocated from time to time in the past in regard to such other touchy matters as the fairness of peer review and the welfare of laboratory animals.

In the present case we believe the scientific community faces some real risks in allowing the problem to go unexamined. Lobbying for facilities may well not go away by itself. In fact, it may get worse and have some rather profound effects on the conduct of academic research and the well-being of the scientific community. It seems quite possible that the pork barrel is really a symptom of more deeply-rooted problems in the allocation of resources for research and research facilities.

The evidence that a serious problem exists in academic research facilities is strong and has been the subject of discussion and debate in the academic and science policy community for several years. Smith and Karlesky called attention to the problem of deteriorating facilities and obsolete equipment in 1977 in their book, The State of Academic Science. They also noted the sharp decline in capital investment in R&D facilities at universities, due in part of reductions in direct federal support and "unrealistic allowances for universities in indirect cost recovery for use of capital facilities." (29) According to NSF figures, total capital expenditures for R&D at universities and colleges fell by more than 46 percent in constant dollars between FY 1972 and FY 1981; the federal share dropped by 67 percent--from $237 million in FY 1972 to $79 million (in constant FY 1972 dollars) in FY 1981. (30-32)

In 1980, a study by the Association of American Universities (AAU) found that,

> Many research facilities are in need of renovation or replacement . . . Signs that aging facilities now impede research were found in many laboratories and support facilities. (33)

A follow-up report by AAU a year later noted that,

> a substantial backlog of research facilities and equipment needs is accumulating. Projected funding needs over the next three years equal almost twice the level of expenditures by universities for their research laboratories and equipment over the past four years. (34)

The federal government has responded to this situation by initiating dedicated programs for research instrumentation in NSF, DOD and other agencies, but the amounts of money involved have been small relative to the apparent size of the problem, and the programs have addressed instrumentation, but not R&D facilities. Some observers have suggested that the pork barrel phenomenon is a measure of the severity of the facilities problem and the despair of academic administrators for finding funds for new facilities.

Whether or not one accepts the notion that it is symptom of deeper problems, we would argue that the scientific community needs to take seriously the pork barrel phenomenon. It should initiate a broad-based examination of the alternative courses of action open to it, debate and discuss their pros and cons, and advise the nation's political and academic leaders on what needs to be done.

Possible Courses of Action

We see a variety of possible courses of action, ranging from attempts to restore the status quo ante (i.e., to suppress the pork barrel without making other changes in the system) to formally embracing the pork barrel process, or at least allowing it to grow unhindered. Each has advantages and disadvantages.

Return to Status Quo Ante

This approach would involve simply regarding the pork barrel as the problem and attempting, through legislation, political pressure, or other means, to halt the practice while not making any other changes in the system through which funds for academic research facilities are allocated. The existing system, on which we would continue to rely under this approach, is one which has no dedicated academic research facility programs and has not had any for over a decade. Universities obtain funds for research facilities on an ad hoc basis from federal agencies (which occasionally provide support in instances of demonstrated need), from philanthropic sources, from state governments, and, to a small but increasing extent, from industrial sources.

Returning to the pre-pork barrel system has several things to recommend it. Apart from whatever political actions that are necessary to suppress the pork barrel, this is a minimal change strategy. It is, furthermore, consistent with the current Administration's political philosophy: it is fiscally conservative and it avoids the establishment of new programs or bureaucratic entities, instead basing specific facility decisions and appropriations on demonstrated scientific needs. Some would argue, finally, that the system has worked reasonably well in recent years and deserves to be perpetuated.

On the negative side, strong arguments can be made that the system has not worked very well. As noted above, it has been suggested that the pork barrel phenomenon itself is evidence of the failure of the existing system and of the fact that the need for new facilities is far larger than the process has recognized. Apart from this, one might argue that the existing system may not be attuned to new opportunities in R&D, where new facilities may help us maintain or improve our international economic competitiveness. Finally, this approach simply may not work. The pork barrel temptations may just be too strong to suppress if no alternatives are offered.

New Facilities Support Program

Some scientists and administrators and some public officials have suggested establishment of a new targetted support program (or programs) through which one or more federal agencies would provide universities with funding specifically for facilities. The House Armed Services Committee and the House Science and Technology Committee have requested that DOD and NSF study academic research facility needs, and make recommendations. (35,36) The "University Research Capacity Restoration Act" (S.1537) introduced in the last session by Senators Danforth and Eagleton adds a somewhat different wrinkle in spreading the burden among the major research-supporting agencies.

As a means of addressing the problem that presumably has led to the academic pork barrel, this approach has several points to recommend it: A new facilities support program would create an orderly administrative process through which informed decision-making could proceed, yielding an effective allocation of resources. It is a funding mechanism which has been previously tested (in programs during the 1950s and 1960s) and is known to work. As an approach that employs a direct budget mechanism, it has the virtue of being straightforward and conceptually simple. As a targetted program, it could be terminated when real needs have been addressed.

On the other hand, the notion has several drawbacks: It represents a new and potentially costly addition to the federal budget for R&D. A dedicated facilities program might lead to overexpansion of academic research facilities beyond what is warranted by anticipated funding for conduct of R&D. For all of the popularity and perceived effectiveness of such programs in the academic community in past years, the actual literature giving evidence of the success or effectiveness of targetted facilities programs is rather slim. (37)

New Funding Mechanisms

A third approach is the initiation of new funding mechanisms not generally employed in academic research. We might suggest, for example, a set-aside of a certain percentage of each year's appropriations for the conduct of R&D which would be allocated by states or other non-federal bodies for academic facilities. This could be done either with or without peer review. The state program of the National Endowment for the Arts, which employs peer review, is one possible model.

The approach has several potential virtues. Most important is its potential for equity--i.e., redressing perceived or real biases in the existing peer review system which are felt to favor the top-ranked institutions and to place second-tier (but still very capable) universities at a disadvantage. In being more equitable, such a funding system might also be more efficient and might share control over decision-making with those closest to the needs of the affected universities, reducing federal involvement in academic affairs.

A formula or set-aside funding mechanism would also have some liabilities. First, in providing for equity it would reduce the role of excellence as the primary criterion for support and thus, possibly, decrease the effectiveness of federal research. Similarly, it may make the research system more parochial and less responsive to national needs and priorities. Finally, it might set a bad precedent by demonstrating that the scientific community is willing to use other ways, aside from traditional peer review, for distributing R&D funds. This could increase the vulnerability of academic research to budget cuts since formula funds are more likely to be viewed as federal aid, serving local interests, rather than federal support for an important activity conducted in the national interest.

New Financing and Expensing Mechanisms

As a means of diminishing the incentive for pork barrel activity, policymakers might also consider a variety of non-budgetary mechanisms for assisting universities in generating support for research facilities. Such mechanisms include modifying indirect cost guidelines to allow expensing of interest on loans for facilities on federal research grants and encouraging the use of facilities shared among several institutions. (30,31) Among the advantages of this approach are, first, the expenditures, while real, do not show up as dedicated budget items. Second, the mechanisms are consistent with the philosophy of the current Administration, in that they employ the market system and allow for local initiative. The disadvantages include the fact that changing federal indirect cost guidelines may prove problematical and could produce budget impacts of unknown magnitude. Furthermore, this approach by itself may simply not produce enough results, quickly enough to solve the research facility problem.

Pork Barrel

There is yet another course of action (or perhaps inaction) available to the scientific community. This is to allow the academic pork barrel to grow unhindered, or even to embrace it formally in some manner. It is worth examining the pros and cons of such an approach for they may suggest possible consequences of current trends.

There are, in fact, several positive aspects to distribution of academic research facilities via the pork barrel system. Some would say that the pork barrel system rewards true entrepreneurship--the industrious and creative pursuit of resources by any institution, not just the privileged few. It is furthermore, a time-honored means of gaining resources if all else fails, a proper court of last resort, particularly in the eyes of the "have-nots" who have not succeeded by other means. Political influence is, in this view, a legitimate way for "outsiders" (individuals and institutions) to gain funding.

The other side of the coin has been much discussed. Most prominent is the fear among many in the scientific community that a system based on political influence rather than peer review will not produce the best science in the national interest. To use some current examples, one might ask what basis is there to expect that the expenditure of $160 million on the Georgetown University fuel cell demonstration project will yield more useful results than if the money were spent by the Army Research Office on peer-reviewed projects in areas of its choosing? Or who is to say that a vitreous state laboratory at Catholic University (unexamined by the conventional processes of review and consultation) will produce better science than would be obtained by spending the several million dollars on other DOE-funded academic programs?

The effectiveness of pork barrel projects is a valid concern. A somewhat neglected side of the pork barrel question, however, is the prospect of turning academic research funding into a free-for-all akin, say, to federal water projects. This is a very serious matter. To the extent that the public--and especially the Congress--begins to see academic research and the supporting facilities as a matter of distributing resources among essentially private interests rather than supporting a nationwide activity in the public interest, the current broad base of political support will start to erode.

Conclusion

The peer review system of allocating resources for federal support of research and research facilities has operated remarkably well for nearly four decades and has gained a considerable amount of autonomy for the scientific community. A number of circumstances, including a "high tech bandwagon" have combined to generate an unprecedented high level of political support for basic research during the current period. Ironically, as this support has grown, it has created pressures which may lead to the weakening of the system by undermining the basis of public support. By allowing a pork barrel system to develop, scientists run the risk of becoming another vested interest group. Scientists would join the ranks of such claimants on federal resources as the highway construction lobby, the water project lobby, and these who pursue such other federal prizes as post offices and veterans' hospitals. The spectre is chilling.

REFERENCES AND NOTES

1. "Rheum and Board: AAAS Chiefs Speak Out," *Science and Government Report* 14, (January 15, 1984), p.8.

2. "Perturbed Peers," *New York Times* (May 30, 1984), p. A20.

3. C. Norman, *Science* 224, 1076 (1984).

4. Intersociety Working Group, *AAAS Report IX: Research and Development, FY 1985* (AAAS, Washington, D.C., 1984).

5. *Congressional Record* (May 12, 1983), p. H-2905.

6. *Congressional Record* (May 12, 1983), p. H-2911.

7. *Congressional Record* (June 10, 1983), pp. S-8173-8174.

8. *Congressional Record* (October 4, 1983), pp. 13505, 13543.

9. C. Norman, *Science* 222, 1211-1213 (1983).

10. A. L. Robinson, *Science* 222, 308-309 (1983).

11. U.S. House of Representatives, Committee on Science and Technology Department of Energy Civilian Research and Development Authorization Act for FY 1984, Report 98-81 (U.S. GOvernment Printing Office, Washington, D.C., 1983).

12. U.S. House of Representatives, Committee on Appropriations, Making Appropriations for Energy and Water Development, Conference Report to Accompany H.R. 3132, Report 98-272 (U.S. Government Printing Office, Washington, D.C. 1983), pp. 42, 53.

13. U.S. Department of Energy, *Congressional Budget Request, FY 1985*, Volume 3 (U.S. Department of Energy, Washington, D.C. February 1984), pp. 154-155.

14. C. Norman, *Science* 222, 592 (1983).

15. "AAAS Board Statement on Politics and Science," *Science* 223, 27 (1984).

16. D. K. Price, *The Scientific Estate* (Belknap Press, Cambridge, Mass., 1965).

17. H. Brooks, *The Government of Science* (MIT Press, Cambridge, Mass., 1968).

18. E. Shils, ed., *Criteria for Scientific Development* (MIT Press, Cambridge, Mass., 1968).

19. V. Bush, *Science: The Endless Frontier* (U.S. Government Printing Office, Washington, D.C., 1945; republished by National Science Foundation, 1980).

20. Committee on Science and Public Policy, National Academy of Sciences, *Chemistry: Opportunities and Needs* (Printing and Publishing Office, National Academy of Sciences, Washington, D.C., 1965).

21. Committee on Science and Public Policy, National Academy of Sciences, *Physics: Survey and Outlook* (Printing and Publishing Office, National Academy of Sciences, Washington, D.C., 1966).

22. Committee on Science and Public Policy, National Academy of Sciences, *Physics in Perspective* (Printing and Publishing Office, National Academy of Sciences, Washington, D.C., 1972).

23. U.S. President's Science Advisory Committee, *Scientific Progress, the Universities and the Federal Government* (The White House, Washington, D.C. 1960).

24. U.S. National Science Foundation, *Sixteenth Annual Report to the Congress* (U.S. Government Printing Office, Washington, D.C., 1966), p. ix.

25. Assembly of Mathematical and Physical Sciences, National Research Council, *Astronomy and Astrophysics for the 1980's* (National Academy Press, Washington, D.C., 1982).

26. E. Marshall, *Science* 220, 929-932 (1983).

27. D. S. Greenberg, *The Politics of Pure Science* (New American Library, New York, 1967) provides a lengthy discussion of earlier instances of political infighting over R&D facilities.

28. A. H. Teich and W. H. Lambright, *Minerva* 14, 447-474 (1977).

29. B. L. R. Smith and J. J. Karlesky, *The State of Academic Science* (Change Magazine Press, New York, 1977), p. 175.

30. U.S. National Science Foundation, *Academic Science 1972-81* (U.S. Government Printing Office, Washington, D.C., 1982), Table B-12.

31. U.S. National Science Foundation, Summary Statistics on Academic Science/Engineering Resources, unpublished, March 1983, Table 4.

32. U.S. National Science Foundation, National Patterns of Science and Resources, 1983 (U.S. Government Printing Office, Washington, D.C., 1983).

33. Association of American Universities, The Scientific Instrumentation Needs of Research Universities (Association of American Universities, Washington, D.C., June 1980), p. x.

34. Association of American Universities, The Nation's Deteriorating University Research Facilities (Association of American Universities, Washington, D.C., July 1981), p. i.

35. U.S. House of Representatives, Committee on Armed Services, Report on the DOD Authorization Act, 1984, H.R. 2969 (U.S. Government Printing Office, Washington, D.C., 1983.

36. U.S. House of Representatives, Committee on Science and Technology, Report on the NSF Authorization, 1984, H.R. ____ (U.S. Government Printing Office, Washington, D.C., 1983).

37. National Board on Graduate Education, Science Development, University Development, and the Federal Government (Printing and Publishing Office, National Academy of Sciences, Washington, D.C., 1975). This report is based in part on D. Drew, Science Development: An Evaluation Study, a technical report presented to the National Board on Graduate Education (Printing and Publishing Office, National Academy of Sciences, Washington, D.C., 1975).

How to Win Buildings and Influence Congress

A lobbying firm raises university construction funds and a storm of criticism through pork barrel politics

For the past 6 months, the Washington lobbying firm of Schlossberg-Cassidy and Associates has been at the center of a storm. In May, it pulled off a coup by securing funds for research facilities at Catholic and Columbia universities through an amendment first proposed on the floor of the House of Representatives. The move immediately brought a gush of criticism because it was pure pork barrel politics that bypassed all the peer review processes that are supposed to guide the funding of scientific projects. More recently, it has prompted a clutch of high-minded resolutions from academic organizations urging that such unorthodox funding mechanisms not be used.

The adverse publicity has not been bad for business, however. Schlossberg-Cassidy has signed up almost a dozen universities as clients, and several more have inquired about its services.* Academic work now accounts for about half the firm's business.

Moreover, Schlossberg-Cassidy, undaunted, has since masterminded two other attempts to win funding for its clients by similar end runs around the usual departmental and congressional review processes. The first secured a $7.5-million grant for Boston College for a new library and the second narrowly missed getting $15 million for Boston University for an engineering facility. Both projects first appeared in amendments proposed on the floor of the Senate, which approved them without debate. The House went along with the Boston College amendment, but in an unusual display of budgetary scruples, refused to support the Boston University funds.

It is not just Schlossberg-Cassidy's clients that have secured academic facilities recently through pork barrel politics. The amendment providing the Boston College grant also channeled $20.4 million to Oregon Health Sciences University for an information center and $15 million to the University of New Hampshire for a space and marine science building. And the Boston University amendment also contained $20 million for the University of New Mexico for an engineering center. Although these schools were not represented by Schlossberg-Cassidy, they were part of political coalitions that the firm helped put together to benefit its clients.

---

*Schlossberg-Cassidy's academic clients include Tufts University, Columbia University, Boston University, Boston College, Indiana University, Northwestern University, Atlanta University, Rensselaer Polytechnic Institute, Catholic University, the Oceanic Institute, and the Pratt Institute.

Pork barrel politics is no stranger in the world of academic science, but this rash of episodes was more than the scientific and academic establishment could stomach. In the past few weeks, the National Academy of Sciences, the Association of American Universities (AAU), and the National Association of State Universities and Land-Grant Colleges have all approved resolutions urging the universities and Congress not to bypass peer review in parceling out funds for scientific projects and facilities. The American Physical Society has written to every member of Congress asking for an end to sweetheart deals for specific universities. The Council of Scientific Society Presidents is thinking of doing the same thing, and the AAAS plans to take up the issue as well.

One outcome of all this brouhaha is that political attention is finally being focused on the problems universities face in raising funds for facilities. Federal programs to finance university construction, which were launched in the post-Sputnik panic, all dried up at least a decade ago. And private financing is not easy for less well-endowed institutions to raise.

"It ill behooves those who are rich in research resources to tell others, who are not, that they should not pursue the only legal, ethical route open to them to meet their pressing needs," says Kenneth Schlossberg. His associate, Gerald S. J. Cassidy, adds that given the shortage of funds for academic facilities, their distribution is bound to be political. "I don't believe members of Congress are going to sit by and watch that need be unfulfilled [in their districts] because of some ideal people have regarding this. It is not a research question at all; it is an economic question," he contends.

Although they have only recently gained notoriety for their efforts, Schlossberg and Cassidy have long been working the Washington scene for university clients. Schlossberg, a former journalist, and Cassidy, a lawyer, were, respectively, the staff director and chief counsel of the Senate Select Committee on Nutrition and Human Needs from 1969 to 1975 when they set up shop. They were joined by James Fabiani, who had been on the Republican staff of the House Appropriations Committee. (Two more professional staff members have since been added.)

One of their early clients was Tufts University, whose president, former Harvard nutritionist Jean Mayer, got to know Schlossberg when he worked for the select committee. Schlossberg says Mayer called him up out of the blue and asked whether he could do anything for Tufts.

The first thing Schlossberg-Cassidy did for Tufts was to secure a $32-million contract to set up a center at the university for research on nutrition and aging. Schlossberg, using old contacts at the

Department of Agriculture, floated a proposal for the center which the department—in large measure because of Mayer's reputation and expertise in the area—included as a line item in its budget request to Congress. The budget request specifically named Tufts as the location of the center, and Congress gave the venture its blessing.

Schlossberg-Cassidy has since helped steer $10 million in federal funds to Tufts to establish a veterinary school and $7.5 million for an intercultural center. And, thanks in part to initial work by Schlossberg-Cassidy, Tufts is about to get a $2-million grant from the Environmental Protection Agency (EPA) for a center on toxic waste research. In each case, Tufts was the beneficiary of bills or amendments in Congress that channeled funds to the university by name, or spelled out criteria that virtually no other university could meet.

Most of the firm's work for universities consists of advising on the establishment of government relations programs, keeping clients informed of legislative and regulatory developments that may affect them, shepherding administrators and researchers around Washington, planning fund-raising drives, and sniffing out areas likely to produce funds. The Tufts toxic waste center, for example, had its roots in advice from Schlossberg-Cassidy a few years ago that funds for research on toxics would probably be a growth area. The university put together a proposal and sold the idea itself to key members of the House Appropriations Committee. A directive to EPA to establish a center at "a university with schools of biomedical sciences, engineering, nutrition, and veterinary medicine as well as proven programs in urban and environmental policy" appeared in a conference committee report without having first been approved by either the House or Senate. The description was tailored for Tufts, and no other institution has submitted a proposal.

Schlossberg and Cassidy might have gone on quietly working deals for their clients without raising much of a fuss were it not for the Catholic and Columbia amendments. Catholic won a $5-million grant for the first installment on a $14-million vitreous state laboratory, and Columbia secured a $5-million downpayment on a $20-million National Chemical Research Center. Both amendments were greased with some slick lobbying and easily slid through. In the case of the Catholic proposal, for example, several bishops on Catholic's board of trustees called their members of Congress, and House Speaker Thomas P. O'Neill's support was enlisted through an approach by Humberto Cardinal Medeiros of Boston. But what really rankled was that the funds were taken directly from somebody else's pocket.

"They risk undermining the whole basis of decision-making in research funding."

Both amendments were applied to a budget bill for the Department of Energy (DOE). In Catholic's case, the amendment diverted funds already approved by the House Committee on Science and Technology for the National Center for Advanced Materials (NCAM) at the Lawrence Berkeley Laboratory. Columbia's $5 million was cobbled together from a variety of sources, including construction funds for accelerator projects at Yale and the University of Washington, and support for instrumentation in high-energy physics. Not surprisingly, those who were cut complained loudly.

Schlossberg and Cassidy plead innocent to the charge of robbing others for their clients. They claim that although their firm helped plan the political strategy and conduct the lobbying, it did not draft the amendments. "We did not understand what had happened until we read the [*Congressional*] *Record* the next day, and we were very distressed by it," says Schlossberg. He blames staff members of the science and technology committee who, he believes, deliberately drafted the amendments to create maximum uproar, calling it a "shortsighted and unwisely vindictive thing to have done."

Whether by design or not, the moves drew instant opprobrium. In contrast, Schlossberg-Cassidy's later forays into the budget process on behalf of Boston College and Boston University initially attracted little attention. One reason is that nobody else got hurt in the process.

The Boston College deal illustrates the old Washington lobbying tactic of aligning your client's interests with those of more powerful political constituencies. Schlossberg and Cassidy say they were aware that Oregon was interested in obtaining funds for a regional medical library and information complex, and that the University of New Hampshire had been talking with Senator Warren Rudman (R–N.H.) about its needs for a new science building. A combined amendment was cooked up providing funds from the Department of Education for all three institutions. The Senate approved it without debate, and the House eventually followed suit.

The coalition had several powerful features: the amendment had the backing of Senator Mark Hatfield (R–Ore.), the chairman of the Senate Appropriations Committee, and Speaker O'Neill could be counted on to lend his support when it went into a conference committee with the House. O'Neill is a graduate of Boston College.

In attempting to get funds for Boston University, a coalition was formed between Senator Edward Kennedy (D–Mass.) and Senator Pete Domenici (R–N.M.), the chairman of the Senate Budget Committee, who was interested in channeling funds to the University of New Mexico for a new engineering center. According to an aide to Domenici, the Senator was persuaded to support a pork barrel amendment because a recent study had concluded that if Albuquerque is to compete effectively with other cities for high-technology industry, the University of New Mexico's engineering school needs to be beefed up.

An amendment providing funds for the two schools sailed easily through the Senate but failed to win acceptance by a House-Senate conference committee. The committee did, however, recommend that the universities submit grant proposals to the Department of Education and directed the department to give them a high priority. There is thus a good chance that they will get funds next year.

The Academy and the AAU have reacted to all this by passing resolutions noting that funding for scientific projects and facilities in the United States has traditionally been based on peer review by the scientific community and that this system has served U.S. science well. Both statements called on universities and Congress not to undermine the system through pork barrel politics.

Others worry that, by participating in pork barrel politics, universities could undermine efforts to head off further political control over research decisions—such as legislation proposed by Representative Henry Waxman (D–Calif.) to increase Congress's influence over the National Institutes of Health (NIH). "It bothered me when AAU institutions started doing this—institutions that have benefited enormously from the peer review system," says Albert Bowker, dean of the school of public affairs at the University of Maryland.

Schlossberg and Cassidy respond by pointing out that decisions on funding for science have never been free of politics, citing in particular the recent scrap between a coalition of southern universities and the Argonne National Laboratory over the siting of an accelerator (*Science*, 27 May, p. 929), and the Administration's decision to seek funds for NCAM without first bothering to consult the research community on the need for such a facility. Moreover, they note that many universities already maintain large government relations staffs to influence political decisions.

Schlossberg also says he believes there is some "confusion" about what his firm has been doing, pointing out that it has been helping universities acquire funding for buildings, not for research grants or major facilities that should be put through a peer review process. The distinction between buildings and research is not always clear, however. The Catholic and Columbia buildings, for example, will require another $24 million before they are completed, and the funds presumably will have to come from DOE's research budget. The Tufts toxic waste center, which could eventually be a $10-million-a-year operation, will also take a bite out of EPA's research budget. The Tufts funds "will not be part of a competitive peer-reviewed process, and in my view that sets a poor example," says Courtney Riordan, EPA's research chief.

Finally, Schlossberg and Cassidy argue that the AAU, which represents 50 of the largest research universities in the country, reflects the views of those that are already comparatively well off. "It is somewhat hypocritical for universities like Stanford, Harvard, and Yale, to be advising the rest of the university community . . . not to seek support where they can find it, including from the U.S. Congress," says Schlossberg.

One reason why so many universities have sought special interest amendments in Congress is that the approach evidently works. But a deeper reason is that there are no longer any programs to which they can apply for building funds. The Department of Education and the National Science Foundation both ran out of funds for facilities in the early 1970's, and an NIH program for construction of biomedical facilities expired in the late 1960's.

The AAU earlier this year drafted a bill that would provide funds for construction, equipment, and graduate fellowships. It was introduced into the Senate by John Danforth (R–Mo.) and Thomas Eagleton (D–Mo.) but did not get very far. The AAU intends to push it harder next year.

Schlossberg, however, accuses the AAU of "standing still or being very ineffective in whatever efforts they have made to increase funding for facilities." He adds: "If the AAU would like to hire my firm's resources and use our professional expertise to obtain general funding [for facilities] we would be absolutely delighted." Robert Rosenzweig, AAU's president, responds that "we would welcome their help on that, but we don't welcome their shooting for individual universities. . . . They risk undermining the whole basis of decision-making in research funding."—**Colin Norman**

Colleges Lobby Congress for Facility Funds . . .

More than a decade after the federal government got out of the business of investing heavily in bricks and mortar on college campuses, a growing number of universities are now going directly to Congress for federal construction aid.

The 98th Congress saw a spate of efforts to secure funding for the construction or renovation of research centers and other campus facilities.

For example, the fiscal 1985 appropriations bill (HR 5653 — PL 98-360) for the Department of Energy included almost $12 million for research laboratories at Catholic and Columbia universities. *(Weekly Report p. 1654)*

Some $16 million was provided for Northwestern University for an industrial research facility under the catchall continuing appropriations bill for fiscal 1985 (H J Res 648 — PL 98-473). And $19 million for an engineering and technical training center at Boston University was included in a fiscal 1984 supplemental appropriations bill (HR 6040 — PL 98-396). *(Weekly Report pp. 2732, [illegible])*

Critics charge that such efforts are a form of academic "pork-barrel" politics that allows some universities to receive funds without going through the traditional procedures of submitting plans for review by academic experts and competition with other institutions.

Particularly in the construction of scientific facilities, the practice has been seen as undermining the "peer review" system that is typically used in the allocation of federal research money. Under peer review, funding proposals are submitted to agencies for evaluation by panels of academics or in-house experts.

"Institutions are increasingly persuaded that political maneuvering is more important than scientific justification in securing federal funds," said Mildred S. Dresselhaus, president of the American Physical Society, in an August letter to members of Congress.

But those seeking funds say it is a legitimate use of the political process. Some charge that the peer review system itself tends to skew federal money to a handful of elite universities.

John Silber, president of Boston University, a $19 million winner this year, said complaints about direct congressional funding of university projects represented the "squeal of the old-boy network as it finds that the establishment is being expanded behind its back."

The controversy has divided the academic community, as entrepreneurial institutions have ignored pleas from scientific societies and university groups to refrain from what critics call an end run on the peer review process.

While those pleas also were directed at lawmakers, there were few signs that the practice was abating in the 98th Congress. *(Pet projects, Weekly Report p. 2708)*

But in an apparent sign of some members' frustration with having to contend with the clamor for special appropriations, Congress this year provided funds for the first time since 1973 for competitive grants for university facilities construction under a measure providing fiscal 1985 appropriations for the Education Department (HR 6028 — PL 98-619). *(Weekly Report p. 2746)*

The Education Department construction program has been in existence since 1963, but the only funds appropriated under it for the last 10 years have been for congressionally designated recipients.

The $28 million in construction money in fiscal 1985 may help universities that lack the desire or political clout to win special appropriations. But it is a small pot of money to be divided among universities, compared with the amounts lawmakers have managed to earmark without competition for home-state projects.

Indeed, the same Education Department appropriations bill also included $22 million designated for construction projects at four institutions, universities in Connecticut, Kansas, Georgia and Massachusetts.

Time-Honored Practice

The practice of channeling funds to home-state universities is not new. Agriculture appropriations bills, for example, typically are replete with research money for specific institutions or to address narrowly defined local problems.

"Congress likes the principle [of peer review], but they also like to deliver money to the home district," said Dale Stansbury, director of food and agriculture for the National Association of State Universities and Land Grant Colleges.

Although the federal government invested heavily in building college research facilities during the 1960s, that investment essentially ended in the early 1970s. With federal money hard to come by, universities have become increasingly aggressive in pursuing funds from Congress, with some turning to professional lobbyists for help.

Kenneth Schlossberg, who as a member of Schlossberg-Cassidy and Associates helped several universities obtain aid from Congress, said the lobbying firm took on its first university client in 1977. Between 1981 and 1984, the firm's university clients increased to a dozen, said Schlossberg, who is now setting up his own firm that primarily will represent universities.

"... One man's peer review is another man's good-old-boy network."

—Rep. Ronnie G. Flippo, D-Ala.

[illegible] 1984 Congressional Quarterly Inc
[illegible]

. . . Avoid Academic 'Peer Review' Procedures

Schlossberg said there is increased interest on Capitol Hill among members as they see university projects as a way to bolster the economy and create jobs.

That was part of the strategy used successfully this year by Boston University, a client of Schlossberg-Cassidy, when it secured funding for its engineering center. The project was pitched to Congress as a way to revitalize economically the Boston area while addressing the nationwide shortage of engineers.

Defending his institution's direct appeal to Congress, Silber of Boston University said congressional decisions about funding for university facilities, unlike basic research, are sometimes based on "factors [that] are not open to competition."

"If they want to develop Kenmore Square," said Silber, referring to a Boston neighborhood adjacent to the university, "there is only one institution in Boston to help."

The university's effort this year was its second pass at direct congressional support. Sen. Edward M. Kennedy, D-Mass., last year helped persuade the Senate to add $20 million for it to the fiscal 1984 Education Department appropriations bill, under an amendment that packaged it with a University of New Mexico project backed by Sen. Pete V. Domenici, R-N.M.

However, the amendment did not survive in conference with the House, even though Boston University is located in the district of House Speaker Thomas P. O'Neill Jr., D-Mass.

A key figure in the conference — William H. Natcher, D-Ky., chairman of the House education appropriations subcommittee — is a particularly ardent opponent of earmarking funds in appropriations bills and has not been shy about scotching even those projects with powerful backers.

But Boston University found another opportunity this year through a Commerce Department program not overseen by Natcher's subcommittee.

The fiscal 1984 supplemental bill came to the Senate floor with some $7 million in economic development funds to establish the Institute for Technology Development in Mississippi, money sought by Sen. John C. Stennis, D-Miss., ranking minority member of the Appropriations Committee.

During floor debate, the Senate added a Kennedy amendment adding funds for the Boston University center under the same Commerce Department program. This time, the proposal stuck in conference with the House.

Critics Take Aim

The strongest criticism of the direct congressional funding has been over those facilities for scientific research.

There has been a barrage of resolutions in the last year calling for firm adherence to the peer review system by scientific societies and university groups including the land-grant university group, the American Physical Society and the Association of American Universities (AAU), an organization of 52 major research universities.

While acknowledging that the award of federal funds to universities has never been totally devoid of political influence, AAU President Robert M. Rosenzweig said, "Science has been, on the whole, outside the logrolling network — for good reasons.

"I think [members of Congress] have been persuaded that quality really counts in the conduct of science," said Rosenzweig.

Although most of these direct grants to universities have sailed through without debate, critics spoke up last year when Congress authorized the first installment of funding for the laboratories at Columbia and Catholic universities. They maintained the projects were inadequately planned and had not been reviewed by scientists and congressional committees.

Rep. F. James Sensenbrenner Jr., R-Wis., charged then that the practice would "politicize scientific research and the construction of scientific research facilities by encouraging every college and university in the country to contact their representative to obtain a facility."

This August, the administration joined the battle, supporting expert review in federal science funding in a letter sent to Rep. Don Fuqua, D-Fla., chairman of the House Science and Technology Committee, and other members of Congress.

The administration letter warned against awarding funds on "narrowly based political considerations."

Fuqua agreed that expert opinion should dominate distribution of funds for research, but said "additional and broader factors" had to be considered in weighing expenditures for major new facilities and policies that have a "direct impact beyond science."

Rep. Ronnie G. Flippo, D-Ala., supported Fuqua, arguing that a strict reliance on peer review might not give due weight to such factors as geography or the gains made by "middle-echelon" universities.

"We all support the concept of peer review when it is properly applicable," Flippo said. "At the same time, let me point out that one man's peer review is another man's good-old-boy network."

—By Janet Hook

"... Additional and broader factors" must be considered in providing funds for new facilities.

—Rep. Don Fuqua, D-Fla.

COPYRIGHT 1984 CONGRESSIONAL QUARTERLY INC

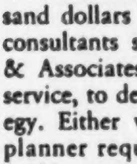
ROBERT C. COWEN

Scraping the Bottom of the Pork Barrel

AMERICAN universities are suffering a form of poverty that drives some of them to make desperate raids on the federal purse. For more than a decade, federal support for university research has fallen short on funds for new facilities. Now, with urgent needs for new laboratories, some university administrators are bypassing both the peer-review system and the Office of Management and Budget to seek funds directly from Congress.

Does the school need a new chemistry lab? Then have a friendly representative or senator shepherd legislation through Congress that "directs" a federal agency to grant the money. Or, better yet, dip into the endowment for several hundred thousand dollars and hire some Washington consultants such as Schlossberg-Cassidy & Associates, who have pioneered this service, to develop a total lobbying strategy. Either way, funds that no budget planner requested may materialize for projects not subject to independent, objective review.

Such is the academic pork barrel. Although only a few universities have resorted to relying on it so far, this novel scramble for funds threatens the integrity of support for U.S. research. That's why this technique aroused a storm of protest within the academic research establishment when it was first used last year. Indeed, the reaction was so strong as to engender hope of quashing the practice. But for fiscal 1985, the pork-barrel raiders have again brought home the bacon.



ROBERT C. COWEN IS SCIENCE EDITOR OF THE CHRISTIAN SCIENCE MONITOR AND FORMER PRESIDENT OF THE NATIONAL ASSOCIATION OF SCIENCE WRITERS.

By late summer, when Congress wrapped up its business and went campaigning, it had also wrapped up some $21 million of Department of Energy (DOE) grants for favored universities. Don Fuqua (D-Fla.), chairman of the House Committee on Science and Technology, saw to it that DOE was "directed" to award $7 million to Florida State University as a down payment on a supercomputer facility that DOE did not particularly want. The total cost for the facility will probably be around $55 million over 5 years, with DOE picking up 60 to 65 percent of the tab. DOE is also "directed" to give the University of Oregon $2.3 million for planning a science facility—a project Oregon's Republican Senator Mark Hatfield favors. Catholic University gets $8.9 million to finish a vitreous-state laboratory, for which it received an initial $5 million in last year's pork-barrel deal. And Columbia University—which also got $5 million last year—receives an additional $3 million to continue building new chemistry facilities with an estimated total cost of around $20 million.

> *Desperate for new facilities, some universities are subverting the traditional peer-review system.*

The redirection of DOE funds is

Nov./Dec '84

V. 87

financed partly by $16 million in cuts from DOE-supported research projects that had been reviewed and approved in open competition. This is exactly the sort of corruption that many academic research leaders fear.

For example, a National Academy of Sciences resolution insists: "Informed peer judgments on the scientific merits of specific proposals in open competition should be a central element in the awarding of all federal funds for science. We urge that the academic community and public officials exercise vigilance to protect this informed evaluation and decision process." Also, the American Association for the Advancement of Science warns that "failure to adhere both in principle and in practice to criteria of scientific choice at all times will serve only to diminish public confidence in the peer-review system as the basis for allocating national resources with serious negative consequences for the integrity and advancement of science." Even the National Association of State Universities and Land Grant Colleges has condemned the trend toward "an irrational system of distribution based solely on political influence," which Florida State's supercomputer deal represents.

Disrepair and Deterioration

Yet leaders of some prominent universities still feel compelled to subvert the system. This tells us something about the state of U.S. research funding in general. The neglect of facilities that has marked federal support for more than a decade is bringing university research to its knees.

Rep. Fuqua himself stated the problem well in hearings last May. "Many of our university laboratories," he said, "are in an alarming state of disrepair and deterioration. They do not provide suitable facilities for modern research. . . . The federal role must be considered . . . if our universities are to continue to produce at their full capacity."

Thus, some university administrators may feel themselves to be like the honest but jobless citizen who, in desperation, robs a store, to feed his family. They may well see a direct appeal to Congress as a lesser evil than having their competence in doing research eroded. And, as Kenneth Schlossberg of Schlossberg-Cassidy has candidly observed, if representatives or senators can help "in a way that isn't obnoxious to them—and even serves the public interest—they're delighted to."

The dangers of pork barrel funding are serious, but so are the problems of facilities-poor universities. The real need is for concerted action by the academic community and its congressional friends to prepare and implement a remedial program that preserves impartial expert review of project proposals. Merely preaching the gospel of competitive grantsmanship will do little good. The pressures that drive universities to bypass peer review will, if unrelieved, continue to produce an academic pork barrel. ☐

C. CONGRESS AND THE POLITICS OF GEOGRAPHICAL ALLOCATION

JOHN A. FEREJOHN

PORK BARREL POLITICS

Rivers and Harbors Legislation, 1947-1968

Stanford University Press, Stanford, California 1974

3

Political Benefits and Political Costs

If there is a widely accepted theory of democracy* it is probably that put forth by Schumpeter and Dahl,[1] which identifies as the key feature of democracy the competition among elites for votes in order to obtain or retain public office. Competition for office is presumed, in itself, to call forth policy proposals (or platforms) from the competitors that are capable of winning the popular test (obtaining a majority of votes). The requirement of competing for votes is supposed to instill in office-seekers accountability or responsibility to the voters. If incumbents wish to retain their positions, then they are bound to act in such a manner as to enable them to win reelection.

The actual accountability of an officeholder to his constituents rests on two factors: his desire to retain office and the presence of a genuine competitor for the office. Dahl and Lindblom maintain that this theory of democracy corresponds closely to reality.[2]

> The responsiveness of leaders in polyarchies is a good deal more complicated than a simple function of their expectation of votes in the next election. But there can be no doubt that this expectation keeps them highly re-

---

* The notion of a "theory of democracy" may be ambiguous. The authors of the theories discussed here intend a theory of democracy to provide some conditions that must be satisfied for a government to be considered a democracy. There seems to be a scholarly consensus that in a democratic government the preferences of all citizens must bear some relation to the public choices made by the government. There have been disagreements over the years, though, between theorists who, with Rousseau, believe that individual preferences when expressed in political action must be "educated" or distilled in order to express the "general will" as distinct from the "will of all," and theorists who believe that individual preferences must be taken as given and that a set of institutions must be designed to translate these preferences into public policy. The theorists dealt with here belong to the latter school.

sponsive, sometimes astonishingly so. The fear that a bloc of voters will support the opposition in the next election if not given at least some of what it wants; the desire to forestall criticism; the realization that a legislative alliance might crumble if a leader chooses one policy rather than another; the willingness to listen to pressure groups; the abruptness with which some obscure matter suddenly becomes high policy because some group or other is making a row over it; the sensitivity to charges and countercharges—are all clearly evident in the behavior of polyarchal leaders and all are testimony to the need such leaders feel to respond to ordinary citizens and subleaders.

In practice there is a crucial difference between the situation of the current officeholders and that of their competitors for office in the next election. Incumbents are vested with the power (or more frequently, a part of the power) to make policy. Where Congress is concerned, however, this fact may appear at first glance to give the individual member, especially of the House, no great advantage. After all, a congressman is but one of 435 representatives, each endowed with a single vote. Nonetheless, the institutions of Congress are so structured that the individual congressman can in effect make certain policy decisions by himself or with a small (generally likeminded) group. For example, if some agency is considering building a project in a congressman's constituency, his announced support, opposition, or indifference will have a great effect on whether or not the project is built. It is the committee system that is designed to facilitate the decentralization of decision-making from the whole House or Senate to an individual or small group. Most decisions made in committee survive substantially unaltered during the remainder of the congressional process. In short, the requirements of the division of labor and the practice of reciprocity on constituency matters serve to "privatize" many congressional decisions, that is, to leave them up to individual congressmen.

The first part of this chapter will focus on the individual congressman, presumably seeking reelection (or perhaps advancement to a higher office), who is faced with the decision of whether to support, oppose, or remain neutral on a proposed project within his constituency. The conclusions reached in the previous chapter may seem to make the solution to the congressman's dilemma obvious. We might be tempted to argue that if a project has a benefit-cost ratio greater than one, and if the local district pays about 1/435 of the total cost of the project while retaining nearly all of the benefits (both assumptions hold approximately), then the benefit-cost ratio to the district will be in excess of 400 to 1. Surely the congressman could not afford to oppose or remain neutral on a project of such great profitability to his district. Yet sometimes congressmen do oppose projects with such favorable ratios for their districts, and we shall try to discover why.

The second section of this chapter will investigate the conditions under which House members will favor or oppose projects outside their constituencies. One might think that, if project benefits are generally concentrated within single congressional districts while costs are spread out, congressmen would tend to oppose projects outside their own districts or remain neutral toward them. In general, however, they do not. On occasion congressmen actively favor some projects outside their districts, and they rarely oppose others. The motivations behind this phenomenon will also be explored.

Political Benefits and Costs of Projects Within the District

In the literature about Congress it is generally assumed that local projects are always desired. Bailey and Samuel, in *Congress at Work*, describe the genesis of a project as follows. "After a local group has become interested in a project, the next step is for its members to make a trip to their representative or senator, who normally is happy to fill their request."[3] But this description does not take into account the philosophy behind the adage that "for each political appointment you make nine enemies and one ingrate." Local projects can help a congressman, but they can hurt him, too, especially if they are controversial or poorly handled.

Congressmen generally agree that constituency-oriented expenditure can be valuable to them. There appear to be three separate reasons why congressmen value projects in their districts. First, projects are valuable in a reelection campaign as part of an incumbent's record in office. Second, they maximize discretion over other legislative issues. Third, they ensure that a project issue will not be held against the congressman by groups who could enter or back an opposition candidate in the next election.

Water projects in particular are something that congressmen generally believe can help them get reelected. Basically, the process can work in two ways. In the first place, getting the projects can be symbolically important in a reelection campaign, since it shows voters that their congressman can do things for them in Washington. Examples of this sort of election appeal can be found in campaign literature. In Senator Mundt's (Republican–South Dakota) 1966 reelection campaign, one of his flyers claimed that when the Budget Bureau cut funds for the Oahe Dam in 1954, Mundt got them restored. Perhaps even more important is the fact that projects provide benefits for a few well-organized groups in the district. Construction workers, contractors, and local businessmen receive economic rewards from nearly any sort of construction project; these groups frequently contain important potential contributors to, or workers in, a particular congressman's campaign. Clapp quotes a congressman in this regard:[4]

> Wouldn't everyone agree that if you can get something done that you can't justify on merit, but which goes into your district, the so-called pork-barrel project, that you find it is effective. I agree there are a sophisticated few very substantial opinion molders who get the implications of this, perhaps even before you bring home the bacon. And I think the sophisticated few are pretty important.

It is clear that many congressmen feel that local projects are a relatively cheap way to ensure reelection.

In the second place, a congressman who successfully "brings home the bacon" may be able to ensure himself more freedom in voting on other issues of greater interest to him. The most famous example of this was Frank Smith, a former Mississippi congressman. Of course, no amount of federal expenditure in his district would have allowed him to vote for a civil rights bill, since his constituency had very intense feelings against such legislation. However, by bringing a lot of projects into his district, Smith felt that he could afford to go along with the legislative desires of the Democratic leadership fairly often. He remarked, "In my twelve years in the House I voted with the Democratic leadership three-fourths of the time."[5]

One issue on which he was able to vote with the leadership consistently was free trade. "Speaker Rayburn asked me if I would be interested in going on the Ways and Means Committee in 1955, but I couldn't even afford to think of the change. My one hope of keeping a Delta district together was the importance of my position on the Public Works Committee." Smith summed up his position in the following way: "When I came to the House of Representatives I wanted to be a responsible member. If my constituents were to give me this freedom, I felt I had to render them special service in areas of major concern to the district. This called for specialization in flood-control and water-resource developments."[6]

The third motivation, that of inhibiting potential opposition, is somewhat mingled with the desire to ensure reelection. Not all congressmen fear that defeat awaits them at the next election. For some, although there is still some uncertainty about being returned, reelection campaigns are a bothersome ritual made necessary only if the opposing party puts up a candidate or if the opposing candidate has serious backing. Sometimes careful attention to projects in the district can help forestall opposition. Frank Smith, for example, was opposed by the powerful Delta Council in his district in his initial run for a House seat. By 1962, however, after he had worked for 12 years on the Public Works Committee, the majority of the Delta Council business leaders supported him in his campaign against Jamie Whitten (Democrat–Mississippi), with whose district his had been merged.[7]

Even though a project may be useful to a congressman in the ways listed above, it may also be dangerous in other ways. Joe Martin (Republican–Massachusetts) gives two examples of dangers in his autobiography:[8]

> When I was first elected to Congress . . . I fell heir to the old Taunton River issue. For a hundred years my district had been interested in having the river widened and deepened to make it navigable for steamers.
>
> Where my predecessors had failed I got the necessary legislation through Congress, only to find it the biggest issue raised against me in the next election. The railroads were angry because it threatened them with new competition. The gas company people were furious because they feared that they would be put to great expense removing their pipes from the river bed. The owners of a large stove company at Taunton were frantic lest the widening of the river weaken the foundation of their factory. And to top it off many of the voters were suspicious that I, who was to become a veritable symbol of economy in government, was a big spender.
>
> Many people suppose that rivers-and-harbors legislation and the pork barrel are a congressman's surest means of keeping a hold on his district. Before going to Congress I may or may not have shared this belief, but once there it did not take me long to discover the fallacy.
>
> In 1928 a fire destroyed a large part of the Fall River business section. It occurred to me that it might inspire private owners to rebuild quickly if the federal government would step in and put up a new post office, even though the old one had not been damaged. I went to see President Coolidge about it . . . [and] Coolidge penned a note to Secretary of the Treasury Andrew Mellon: "please see what you can do to give Joseph a post office." In no time I had the funds, which I might reasonably have concluded would make everyone in Fall River happy and, therefore, well disposed toward me on the next election day. The first thing I knew I was in the middle of a sizzling fight in Fall River over the location of the post office.

These are disturbing anecdotes for those who believe in the truth of the conventional wisdom about the pork barrel. Why does one project help a congressman and another hurt him? What can he do to prevent making mistakes and miscalculations?

One of the common explanations for the existence of pork-barrel expenditure is that the costs are spread thinly over all districts and the benefits are highly concentrated in one or a few districts. The costs of organizing opposition to a congressman whose district must bear only 1/435 of the costs of a project are so great relative to the potential benefits that it is not worth the trouble to get other congressmen together to oppose the project. If a congressman tries to oppose a project without previously having organized opposition, he will gain only the enmity of the project's sponsors; as a result, they may try, more successfully, to oppose his projects. The few congressmen whose districts gain heavily from the project have lots of incentive to organize support for it at the critical points of decision. The basic theory is that when costs are spread out and benefits concentrated, the project eventually ought to work its way through the whole formal procedure.

This theory presumably applies to economic costs and benefits, and it is stated in a way that ignores the distribution of benefits and costs within the district. The Corps' evaluation of project benefits generally measures the following kinds of benefits, among others: flood control, navigation, recreation, water supply, water-quality control—low-flow augmentation, fish and natural wildlife enhancement, electric power, irrigation, and beach erosion. All benefits are measured in dollars according to certain specified rules of thumb.

Certain of these benefit categories plainly entail a cost to particular concentrated economic groups. For example, the presence of navigation benefits in a project indicates that the project will make water transportation available over certain routes at marginal costs low enough to be competitive with other modes. If these alternative modes have economies of scale in operation, as railroads do (ton-mile costs decline as more tons are shipped), the marginal costs to the railroad increase since tonnage is directed to the waterway while the rates are forced to decline (all of this is subject to I.C.C. regulations, of course). Thus building a project with navigation benefits imposes a severe tax on railroads shipping over the same routes.

Another example is power benefits. Building a project with power benefits will generally act to lower local rates and at the same time raise average costs to local private power companies.* In addition, dams built to provide power must be taller than flood-control dams so that more upstream land is flooded. As a result, coalitions between power companies and upstream landowners often form to oppose

---

* Costs will increase because there are economies of scale in electricity generation and supply too.

TABLE 3.1

Classification of Projects

		WINNERS	
		Concentrated and organized	Dispersed
LOSERS	Concentrated and organized	I Strong pressure both ways	II Local opposition to project
	Dispersed	III Local support for project	IV Little controversy —congressmen can safely choose sides

such projects, although power company opposition has lessened somewhat since the late 1940's as the federal government has sold more and more of the power it generates to private companies for distribution.

Flood control generally can be accomplished in two ways: the first is to build dikes and levees to keep the river in its stream bed in case of high water not exceeding the height of the structures; the second is to build dams to hold the water during flood stages. The building of dams involves the flooding of upstream areas in order to provide flood protection downstream. Consequently, upstream landowners can usually be counted on to oppose the building of dams.* This provides a more or less constant source of opposition to building dams for flood-control purposes.

Some benefits in the benefit-cost ratio are widely dispersed over large population groups. In particular, recreation, wildlife, water-supply, and water-quality-control aspects of projects provide small benefits to large numbers of people. Sometimes organized groups such as the Sierra Club or Friends of the Earth will participate in the debates about a project, but most often these dispersed-benefit recipients are unorganized and exert little pressure one way or the other.

Table 3.1 indicates the kinds of forces the local congressman will generally be subjected to. If the project is built there will be two groups: winners and losers. This figure treats characteristics of these

* "Eminent domain" is frequently employed to procure the land that will be used for the project. This generally has the effect of forcing upstream people to sell land at a price lower than they might voluntarily take on the open market. Thus upstream landowners will frequently oppose such projects.

groups as independent variables. The entries describe the local pressures. The various types of benefits from projects arrange themselves very approximately into the figure in the following way.

I. Navigation (railroads lose; barge operators, construction operators, construction workers, and shippers gain). Flood control where land upstream is privately owned. Electric power when there are power cooperatives.

II. Power if there are no cooperatives. Flood control when downstream land is owned by poor people or the government.

III. Most flood control (especially dikes and levees), beach erosion (hotel owners gain), water supply (local water companies can buy the water).

IV. Recreation, water-quality control, or fish and wildlife enhancement when land to be flooded is not valuable or is owned by the government.

The benefit categories are not an infallible guide to the local response to the building of a project. Consequently, over the years very elaborate and time-consuming formal procedures have been evolved to allow the localities affected by the project ample time to develop opposition and support. The congressman need not commit himself to any great degree until fairly late in the process, and then only when he is fairly sure of local opinion on the matter.

Arthur Maass provides an excellent discussion of the Corps' activities in adjusting group interests. He says:[9]

> Engineers have encouraged participation by local interests for the purposes of (1) protecting and safeguarding the respective interests; (2) obtaining information desired and necessary for the planning of projects; (3) making possible an adjustment of diverse viewpoints and the winning of consent for proposed action based on such an adjustment.

This quotation makes clear the obvious fact that projects do not just spring forth from some Zeus's head complete with a particular configuration of benefits. In the planning process the Corps may choose to design a project to include water-supply benefits but not power benefits, for example, because of a desire to maximize local support for the project. Bailey and Samuel state it concisely:[10]

> In the field, working with various local groups interested in flood control, canals, drainage, and a host of other projects, they have learned to "maximize the total desires of the community." Translated this means adjusting local differences and uniting local pressures behind the most feasible method of accomplishing the project. And being extremely politically conscious, the Corps has learned how to "maximize" its congressional relations as well.

Project planning is inevitably a political process; it determines whose land will be taken and whose protected, what shipping costs certain firms will pay, who will be employed locally, and who will make financial contributions. In short, planning helps to shape the future of the local community; therefore, in making up plans for improvement of the water resources of a community, the Corps must and does try to build a coalition broad enough and stable enough to see the project through. Local opposition and controversy constitute an invitation to congressional opposition, or at least reluctance. Projects with strong local opposition are put off until most opponents can be mollified.

The observer might wonder why there are projects with opposition at all. The Corps can move a dam (for example) up or down the river, change its height, remove power-generating facilities, and make other variations until all major groups are contented with the project. There are, however, some constraints on the process of adjustment to group interests. The chief one appears to be the requirement that the benefit-cost ratio exceed one. A low flood-control dam could be favored by the local community, but its benefit-cost ratio might be below one. The addition of electric-power-generation facilities may raise the ratio over one but, at the same time, may generate the opposition of local private power companies.

For an example of such a situation we can take the Roanoke River Basin Project. The Corps report issued in 1944 (House Document 650) concluded that "comprehensive development of reservoirs for flood control only was not justified economically on a basin-wide program."[11] However, the Corps found that if electric-power-generation capability were to be built into the basin projects, there was an economically feasible plan to provide flood protection and power. Wallace sums up the report in the following manner.[12]

> The economic balance sheets make it perfectly clear that the Buggs Island project [one of the separable parts of the basin project] could not be built economically for flood-control purposes only. Furthermore, the facts also make it plain that flood control for the basin could not be achieved except by use of multi-purpose reservoirs with sale of hydroelectric power to provide a favorable economic ratio.

After the issuance of the report, the House Flood Control Committee held hearings for the 1944 Flood Control Act. The major power companies, the Virginia Electric Power Company (VEPCO) and the Carolina Power and Light Company, led the coalition against the project. The power companies contacted members of Congress who were known to be ideologically opposed to federal power projects and hired a public relations firm to arouse local opposition. The opposition aroused by the companies was sufficient to turn one local congressman, Harold Cooley (Democrat–North Carolina), against the project. He argued, procedurally, that there had been inadequate hearings and that as a result the project should not receive funding.

The planning process engaged in by the Corps is likely to help determine the electoral impact of a project for affected congressmen as well as the project's engineering features. Even so, there are constraints placed on the Corps' discretion not only by the benefit-cost ratio but also by geography. If a river runs between two states (as does the Savannah River between Georgia and South Carolina) or between two congressional districts, the political benefits and costs of a project may fall unevenly among the affected senators and representatives. There are plenty of examples of conflicts between states or districts on matters of this sort: Georgia and South Carolina over the Savannah River, North Carolina and Virginia over the Roanoke River basin, and Illinois and Indiana over the Wabash River are only a few. This last conflict is illustrated in the following testimony before the Senate Appropriations Committee's Subcommittee on Public Works.[13] Senator Douglas (Democrat–Illinois) is testifying in favor of a levee system on the Illinois side of the Wabash River at Mount Carmel and an upstream reservoir (Lincoln Reservoir).

> *Senator Douglas.* Mr. Chairman, I feel deeply indebted to you for the way you recognize the merit of claims of those on the Illinois side of the river. I do not want to exacerbate the cleavage between Illinois and Indiana which I want to have healed at the earliest possible moment. But our Hoosier friends slipped things over on us in the past in having levees built on the Indiana side which do protect their lands—and we are very glad to do that—but it throws the water over on the Illinois side. This having been done in connection with a project at Vincennes, the same crime was about to be perpetrated on us at Mount Carmel. But at my insistence, and with your help, Mr. Chairman, we are instituting parallel construction on the Illinois side which will save Mount Carmel in all probability from being flooded, and which will protect agricultural land south of Mount Carmel.
>
> We are very glad to join in a levee program—although we think it is not the ultimate solution—provided that the levees are built on both sides of the river, are of an equal height, and are constructed more or less simultaneously. We could not construct them simultaneously this time, because our Hoosier friends got theirs first. But they have reformed, and it is all over now. And we hope that they will move in the paths of virtue in the future. If they do not, we will properly defend our interests.
>
> So I hope that this work on the Illinois side of the river may proceed.

Now, Mr. Chairman, I also hope that we may start work on the upstream reservoirs on the Illinois side of the river. I want to say frankly that I think our Indiana friends showed greater alertness in this matter than we have on the Illinois side. They started a reservoir system before we did. We lost one authorization on the so-called Lincoln Reservoir south of Charleston when it was withdrawn by popular vote.

But I want to thank you, Mr. Chairman, for last year, both for authorizing this, and for supporting it in your other capacity as a member of the Appropriations Committee. I am not betraying anything when I say that the Lincoln Reservoir was something unique in congressional history, in that the money was appropriated a few days before the authorization was given. And I want to thank you, Mr. Chairman, for making this possible. I use it to show to our Hoosier colleagues that we are not quite as dumb as they once thought us to be.

Senator Hartke. Will the Senator yield?

Senator Douglas. It is hard to keep up with these country boys.

Senator Hartke. Will the Senator yield?

Senator Douglas. Yes.

Senator Hartke. I might call attention to one very important fact that the State of Illinois, rich as it is, is able to take care of most of their problems themselves. We are poor people over in Indiana. We appreciate the help that you have given us. But let me point out the fact that the Indiana territory used to include the State of Illinois, and that they were so capable of taking care of themselves that we have let them go by themselves. We have disentangled ourselves from their properties. They have never repaid us for that. We want you to know that we did not relinquish it in fee simple, that we only gave them a life estate. If they feel that they are neglected too much, if they want to join us, we know it will be to their benefit. If they want to join the State of Indiana, we will be glad to have them back.

Senator Douglas. I am very glad this offer is made, but I don't think we should accept it, because then Illinois would control Indiana again.

Senator Hartke. Some people think that this has happened already.

Senator Ellender. I want to say that I have always been very sympathetic with the situation in Illinois and Indiana. I need not repeat this too often, but I thought it was a bad thing for us to have constructed the levees on the Indiana side and not on the Illinois side. Of course, the reason why I suggested that we build the levees on both sides of the river is that we had the same situation in my area years ago when Congressman Whittington was chairman of the Flood Control Committee of the House. He always saw to it that the levees on the Mississippi side were about three feet higher than the ones on the Louisiana side, so that Louisiana would get flooded before Mississippi would. So it is because of that experience that I sympathize with the problem in Illinois and Indiana.

Senator Douglas. We are very grateful to you.

Senator Ellender. I hope to continue with the construction proceeding on both sides of the river, because I certainly believe that it ought to be handled that way. I don't believe in drowning out some people to take care of others.

Senator Douglas. I will say that in recent years there has been an improvement in the behavior of the Senators from Indiana, partially due to the change in the composition of the delegation, and partially due to an inner state of grace, too.

Senator Ellender. I am glad to have a little help in my efforts to have these levees built concurrently.

Senator Douglas. You have been of tremendous assistance.

The economic theory developed in Chapter 2 indicated that the economically optimal package of projects for a given budget would consist of the group of projects with the highest benefit-cost ratios. This chapter has shown, though, that in large part the benefit-cost ratio is determined by the essentially political nature of the processes of project choice. The requirements of local support—and especially local congressional support—make it imperative on occasion to trade off a higher benefit-cost ratio for more local support. In addition, there are categories of benefits, particularly in the areas of recreation and water supply, that are evaluated according to rules of thumb that have little relation to the real benefits received by the people living near the project. The addition of recreation benefits, for example, has allowed many previously unfeasible projects to display favorable benefit-cost ratios.

When a congressman or an aspiring opponent faces the decision of whether to support or oppose a proposed project, or of whether to ask the Corps to modify it so that it becomes more acceptable to him, he must evaluate the benefits and costs to himself of building the project or some variant of it. The political benefits and costs of a project to a congressman depend first on who stands to gain and who to lose if the project is built, and second on how these people or groups will reward or punish the congressman for his decision. In many cases his decision is simple, but in others he faces uncertainty. In such situations the rule of thumb available to the congressman is that only those people who stand to gain or lose large amounts if the project is built (or not built) might change their political behavior as a consequence of the congressman's decision. It may pay them to find out his position on the project and then to try to change or to reward it. Typically, certain people stand to gain substantially from the construction of any local project: contractors, construction trade unions, and local merchants. In addition, these groups are often deeply involved in local politics. If a congressman must rely on these segments of the local population for political support, then he is likely to favor construction of many projects that produce few benefits in a benefit-cost analysis, even in the local district.

There are also strategic considerations behind the congressman's choice. If his opponent already has taken one position on the project, or is likely to, the congressman may take the same position out of fear of losing votes on the issue if he guesses wrong. An interesting example of this phenomenon occurred in the race for the congressional seat representing the newly redistricted 21st district of Illinois for the 93d Congress. This district had previously been the 22d district represented by William Springer (Republican–Illinois), who announced his retirement as of the end of the 92d Congress. The Democratic nominee for the seat was Lawrence Johnson, Champaign County State's Attorney, while the Republican candidate was Edward Madigan, a representative in the Illinois state legislature.

The project under consideration was the controversial proposal by the Corps of Engineers to build Oakley Dam on the Sangamon River above Decatur, Illinois. The principal benefits of the dam would be water supply (for Decatur), flood control, and recreation. If it was built, the flooding pattern of the Sangamon upstream of the reservoir would be substantially altered so that some of the forest in Allerton Park, which is owned by the University of Illinois, would occasionally be subject to severe flooding. Environmentalists contended that many trees would be destroyed[*] and that the project, properly evaluated, was not economically feasible.[†] Many upstream farmers opposed the project since it might have raised the water table sufficiently to cause serious drainage problems.[‡]

[*] Most of the forest is second-growth, although according to the Committee to Save Allerton Park, one of the major groups opposing the Oakley Dam project, there are some virgin stands. Allerton Park has been used by the University of Illinois as a field experiment area for more than 30 years. The park is one of the very few forested areas in the state in approximately presettlement ecological condition; it is considered valuable for recreational and research purposes.

[†] Professor A. J. Heins of the Economics Department of the University of Illinois has analyzed the benefits and costs of the Oakley Dam project and has demonstrated, employing the Corps' techniques, that the Corps committed several errors in its original analysis. He shows that the benefit-cost ratio is actually .97 rather than the 1.15 the Corps reports. If the Corps were required to employ the discount rate in effect in 1971—5.125 percent instead of 3.25 percent—the benefit-cost ratio would drop to .69. See Heins, "A Reconsideration of the Economic Aspects of the Oakley Reservoir Project."

[‡] Central Illinois was originally marshland, and settlers in the area have always had drainage problems. Most of the farms in the area have been "tiled," or provided with underground drainage channels leading to drainage ditches. The state is organized into drainage districts which levy local assessments for this work. Farmers in the area are very conscious of the drainage problem since they have had to combat it all their lives. Some farms around the Carlyle Reservoir, a nearby Corps project on the Kaskaskia River, have been subjected to severe problems of this type during the spring planting season.

Congressman Springer was a strong supporter of the Oakley Dam project, but the two congressional candidates for his vacated seat had not formally announced positions on the project prior to May of 1972. Environmentalists, who are well organized in Champaign County, had hoped that Johnson, the Democratic candidate from Champaign, would come out against the project. The principal benefits of the project, those relating to water supply, would go to traditionally Democratic Macon County, where Decatur is located.

Terry Michael, a staff writer for the *Champaign-Urbana News Gazette*, argued that Johnson would have to run very well in Macon County if he hoped to defeat the Republican Madigan, who was expected to do well in his home area of McLean, DeWitt, and Piatt Counties as well as in traditionally Republican-Champaign County.[14] The environmental opposition was centered in Champaign-Urbana. Johnson expected to do well in the university community in these cities regardless of his announced position on Oakley Dam. Therefore, it came as no surprise to environmentalists when both Madigan and Johnson endorsed Oakley Dam on May 7, 1972. Both felt that there were votes to be gained in Macon County but none to be lost in Champaign County. Johnson remarked: "If there is a feeling I am taking a politically popular position (in support of Oakley) I don't recognize one. I regret that some people in Champaign County will be upset with my position. I would point out that there are many other issues to consider."[15] Johnson's staff was very sensitive to the environmental groups in the area, and environmental group leaders believed that Johnson was sympathetic to their arguments.* They felt that he had to announce the position he did for fear of losing too many votes in Macon County.

This example serves as a partial illustration of the very complicated relationship between a proposed project's political benefits to a candidate for office and its economic benefits. One of the complicating factors is the fact that congressmen do not know who cares enough to switch votes on a project issue. Madigan and Johnson both apparently felt that most Champaign County voters were traditionally of either one party or another, or that they would make their voting decisions on the basis of other issues.† When there is significant local opposition to a project, considerations of this sort would seem to make the decision of whether or not to favor a project a difficult one for candidates.*

---

* This statement is based on private interviews with a director of the Committee on Allerton Park. Although Madigan did not endorse the Oakley Dam until May, there seems to have been little doubt in anyone's mind that he would do so.

† The matter was complicated by the fact that the Lindsay-Schaub newspaper chain, which owns the only Decatur newspaper and one of the two Champaign-

Political Benefits and Costs of Projects Outside the Constituency

The simplified model offered in this chapter establishes an image of a congressman supporting certain projects in his district, staying neutral on others, and even opposing a few. On projects outside his own constituency one might expect his mild opposition or neutrality. However, the observer of public works decision-making is struck by the fact that congressmen regularly express support for projects that do not have a direct and positive effect on their constituency. Why does a representative from Los Angeles lend enthusiastic support to a project in the 1st district of California, located more than 400 miles away? Why does a congressman from the Delta region of Mississippi come out strongly in favor of a navigation project in Ohio or Florida?

Partial answers to these questions can be found in the formal and informal institutions of the House of Representatives. Two well-documented phenomena provide explanations: first, members of the same state delegation cooperate with each other to a great extent; second, members of the same committee cooperate with each other. The second phenomenon is easy to understand, since the committees are decision-making entities. Cooperation among members is likely to be exhibited since committee decision-making is the major channel for exercising influence over policy for most representatives. Representatives frequently join the Public Works Committee or choose to stay on it because they desire a particular kind of decision on some project from their peers. It is not surprising, then, that committee members support each other's projects.

Urbana newspapers, was strongly in favor of the Oakley Dam. A director of the Committee on Allerton Park said that the chain would certainly try to make an issue of Oakley Dam in Decatur if one of the candidates failed to endorse it.

* Corps of Engineers projects in central Illinois are facing more and more local opposition in part owing to the fact that environmental groups are forming alliances with local farmers and drainage districts. In addition, environmentalists have informed the author that the Illinois Central Railroad is showing interest in financing some of their efforts to combat navigation projects in the state. These alliances have formed the backbone for opposition to several projects in the central and southern part of the state. For example, a coalition of a drainage district and an environmental group recently obtained an injunction enjoining the State of Illinois from continuing work on a project in central Illinois. This sort of thing seems to be becoming more common throughout the nation.

State delegations seem not to play as critical a role in House decision-making as committees do, for they have no formal part in the ordinary course of policy-making. However, several scholars have found that certain delegations meet regularly, tend to vote en bloc on many issues, and favor each other's pet projects.* Why should this occur? It has been argued that by voting in roll calls as a group whenever possible, state delegation members can maximize their bargaining advantage and hence their ability to get projects for their districts. This argument would seem to apply with equal force to any arbitrary group of congressmen though.

Another argument has been advanced to the effect that congressmen from the same state tend to have "similar" districts, and that hence their interests will tend to converge on a broad range of policy issues. This is surely an empirical question. There are many projects that, if built in one district, actually harm adjacent districts. In other cases, there are projects that could reasonably be built in any of several districts in a state, but that when built in one district cannot be built in another. It is evident that with regard to some projects, congressmen from the same state will tend to have more conflicting than parallel interests.

There are certain mechanisms that link the interests of congressmen from the same state. The democratic theory outlined by Dahl, Schumpeter, and Downs[16] is characterized by basing the link between popular demands and policy-making on the desire of political decision-makers to retain office. This theory has been extended by Joseph Schlesinger[17] to include a mechanism (ambition for higher office) that encourages certain political decision-makers to respond to broader constituencies than if they were only seeking reelection. Many representatives seek the opportunity to run for senator, governor, or some other statewide or even national office.

Another mechanism is more technical in character. The growth of economic activity in one section of a state is likely to have certain indirect effects on economic activity in other sections of the state. These mechanisms depend heavily on the structure of transportation networks and of the economy in the particular state and region. This explanation is viable only where a relatively integrated intrastate economic structure exists, or where a project built in one district actually has *direct* spill-overs into other districts (this would hardly account for state delegation *units*, however).

---

* There is an extensive literature on the influence of the state delegation on the behavior of the individual congressman. In the following analysis, Barbara Deckard's "State Party Delegations in the House of Representatives" will be heavily relied on because of its richness in interview material and breadth of scope.

Still another source of incentive for cooperation within the delegation is the fact that the state delegation chairman has some control over committee assignments. The delegation chairman, at least, would seem likely to have a clear preference for a cohesive delegation over which he would have some influence through his control of committee assignments.

In certain issue areas in which the location of projects or the distribution of federal funds is a major payoff to the congressman, local interest groups have formed statewide alliances. Chambers of commerce, river basin associations, and various associations of mayors, city managers, farmers, and others are frequently organized at the state level. Consequently, in many policy environments the influences of local interests are coordinated by state-level organizations.*

There are, of course, additional incentives for cooperation within the delegation which are peculiar to various states. The Chicago delegation (which happens to include most of the Illinois Democrats in the House) is cohesive in part because the reelection of its members is largely controlled by a strong centralized political party organization which believes that a cohesive party delegation from the city advances its interests. Each state probably generates forces for or against delegation cooperation that are unique to it.

Whether all of the relevant forces have been catalogued or not, it can be stated with certainty that the state delegation structures much important congressional behavior. Roll-call research[18] has established that the state delegation is an important source of cues for the representative's voting decision. Matthews and Stimson, in their analysis of roll calls in the 85th through 88th Congresses, found that the state party delegation was the most important "cue-giver." Clausen, analyzing roll calls on five issues in the 85th and 86th Congresses, found that state delegations are an important explanatory variable.

There is more to congressional behavior than roll calls, of course. Recently there has been some interesting work done on state delegation cooperation over a wider range of behavior than roll calls. Barbara Deckard's dissertation attempted to find out just how members of state delegations cooperate with one another. Not surprisingly, she found a good deal of variation. In some states, the members of the same party met frequently, talked over a wide range of topics, and expected to give and to get help from other delegation members in achieving policy goals. In other states, members of both parties expected regular cooperation across party lines on certain types of issues. In still others, members of the same party never met as a group, and considerable acrimony reigned among delegation members.

---

* Daniel Elazar has written extensively on this subject. He argues that certain states are very successful in obtaining federal funds because the state government, local groups, and congressional delegation have learned to work together on matters of interest to the state. Such states function as what Elazar calls "civil societies." He says that "the key to the acquisition of federal benefits is intensive state and local activity to secure those benefits. The better a state is able to function as a coordinated civil society, the more likely it is to obtain a large share of federal expenditures" (Elazar, pp. 74–75).

Matthews and Stimson, in the work already cited, interviewed a large sample of congressmen in an attempt to explore to what extent state delegations work together. As in Deckard's interviews, we find in Matthews and Stimson the same great diversity in the expectations of congressmen and and in their behavior.[19] Some state delegations meet regularly. For example, the California and Texas Democratic delegations meet once a week, the Tennessee Democratic delegation once a month. Large delegations speak of having a "spread" of members on committees. A 15- or 20-man state party delegation might have members on all of the House committees. This enables the members to get together to review committee work with each other, arrange testimony, and keep track of projects important to them in other committees. Deckard says that the Texas members see the delegation as a good source of information about what is going on in committees because they have a good spread.

Other state party delegations meet only when issues of special interest to someone in the state come up. Matthews and Stimson cite the response of a Florida Republican in one of their interviews: "As far as delegation meetings go that's pretty much left to the discretion of the ranking member.... The entire delegation gets together on his call when a subject matter comes up that he feels we need to discuss. It's more in the form of matters of interest to Florida, rather than national policy." On certain kinds of issues, members of one party delegation help out members of the other party in their state. A Washington Republican said, "If I can get a bill through for one of the Democrats I do so." The Pennsylvania representatives have a steering committee made up of senior members of both parties. Deckard summarized references to some of their activities in her interviews. "Flood control, highways, a proposed canal, an oceanographic lab, problems affecting the Port of Philadelphia, and the closing of the Air Force base near Harrisburg were given as examples of the problems and projects upon which the steering committee had taken action."[20]

Deckard ventured two hypotheses to explain the fact that some state party delegations act together on a broad variety of issues while others do not. First, the greater the number of delegation members interested in a career in Congress, the more cooperation; second, the more stable the delegation, the more it should act as a group. She also argued that large delegations had the additional resource of broad committee coverage. It is evident that among those delegations that occasionally work together the following two propositions are true: (1) cooperation is most likely on divisible or distributive policies (highways, dams, buidings, and so on); (2) there is an expectation that the member of the relevant committee will help out a delegation member, and this expectation crosses party lines in some delegations.

Some delegations are unable to cooperate even on constituency matters. The best known of these is the New York Democratic delegation. Its members claim that the senior delegation members could be very effective in getting projects for colleagues if they wanted to, but that they do not. A New York Democrat complained, "Many of the senior men are barely polite. . . . They wouldn't go out of their way to help you with anything." Deckard quotes an administrative assistant to an upstate New York congressman: "It's like two states. The city congressmen . . . don't even know where upstate is. . . . New York didn't get reimbursed for the Thomas E. Dewey thruway and Buckley was chairman of Public Works at the time. He couldn't care less."

Among delegations that do work together on projects, various types of activity are possible. The simplest and probably the most common type is watching out for one another's bills and projects in committee. A second type of activity is co-sponsorship of bills. In a third activity frequently mentioned, members of the same delegation utilize their access to agenices or to other congressmen to help each other out. A Massachusetts Democrat said of McCormick during his tenure as Speaker: "If you are having trouble with an agency, you can have him call over and that has more effect than if any one of the rest of us called, and he has been very helpful on constituency matters." A New York Republican is quoted, "I needed something from an agency. One of our members is on the Appropriations Subcommittee that handles that. I got him to help. They heard him fine while they seemed to have some trouble hearing me."

The most cohesive delegations will sometimes work together as a group on projects. Certain state delegations testify together as delegations at the hearings on Public Works Appropriations. The same states will also have their delegation testify informally before the Budget Bureau. The most effective of these state delegations work out in consultation with the district engineer of the Army Corps and with local interest groups an agreed-upon set of projects to support. They then go before the Budget Bureau and the Appropriations Committees unified behind a single construction program. Clem Miller has described the Budget Bureau hearings: "Witness after witness files by—governors, senators, congressmen, county engineers, state water directors—an endless procession. . . . Our state delegation (California) irons out its differences in advance. Our people then go in with a united front, agreed on our askings, agreed on our nonbudgeted requests."[21] A Pennsylvania Democrat had this to say: "We work together on state matters. Appalachia, highways, public works projects. We all support anything that's for Pennsylvania no matter where it goes. You have to in this place with its logrolling. You have to convince the delegation of need, then we'll all support you."[22] Congressmen seem to agree that committees take testimony from a united group more seriously than requests from individual congressmen. One congressman is quoted by Deckard as follows: "I'm getting people together to talk about a dam, the North River dam. There's no money in the budget for it, and Jordan and I decided to get together and see if we couldn't get some money for it. . . . I'm going to get the other people in on that. It always helps to have more people, . . . the committee takes it more seriously if you have a number of people involved."[23] Matthews and Stimson's interviews contain these remarks by a Virginia Republican in response to question 17: "I think it tends to help the state when you have the whole delegation working. For example, if there is a project pending in the state and you get the force of a whole delegation behind you on the [*sic*] bipartisan basis—it helps—rather than piecemeal."

The evidence from interviews suggests that in some delegations members are willing to exercise their political resources (their time, their access to agencies and committees, their committee membership, their expertise, and their advocacy) to advance the interests of their colleagues from the same state. Of course they expect reciprocity; and from the satisfied tone of most of the members of cohesive delegations interviewed by Deckard and others they think they receive it. The completely uncooperative delegation (the New York Democrats seem to be an extreme case) appears to be untypical; in particular, its lack of cohesion was referred to frequently in interviews with members from other states.

The phenomenon of cooperation within delegations broadens the concept of political benefits and costs considerably. It makes it necessary to say that if a project is seen as beneficial to one district, it is seen as beneficial to the whole state. The whole concept of political benefit must be interpreted as negotiated within the delegation. If, for example, a project is desired by one congressman in a state but is opposed by another (perhaps the second congressman represents the upstream people adversely affected by a proposed dam), the only way to tell if its political benefits exceed its costs is to see if the delegation comes out in favor of it. The fact is that negotiation and compromise within the delegation prevent the observer from seeing if a project is politically desirable to a congressman when it is considered by itself. All that can be observed is the congressman's negotiated support for or opposition to a project after having dealt with his state delegation.

The importance of the state delegation as an institution within which a significant amount of exchange on certain issues takes place has serious implications for the testing of the hypotheses of this study. Congressmen have well-established expectations that they can convert their influence over some issue area (let us say agriculture or defense) into indirect influence in another issue area (public works or H.E.W.) within their state delegation. This means, to some extent, that if a member of a state delegation has great influence over a particular issue his whole state will reap the benefits. What is more, if his district is not in a position to benefit directly from his influence over a particular agency or program, his state will receive benefits from the program although his district will not.

This situation is not particularly unusual. For one thing, state party delegations have established prerogatives over seats on certain committees.[24] When a seat becomes available on a committee and a particular state has a claim on it, the state will usually have a very limited number of congressmen who will (or can) take the assignment. Thus to hold its claim it may have to put on a committee a congressman who has no constituency interest in the jurisdiction area of the committee. If this congressman must wait a long time before a desirable assignment becomes available for him, his state delegation colleagues will frequently try to get him to stay on the committee where he has built up some seniority and, correspondingly, some expertise and influence. Thus a congressman with no constituency interest in a committee's jurisdiction will frequently stay long enough to become powerful on the committee; part of his motivation will have been the opportunities for cooperation open to him within the state delegation.

The preceding statements indicate that the hypotheses of this study must be examined at the state level. We should not expect to observe a particularly strong relationship between influence in the House and benefits from the agency at the individual district level, except by chance. A member's increased influence on a committee raises the ability of each congressman in that member's state to get projects from the agencies under that committee's jurisdiction; in turn, it raises the influential member's ability to obtain other kinds of projects outside his committee's jurisdiction for his own district. Since this study focuses on a single agency, this latter statement cannot be proved. All that can be measured is the first effect, and a district-level model would underpredict the projects received by the state delegation colleagues of an influential congressman on the Public Works Committee and would overpredict the projects received by the congressman himself. The overprediction will result from the fact that he can obtain a maximum of *n* new starts in a given year through his influence with the House Appropriations Committee's Subcommittee for Public Works; however, if he gets one project for a state delegation colleague, he cannot also put it in his district.

This effect is evident in the data discussed in Chapters 8 and 9. The benefits from projects authorized in the Omnibus Rivers and Harbors and Flood Control Bill of 1968 were allocated (in whole or in part) to congressional districts. A certain number of these projects were new starts in the 1969 Public Works Appropriations Bill. An attempt was made to estimate the effects of various variables that were hypothesized to have a relationship with influence on public works decision-making (membership on the Public Works Committee, seniority, and so on). All of the relationships were of inconsequential size and frequently went in the "wrong" direction. Yet the state-level data reported in later chapters reveal very strong and intuitively appealing relationships. This result suggests the importance of the state delegation as an institution for mediating exchange among certain policy areas. A thorough treatment of this topic from the point of view of its effects on policy outputs and distributions would require a book-length effort. For the present it is sufficient to point out its importance in identifying the concepts of the political benefits and costs of constituency-oriented policy decisions.

Chapter 3

1. See Schumpeter, *Capitalism, Socialism and Democracy*, and Dahl, *A Preface to Democratic Theory.*

2. Dahl and Lindblom, p. 285.

3. Bailey and Samuel, p. 169.

4. Clapp, p. 122.

5. Smith, p. 134.

6. *Ibid.*, pp. 160, 129.

7. *Ibid.*, p. 81.

8. Martin, pp. 57–59.

9. Maass, *Muddy Waters*, p. 51.

10. Bailey and Samuel, p. 170.

11. Wallace, p. 287.

12. *Ibid.*, p. 305.

13. U.S. Congress, Senate, Senate Appropriations Committee. *Public Works Appropriations Bill for Fiscal Year 1967, Hearings*, before the Public Works Subcommittee. 89th Cong., 2d sess., 1966, pp. 2307–8.

14. Michael, p. 3.

15. *Ibid.*

16. See Downs, *An Economic Theory of Democracy.*

17. See Schlesinger, *Ambition and Politics.*

18. For important recent work and short bibliographies see Clausen, "Home State Influence on Congressional Behavior," and Matthews and Stimson, "Decision-Making by U.S. Representatives: A Preliminary Model."

19. Donald Matthews and James Stimson have generously allowed the author to quote freely from their 1969 and 1970 interviews with a selected list of 108 congressmen.

20. This quotation and those in the next three paragraphs are all from Deckard, "State Party Delegations" (dissertation), chap. 4.

21. Miller, pp. 16–17.

22. Deckard, chap. 4.

23. *Ibid.*

24. See Bullock, "The Influence of State Party Delegations on House Committee Assignments.

762

Science, Geopolitics, and Federal Spending

Thomas P. Murphy
University of Maryland

Heath Lexington Books
D. C. Heath and Company
Lexington, Massachusetts
Toronto London

7

NASA's Project Apollo Facilities Decisions

October 4, 1957, was a turning point in American research and development. On that day, the Soviet Union successfully orbited *Sputnik I*, the world's first Earth satellite. Although the Eisenhower Administration deemphasized the space event as having propaganda rather than military value, the nation was shocked. How could a totalitarian nation with a presumably inferior educational system achieve such a success? Because of our system of local control and taxation for public education, Americans were fully aware of the massive expenditures made by the American educational system. In addition to the higher level of investment in education, the United States had a greater gross national product and a longer history of technological accomplishment. The Soviet success in moving into space before the United States was not easily dismissed.

The national shock was registered in Congress. It did not help when Dr. Wernher von Braun of the Army Ballistic Missile Agency testified that the United States could have been first except that the President's policy of utilizing military missiles for space had denied him the necessary resources.[1] The United States did not have a national space program, although its participation in the International Geophysical Year (I.G.Y.) was coordinated. In effect, it had several programs, since the Army, the Navy, and the Air Force were working separately on IRBM or ICBM development programs. The Navy Vanguard, the United States' I.G.Y. satellite program, was on schedule, but several attempted launches had ended in failure. The von Braun group was given a go-ahead in November and on January 31, 1958, successfully orbited *Explorer I* (from Cal Tech's Jet Propulsion Laboratory) using a modified Redstone rocket and a fourth stage from JPL. This was less than four months after the successful Soviet shot, but in the intervening period the Soviets had orbited *Sputnik II* with a canine passenger.

In April 1958, President Eisenhower submitted and Congress quickly approved the National Aeronautics and Space Act of 1958 which established NASA as a civilian space agency.[2] The core of the new agency, which officially began on October 1, was the National Advisory Committee for Aeronautics (NACA), a research organization established in 1915 and which had grown to over 8,000 employees at five laboratories:

NASA Facilities from NACA	*Date of Establishment*
Langley Research Center Hampton, Virginia	1917

NASA Facilities from NACA	*Date of Establishment*
Ames Research Center Palo Alto, California	1940
Lewis Research Center Cleveland, Ohio	1941
Wallops Station Wallops Island, Virginia	1944
Flight Research Center Edwards Air Force Base Edwards, California	1946

This was essential to NASAs success. NACA had under way a number of space-oriented programs and a solid cadre of experienced researchers. Using NACA, which had good relations with the military agencies, was probably the only way to prevent the establishment of a consolidated *military* space agency instead of a civilian controlled program.

As a result of NASAs establishment, the I.G.Y. Vanguard program was transferred from the Naval Research Laboratory to the new agency by Presidential Executive Order[3] and became the core of the first new NASA research center, Goddard Space Flight Center, located at Greenbelt, Maryland. Several Air Force space projects controlled by the Air Force Ballistic Missile Division and several Army satellite projects were transferred to NASA.[4]

NASA and DOD also agreed upon the transfer of several components of the Army Ordnance Missile Command (AOMC). However, the Army resisted this move and developed some support in Congress for its position that the missile program should not be fragmented. "The subsequent uproar forced DOD to modify its position and the issue was taken to the National Aeronautics and Space Council, where a compromise solution was arranged by December, 1958."[5] The Jet Propulsion Laboratory, a contract research agency of the Army operated by the California Institute of Technology, was transferred to NASA, but the Development Operations Division of the Army Ballistic Missile Agency, headed by Dr. Wernher von Braun, stayed under the Army with the understanding that it would service some of NASAs needs.[6]

The continuing space struggle within DOD led to a reopening of the von Braun transfer in 1959. By this time, the Army had completed research and development work on the Redstone rocket and Jupiter IRBM and was working on the Saturn. However, DOD top management felt that the Army had no mission for such a large launch vehicle and that it should probably be transferred to the Air Force. On October 21, 1959, it was decided that both the Saturn project and the von Braun group of the ABMA should join NASA. Because of statutory and administrative requirements, this decision was not fully implemented until the beginning of the new fiscal year on July 1, 1960. NASA redesignated the

Huntsville group as the George C. Marshall Space Flight Center. It was to remain at the Redstone Arsenal as a tenant organization.[7]

No other new facilities were added under the Eisenhower Administration. However, in October 1958, a few days after NASA was established and about a year after *Sputnik I*, Project Mercury, America's first manned satellite program, was announced. The purpose of the project was to test the feasibility of using men in space. The project was assigned to a Space Task Group (STG) of the Goddard Space Flight Center but was physically located at Langley Air Force Base, Virginia. The STG was independent of the Langley Research Center but received housekeeping support from Langley, the oldest of the NACA laboratories. McDonnell Aviation in St. Louis was selected as the Project Mercury contractor and, with the help of the Department of Defense, seven military pilots were selected as the first American astronauts.[8]

Project Apollo

Although the Kennedy Administration seemed more disposed because of its "new frontier" theme to support the space program as an example of American technological prowess, once again the indirect effects of external events supplied the impetus for immediate expansion of the program. Several writers have argued that the Kennedy commitment to Project Apollo was a desperate move intended to help boost the Kennedy image. Elected on a platform which promised to get America moving again, the new President was greeted with a frustrating challenge from Laos, soon followed by the success of Yuri Gagarin's dramatic first manned space flight on April 12, 1961. In much of the world, this event represented a shock just as great as *Sputnik I*. From 1958 to 1961, NASA obligations had tripled to almost a billion dollars a year, yet the Soviets were again able to succeed in establishing a spectacular technological first.

When, a week later, the Bay of Pigs invasion failed, there was an urgent need to do something which would capture the imagination of Americans and of the world. One recent article asserts that "at the very moment when Castro was rounding up the last of the marauders on his beaches on April 19, the unhappy President summoned his spaceman-in-chief, Vice-President Lyndon B. Johnson . . . . The two men met alone for 45 minutes."[9]

On April 20, President Kennedy sent Vice-President Johnson a memorandum "whose tone was that of a man willing to pay any price to avoid further disgrace. . . ." It reads as follows:

> In accordance with our conversation I would like for you as Chairman of the Space Council to be in charge of making an over-all survey of where we stand in space.
>
> 1. Do we have a chance of beating the Soviets by putting a laboratory in space, or by a trip around the Moon, or by a rocket to go to the Moon and

> back with a man? Is there any other space program which promises dramatic results in which we could win?
>
> 2. How much additional would it cost?
>
> 3. Are we working 24 hours a day on existing programs? If not, will you make recommendations to me as to how work can be speeded up.
>
> 4. In building large boosters should we put our emphasis on nuclear, chemical or liquid fuel, or a combination of those three?
>
> 5. Are we making maximum effort? Are we achieving necessary results?
>
> I have asked Jim Webb, Dr. Wiesner, Secretary McNamara and other responsible officials to cooperate with you fully. I would appreciate a report on this at the earliest possible moment.
>
> JOHN F. KENNEDY[10]

Finally, on May 5, 1961, a Mercury Redstone boosted Alan Shepard into a suborbital flight designated MR-3. From launch to impact, the flight lasted 15 minutes and 22 seconds, for 5 minutes of which Shepard was weightless. Three weeks later, on May 25th, President Kennedy spoke to a joint session of Congress and set forth Project Apollo as a planned program to restore American technological leadership in space. In the words of President Kennedy;

> I believe that this nation should commit itself to achieving the goal, before this decade is out, of landing a man on the moon and returning him safely to Earth. No single space project in this period will be more exciting, or more impressive to mankind, or more important for the long-range exploration of space; and none will be so difficult or expensive to accomplish.[11]

To proclaim so boldly that the United States would have a man on the Moon before 1970, only eight and a half years from the time the President spoke, called for some drastic reorientation and reorganization of the space program. NASA had been fully occupied with attempting to integrate the diverse groups brought into the new organization, develop a 10-year plan, and build a reliable launch vehicle capability. Although this necessary reorientation had not yet been completed, there was much pressure to move ahead faster.

In this context, the Bureau of the Budget, which in the preceding January had cut the program, was forced to concede new resources to implement the President's decision.[12] NASA had to formulate a plan for a manned lunar landing program. Major decisions had to be reached as to the manner of organization and the nature of the new facilities needed to accomplish the lunar mission. Specifically, the Air Force had been running Cape Canaveral since 1950 as a DOD launch site for missile testing by all the services. NASA had a different kind of problem in that it needed to have both testing and operational facilities. The Army had launched the *Explorer I*, the Mercury-Redstone, and the Air Force the Thor-Delta and Atlas Booster flights for NASA from Cape Canaveral. However, the scope and magnitude of Project Apollo would be too complex and unique for any of the military facilities. In consideration of the size of the launch vehicles that would be necessary for the lunar mission, NASA would need expensive new permanent facilities and far more separation between launch pads and the surrounding towns than the DOD had at the Cape.

At the time of the Gagarin flight, cost estimates on Project Apollo ranged from $20 to $40 billion. Since NASA would be doing other things in addition to Apollo, it was obvious that NASA would have a substantial impact on the nation's economy. This factor coupled with the need to find new locations for the diverse parts of the complex mission triggered considerable interest on the part of those interested in having such facilities located in their states.

Based on its performance with the Redstone and with the development of *Saturn I,* the von Braun team at Huntsville was the clear choice to take responsibility for developing the launch vehicles needed for Project Apollo. After much debate, it was concluded that Marshall would need a great expansion of test and fabrication facilities. An appropriate pattern had to be worked out to see that it would be able to monitor the activities of contractors engaged in engine and rocket production.

A contract for the Apollo Spacecraft was signed with North American Aviation on November 28, 1961. While some of the STG staff at Langley was working on Apollo, the Project Mercury team was deciding how to extend Mercury in support of Apollo. The final decision was to undertake the Project Gemini in which a larger space capsule carrying two men and to be launched by the Air Force Titan would engage in a series of orbital flights to test out the ability of men to rendezvous and dock spacecraft in space. These skills had to be perfected for Project Apollo because it was unlikely that a rocket capable of providing direct ascent to the Moon would be available soon enough to prove out all the orbital and rendezvous techniques. The announcement of Project Gemini was made on December 7, 1961,[13] although it did not receive that name officially until June 3, 1962. On December 22, 1961, the Gemini contract was signed with McDonnell Aviation.

One major question to be resolved was where the STG, still at Langley, should develop the Apollo mission. It had been assumed that a new NASA research center should be established for that purpose if the Apollo program were officially approved. The STG had been made an independent NASA field activity and renamed the Manned Spacecraft Center on January 3, 1961.[14] Despite the fact that all of these decisions had to be made, NASA did not encourage the development of a site selection competition until after Congress passed the appropriation—which was not until August.

At the time of President Kennedy's Apollo decision, NASA headquarters had an Office of Launch Vehicle Programs and an Office of Space Flight Programs, both reporting to the NASA general manager, the Associate Administrator. Soon after the decision to establish the new field facilities, NASA announced a major reorganization to place its programs on a project basis. In the November 1961 restructuring, all the manned space activities at NASA headquarters, including both launch vehicles and spacecraft, were made the responsibility of a Director of Manned Space Flight. On October 30, 1962, the new position of Deputy Associate Administrator of NASA for Manned Space Flight Centers was established. The first man in this job, D. Brainerd Holmes, was to develop a manned space program office at the headquarters level to supervise the activities of the Marshall Center, the Manned Spacecraft Center, and the Launch Operation Center, as well as those of the thousands of contractors and hundreds of thousands of contractor employees who would be drawn into Project Apollo.[15]

The Apollo Spaceport Decision

The first major facility decision occasioned by the Kennedy decision to go to the Moon concerned location of the spaceport to be used for the launch. The enormity of the facility required indicated that its construction would be a paying item. The extreme expense of a launch facility, the uncertainty as to how big the program would become, and the question as to whether the Department of Defense would be able to make joint use of such a facility led to the establishment of a NASA-DOD team in June 1961. The NASA-DOD team was to review launch requirements and consider where such a facility should be located. Known as the Debus-Davis Report because of its two cochairmen, Kurt Debus, Wernher von Braun's launch specialist, and General Leighton I. Davis, Commander of the Air Force Missile Test Center, Patrick Air Force Base, Florida, which operated the Atlantic Missile Range, the report was presented on July 31, 1961.

In August, the criteria contained in the Debus-Davis Report were applied to a variety of potential sites. After a narrowing down process, on August 24, only three weeks after the report was issued, a final decision was announced. The locations considered by the NASA-DOD team included White Sands, New Mexico; Christmas Island and Hawaii in the Pacific; Mayaguana, a Caribbean island; Brownsville, Texas; Cumberland Island, Georgia; and both an on-shore and off-shore site at Cape Canaveral.[16]

These sites were considered in terms of mission and range support requirements for Cases A (a liquid fueled rocket) and B (a solid or combination solid-liquid fueled rocket). The difference in size, weight, and flight schedules between the different classes of space vehicles resulted in radically different launch complex concepts. The NOVA or solid fueled vehicle concept provided for the erection, checkout, and launch from a fixed Vertical Assembly and Launch Building. The launch complex for a Saturn C-3 liquid fueled vehicle involved a vertical assembly area consisting of a 4-bay vertical assembly building, engineering shops, spacecraft area, electrical ground support instrumentation and a launch control center. The C-3 complex required a launcher transporter assembly which would move the erected and checked out vehicle on racks from the vertical assembly building to the intermediate area for arming and then to the launch pad area for fueling and launching. According to the Debus-Davis report, "vertical assembly and checkout in a centralized vertical assembly building, as compared to vertical assembly and checkout on the pad, is economically and operationally advantageous only if the number of launches exceeds approximately 15 launches per year and are uniformly scheduled over the year."[17]

The comparison of the eight sites listed in order of capital cost is included in Table 7-1.

Table 7-1
Estimated Cost of Potential New Launch Sites
(in millions of dollars)

	Case A	*Case B*
Site 6 – White Sands Missile Range	$ 861	$ 847
Site 1 – Cape Canaveral (on-shore)	876	877
Site 4 – Cumberland Island	922	908
Site 5 – Brownsville	927	922
Site 2 – Cape Canaveral (off-shore)	974	992
Site 8 – Hawaii	1,611	1,589
Site 3 – Mayaguana	2,075	2,026
Site 7 – Christmas Island	2,545	2,459

Source: National Aeronautics and Space Administration and Department of Defense, *Joint Report on Facilities and Resources Required at Launch Site to Support NASA Manned Lunar Landing Program*, July 31, 1961, p. 11.

There were special advantages and disadvantages related to each of the sites. Mayaguana (Site 3), Christmas Island (Site 7), and Hawaii (Site 8) were ruled out not only on the basis of cost of construction but also because their geographic locations would create serious logistical problems and operating costs. Cumberland Island, Georgia (Site 4) and Brownsville, Texas (Site 5) could not be used without periodic closing of the Intracoastal Waterway which would disrupt some commercial enterprises and so cause problems.[18]

The problems of hazards connected with launches was also a serious consideration. Brownsville's launch azimuth could not exceed 80 or 90 degrees without creating potential impact hazards of first and second stages of boosters on land areas. Even so, an aborted flight would endanger major population centers in Florida and even Cuba.[19]

This situation would be even more serious at the White Sands Missile Range (Site 6). The report stated that overflight problems would be serious in Tennessee, Alabama, Georgia, Louisiana, Texas, and Florida and that:

> The first stage for both C-3 and NOVA will impact on land regardless of the launch azimuth within the 60-120 fan. Several cities, such as Big Spring, Midland, Fort Worth, Dallas, Austin, Galveston, and Houston, Texas, are included

> within this fan. To minimize land impact hazards, it would be necessary to severely limit the launch azimuths. Although the second stages of the NOVA would impact in the Atlantic Ocean, the C-3 and second stage impact zone could include portions of eastern U.S.[20]

The report concluded that from a time and cost standpoint the most effective decision would be to expand an existing facility such as White Sands or Cape Canaveral. However, White Sands had another serious drawback in that it required changing some program approaches. Specifically:

> The present concept for transporting large space vehicles on a transport barge from the missile assembly plant to final launch position is economical and allows flexibility in that the missile plant can be located at other than the launch site. Because of available water access, all sites except WSMR can support this concept. Use of WSMR would require some other method of vehicle transportation, and, more important, would dictate that the missile assembly plant and static stands be located near the launch site.[21]

The Debus-Davis Report recommended that the Atlantic Missile Range at Cape Canaveral, Florida, was the most appropriate place to provide the additional launch facilities required for Project Apollo. The Atlantic Missile Range was used for testing rather than operational purposes. The Navy Polaris testing was also done at the Atlantic Missile Range (AMR). The expansion necessitated by the new NASA activity however was substantial since it would involve both operations and testing.

The program called for NASA to take over and operate the rest of the peninsula on which the launch pads for the Atlantic Missile Range were located and also to acquire Merritt Island to the north of the existing facilities. In 1961, the launch area on Cape Canaveral was composed of roughly 15,000 acres used to support the programs of the Air Force, the Navy, and NASA, including the Mercury Program. The launching area could not extend to the east because of the ocean, or to the south or west because of the heavy population concentrations there. The only feasible direction in which to expand was to the north. In all, NASA proposed to acquire 73,000 additional acres at an anticipated cost of $55,000,000.[22]

The additional land adjacent to the launching area and on the same peninsula consisted of roughly 15,000 acres, the same amount which the Air Force already operated. The new 15,000 acres would be used for the early Saturn I and IB flights of Apollo and also for the Air Force Titan III, and so, like the existing area, would be used by both the Air Force and NASA. The cost of expanding to take in the 14,800 acres left on the Cape Canaveral peninsula was estimated at $32 million as shown in Table 7-2.

The additional 73,000 acres on Merritt Island were intended for Apollo flights which were expected to use the Saturn C-3 or the NOVA concept launch vehicles. As planned, the NOVA concept was for a nuclear engine as the second stage.

Table 7-2
NASA Cape Canaveral Cost Estimate

Land acquisition		$27,225,000
Land (14,800 acres at $1,730 per acre)	$25,600,000	
Improvements	1,400,000	
Resettlement Costs	225,000	
Facility construction and modifications		4,250,000
Relocation of Intracoastal Waterway	$ 3,500,000	
New Bridge over Intracoastal Waterway	750,000	
Design and engineering services		525,000
Corps of Engineer administrative services	$ 525,000	
Total Estimated Cost		$32,000,000

Source: U.S. Congress, Senate, Committee on Aeronautics and Space Sciences, *Hearings, NASA Authorization for Fiscal 1963*, 87th Congress, 2nd Session, 1962, p. 189.

Neither of these launch vehicles was ever built. The booster finally selected in 1962 was the Saturn V. This decision did not involve a major reduction in the space needed. Under the original concept to achieve the manned lunar landing goal, a large number of Saturn launches (possibly as many as 48 per year) would be required to accomplish the number of flights envisioned. Three pads would be required for the C-3 vehicle whereas the Saturn V approach required four launch pads. In addition, the spacing of these pads from each other and from inhabited areas had to be greater since the Saturn V used more propellant than the C-3 was intended to use, and consequently, posed greater potential hazards.

Management of the NASA launch operations had been assigned to a Launch Operations Directorate (LOD) of the Marshall Space Flight Center under the direction of Dr. Kurt Debus in 1960. In 1962, after the NASA program developed further at Cape Canaveral, the LOD was made independent of the Marshall Center and redesignated the Launch Operations Center to reflect its actual relationship as a supporting service for launches of all NASA programs.[23] In addition to providing a more integrated launch control operation, this decision to establish an independent center at Cape Canaveral also had the effect of providing NASA with an entity at the Cape on a level comparable to that of the Air Force Missile Test Center. Other NASA centers such as Goddard Space Flight Center, engaged in the scientific programs of NASA as well as the programs involving weather and communications satellites, were also using Cape Canaveral. They had their own launch operations groups there, but logistical support and most of the active launching was provided by the Launch Operations Directorate.

Dr. Debus was named Director of the Launch Operations Center, although obviously he still had extremely close ties to the Marshall Space Flight Center and its Director, Dr. Wernher von Braun. On December 20, 1963, this center was redesignated the John F. Kennedy Space Center. On November 29th President Johnson had renamed the space facilities at Cape Canaveral the John F. Kennedy Space Center.[24] The latest change made all the nomenclature compatible. The headquarters for the Kennedy Space Center was located on Merritt Island as was the launch Complex 39 intended to fire the Saturn V for Project Apollo, the Vertical Assembly Building where it would be assembled, the Command Control Center, central telemetry station, radar installations, and industrial areas.

The total cost of NASA construction at Cape Kennedy from 1959-1970 was just under one billion dollars.[25] The construction cost of launch complex 39 alone was over $488 million, distributed as indicated in Table 7-3.

On February 7, 1962, President Kennedy sent a message to Congress proposing supplemental appropriations for fiscal year 1962, which would end on June 30th. Included in the package was an additional appropriation to NASA of $156,000,000–$85,000,000 of which was to be for research and development and $71,000,000 for the land acquisition programs, including those at Cape Canaveral.[26]

There was no special controversy regarding this location decision. The initial commitment to the area had been made by the Air Force in 1948. NASA had already used the Canaveral facilities for Project Mercury and there was room for expansion. The location was a good one to achieve the kinds of earth orbits NASA anticipated needing for most of its programs. There was some slight question of the possible effects of tropical storms on the massive towers and assembly facilities, but this was not considered serious. In addition, the weather would permit year-round launches, a necessity for the spaceport selection, since a space vehicle might be kept on the pad longer than anticipated. Selection of any location other

Table 7-3
Estimated Funding Requirements, Launch Complex 39
(in millions)

Item	*Current Estimated Total Funds Required*
VAB and Area Facilities	$158.9
Launch Areas & Crawlerway	67.2
Mobile Service Structure	26.6
Communications and TV	29.3
Mobile Launchers	33.6
Crawler Transporters	14.9
Propellant Services	67.4
Firing Accessories	68.1
Instrumentation & Equipment	22.7
Total	$488.7*

*Costs for basic facility only.

Source: National Aeronautics and Space Administration, Office of the Director of Facilities, 1970.

than Cape Canaveral or White Sands would have required duplication of some existing facilities, further inflating the already astronomical costs.

Clearly, this was a good management decision concurred in by the Department of Defense and NASA. In the context of the trials and the threat to national security, criticism would have been the height of political folly. If any political moves in favor of other regions had been planned, the rapidity of the Debus-Davis Report's preparation and adoption hardly allowed sufficient time to mobilize such a movement. Further, it was clear there would be other opportunities to compete for NASA facilities and some of these would not involve the safety and noise problems of the spaceport.

Selecting the Spacecraft Center

NASA also had to move fast on its other facility decisions. However, backed by the vote in Congress to move ahead with Project Apollo and the strong support of President Kennedy who had put the nation's reputation on the line by selecting a target date of 1970 for completion of Project Apollo, NASA was in a position to move fast. There really was no time for prolonged consideration of potential sites and support for the program was so strong it would have been hard to criticize NASA for moving rapidly on the decisions.

The Manned Spacecraft Center was the pearl of the required new facilities. The locations considered as a potential site for the Space Task Group to be moved from Langley Field were Boston, St. Louis, Jacksonville, Tampa, four sites in Louisiana (New Orleans, Baton Rouge, Shreveport and Bogalusa), eight in Texas (El Paso, Dallas, Corpus Christi, Houston, Beaumont, Harlingen, Liberty and Victoria), and six in California (Los Angeles, Berkeley, San Diego, San Francisco, Richmond and Moffett Field). Twenty-two cities were considered, but in the end Houston was selected.[27]

The reasons given for the selection of Houston were:

1. It had a deep channel and port facilities second only to those of New York. This would be significant since it was anticipated the space program would involve moving large rocket and spacecraft components and associated equipment for testing and checkout.

2. Houston was also well located with respect to the Marshall Space Flight Center and Cape Canaveral. Most of the contractors working on Project Apollo were located in California and in major metropolitan areas. The Houston International Airport which was under construction would provide good connections to all major United States cities and on a year-round basis because of the temperate climate.

3. Houston had a substantial industrial community, although it was concentrated in the petrochemical industry. It had about 58 percent of the

nation's oil capacity, 32 percent of its petroleum, and about 75 percent of its petrochemicals.

4. From the standpoint of university support and proximity of scientists and researchers, Houston could boast that it had Rice University, one of the most prestigious private universities in the nation, as well as the University of Houston and the Texas Medical Center of which the Baylor University Medical School was a part.

5. Houston had a population of just under a million (seventh largest in the nation), a stronger growth rate than many of the larger cities, and a work force of over 500,000.

6. Rice University donated 1,020 acres of land as part of the Houston proposal to obtain the space center.[28]

Some interesting dealing surrounded the land contributions. The 1,020 acres of land donated by Rice University was given to the University by Humble Oil Company with the proviso that the land would be made available to NASA for the laboratory. Humble's West Ranch surrounds the NASA site on two sides. After the Houston selection was made, Humble Oil & Refining Company and Del E. Webb Corporation of Phoenix and Los Angeles formed a joint company to develop a $500 million residential and commercial community on the 30,000 acre West Ranch property.[29]

In a congressional hearing the spring following the location decision it was brought out that NASA needed more land to service a more efficient grouping of its buildings, and to establish an antenna test range and hazardous test area. Rice approached Humble Oil for an additional 600-acre donation. However, Humble would not donate any more land and also "Humble would not sell land, that they could not afford, for their own reasons, to receive a cash consideration for it, but that they would consider negotiating an exchange with Rice of land which would be of value to them."[30]

After considerable negotiation, NASA agreed to pay Rice $1.4 million for the 600 acres and Rice bought additional land which it traded to Humble Oil for the 600 acres adjacent to the original 1,020 acre tract. The 600 acres was appraised before the location of the Manned Space Center at $692,875 and at $1,246,175 after the announcement. A second appraisal was $751,000 before the announcement and $1,021,000 after it. A third appraisal was $635,000 before the announcement and $720,000 later. The final price of $1,400,000 was thus 12.4 percent above the highest appraisal. However, Rice University had not intended to donate the first 1,020 acres but rather offered it for NASAs use subject to being returned to Rice when NASA no longer needed it. In addition, Rice had expected to receive some rent during NASAs occupancy. However, NASA legal officials would not agree to build federal buildings on land it did not own and felt that a long-term rental was not in NASAs best interests. If NASA had tried to condemn the land, it is probable that Rice, a nonprofit local institution, would have been awarded even more for it. The price on the additional 600 acres was therefore quite justifiable.[31]

Clearly, Houston had sufficient credentials to be selected for the Manned Spacecraft Center. The speed with which the decision was made and announced on September 19, 1961, did not permit many areas in the nation to put together a strong proposal. Although some undoubtedly felt that they had been outflanked and outgunned, a full public comparison of the merits of the various sites was not available for preview and analysis.

Just after the former Space Task Group physically moved from Langley Field to Houston, President Kennedy made a tour of the new space crescent, extending from Cape Canaveral to Houston, created by the manned space facilities. He was greeted with a wild ticker tape reception and then spoke to over 50,000 in Houston's Rice Stadium. He discussed the meaning of the NASA Center for Houston and declared that in its first five years of existence the NASA Center would "double the number of scientists and engineers in this area–increase its outlays for salaries and expenses to $60 million a year, invest some $200 million here in plant and laboratory facilities, and direct or contract for new space efforts at a rate of $1 billion a year from this space center alone."[32]

The Houston boosters who made the strong proposal to have the Manned Spacecraft Center (MSC) brought to Houston were well aware of this economic potential. They were not disappointed as NASA quickly purchased the 600 additional acres to add to the 1,020 donated by Rice University and "by August, 1963, the Center was spending over $1 million a month in contracts with some 500 local firms and . . . had already spent $5,342,111 in Houston, which was more than two-thirds of the sum it had spent in Texas at large–$8,202,545. In addition, the center also had committed over eighty million more dollars in current or active contracts with firms in the Houston area, and with some in San Antonio, Dallas, Fort Worth and El Paso."[33]

Nationally, MSC contracted for several billion dollars of research and development related to Project Apollo. Texas in general did very well not only on prime contracts, but also on subcontracts. Contracts for design and construction of the facilities–where local firms have a distinct advantage–were especially fruitful for the Texans.

The University of Houston's Bureau of Business Research predicted that:

> within two decades the Clear Lake Development taken collectively, would create technical and research jobs for 70,000 people, would attract some 207,000 new residents, and would add to the area's economy $406 million in personal income, $182 million in bank deposits, $140 million in annual property taxes, and $245 million in increased retail sales.[34]

Under NASAs decentralized system, much of the contracting for Projects Gemini and Apollo would be done directly from Houston. This led many major industrial firms to establish Houston offices. In addition, many firms receiving contracts established liaison offices at Houston to coordinate their contract administration. Over 100 aerospace firms located there in the first three years.[35]

Ninety-two percent of the 1,150 scientists, engineers, and technicians of the Space Task Group actually made the transfer to Houston.[36] This is striking because although the STG was leaving Langley Field, the complement of scientists, engineers and technicians associated with the advanced research programs at Langley was expanding at a rapid rate and many of those who left for Houston could have stayed to work on those projects. It is doubtful that the transfer of any other major activity was so complete. The team spirit and attraction of the project to those associated with it is obvious.

There were some allegations of political favoritism based on the power of key Texans in Washington. At the time of the decision, Vice-President Lyndon B. Johnson was serving as Chairman of the National Aeronautics and Space Council. While serving as majority leader, Senator Johnson had sponsored the Space Act when it passed the Senate in 1958 and then served as the first chairman of the Senate Aeronautical and Space Sciences Committee. Upon becoming Vice-President, he had prevailed upon President Kennedy to have the law amended to provide that the Vice-President rather than the President should serve as chairman of the National Aeronautics and Space Council. His interest in both space and Texas was obvious.

Equally important, Representative Albert Thomas of Houston was the chairman of the Subcommittee on Independent Offices and the fourth ranking Democrat on the Appropriations Committee. Thomas was one of the most powerful appropriations subcommittee chairmen in the House, having been chairman since 1949. He was considered one of the most powerful men in the House of Representatives and this was clearly fine as far as NASA was concerned.

The Speaker of the House of Representatives was Sam Rayburn of Bonham, Texas. The number three Democrat on the House Committee on Science and Astronautics was Representative Olin E. Teague of College Station, Texas, who was to become the next chairman of the subcommittee on manned space programs. Representative Bob Casey, also of Houston, was the number nine Democrat on the House Space Committee. Further, Representative George Mahon of Lubbock, Texas, was slated to become chairman of the House Appropriations Committee when 85-year old Clarence Cannon of Missouri should leave that post. Clearly, Houston's political credentials were at least as imposing as its environmental conditions and locational statistics. The image of political power was not lessened any by the circumstances of the announcement. The selection was first made known by Representative Thomas at a press conference in the Houston office of Vice-President Johnson.[a]

---

[a] Congressman Thomas's power in Houston was just as impressive as his power in Washington. After five years on the House Space Committee, Congressman Bob Casey, who

The Launch Vehicle Fabrication Site

Although the Marshall Space Flight Center at Huntsville would be responsible for the launch vehicle for Project Apollo, the fabrication facilities at Marshall were not adequate. From the standpoint of coordination and supervision, there would be advantages to having the fabrication done at Marshall. However, much of the work would have to be contracted out to the aerospace industry. In some cases, the contractors already had appropriate facilities available, many of which had been built with federal money. Further, Huntsville was already growing at a very rapid rate because of the expansion of the Marshall Center and there was some question about how well it could absorb an even greater rate of growth.

An appropriate solution to the various dilemmas involved in locating the fabrication facility was announced on September 7, 1961, with the selection of an unused government manufacturing plant at Michoud, Louisiana, 15 miles east of New Orleans and 400 miles from Marshall Space Flight Center. The plant had approximately 1.8 million square feet of manufacturing space, with a ceiling clearance of 40 feet. The site contained 825 acres. One of its buildings was at that time the largest one-floor space in the nation, having 43 acres under one roof.[38]

The plant had been built at the end of World War II but was not used much until the Korean War when it served as a tank assembly plant. The federal government had not yet derived appropriate benefit from its investment in this facility. By renovating an existing plant, NASA would not have to pay its contractors to build new facilities or to make modifications on existing ones. In addition, it would simplify the supervision problem for Marshall Flight Center since the plant was large enough to accommodate several contractors working on the different but interrelated parts of the launch vehicle.

There were 850 additional acres of government land adjacent to the site which could be used if further expansion should become necessary. Another major consideration with regard to the plant and its location was that the main line of the Louisville & Nashville Railroad and U.S. Highway 90 (Jacksonville to San Diego) both ran through one corner of the land. There was also a 5,500-foot-long airstrip on the site.[39]

---

still serves another part of Houston, resigned from the Space Committee for a new post on the Appropriations Committee. This was a surprising resignation since Casey had moved up in seniority to eighth among the Democrats on the Committee and needed to move only three more spots to become a subcommittee chairman. Capitol Hill sources indicated that he felt no matter what NASA ever did in Houston, the newspapers and media would attribute it all to the power of Albert Thomas rather than to any influence by Representative Bob Casey. A congressman needs a way of staying in the public eye and Casey apparently decided to move to a committee where he would not be in Thomas's shadow. Ironically, Congressman Thomas died early in 1966.[37]

The great bulk of the boosters made it difficult to transport them so that water transportation was considered to be necessary both to Cape Canaveral and to the testing facility where the newly assembled boosters could be test fired. Michoud is directly on the Gulf Coastal Waterway with access to the Gulf of Mexico through the Mississippi River.

Under the NASA reprogramming authority, $980,000 of construction money was reprogrammed September 25, 1961, to initiate the modification of the Michoud Ordnance Plant for fabrication of the Saturn and Advanced Saturn booster stages. On January 23, 1962, an additional sum of $460,000 was reprogrammed from the research and development appropriation for this purpose.[40] A third reprogramming was announced April 23, 1962, when $4,748,000 was made available for further modifications. The April 23rd reprogramming letter also included $500,000 to start work on a hydrostatic test and cleaning facility and $665,000 for work on the vertical assembly building. The full cost for these items had been included in the fiscal year 1963 budget presented to Congress in January 1962, but this had not yet been acted upon. An additional reprogramming of $2.8 million in R&D money into construction for Michoud modifications was made later in fiscal year 1962.[41]

On December 26, 1962, a NASA letter requested the transfer of $2,800,000 of fiscal year 1962 research and development funds to begin additional modification of the Michoud plant for the S-IC vehicle stage, a project which was expected to total $9,417,000. Another $6,617,000 was reprogrammed at the same time from fiscal year 1963 research and development funds.[42] From fiscal year 1962 through fiscal year 1965, approximately 53.5 million dollars was spent to modify and add new facilities at Michoud. The plant was operational in 1965.[43]

The capital investment of the Michoud plant as of June 30, 1965, was over 103 million dollars and facilities at the main plant had over three million gross square feet.[44] NASA added a new engineering building in 1965 as well as a high bay vertical assembly area, a hydrostatic test facility, and a stage check-out facility. The facility was occupied by the Chrysler Corporation which had the contract for building, assembling and checking out the Saturn I and Saturn IB first stages and the Boeing Company which had a similar contract for the S-IC stage for the Saturn V.

The $4,748,000 included in the April 23rd letter earmarked $390,000 for data processing and computer equipment for the Michoud plant. However, this money was later transferred to another facility located at Slidell, Louisiana. On August 6, 1962, NASA announced that it had taken over from the Federal Aviation Agency (FAA) a surplus air traffic control facility built at Slidell. Located on a fourteen acre tract twenty miles northeast of Michoud, the Slidell facility was to be used as a computer facility in connection with the Saturn I and II stages. It would also provide computer capability for operations at the testing facility. NASA gained the use of 59,497 square feet to house the computer installation but later found it necessary to add another 30,000 square feet for the operation.[45]

In the 1965 hearings, it was reported that the four private contractors at Michoud had hired 10,000 personnel for the operation and that NASA also had 290 employees there. This was a payroll of over $70 million. The New Orleans Chamber of Commerce estimated that the work at Michoud increased employment in other industries by about 6,500 and retail sales by over $33 million a year.[46]

The biggest single gainer among local firms because of the Michoud decision was the Mason Rust Company. After evaluation by a source evaluation board for a contract to provide housekeeping services at Michoud, this New Orleans firm was selected competitively from among 36 bidders. The original contract was for six months with an option for one additional year and NASA exercised its option before the six months expired. In addition to the housekeeping contract, Mason Rust was awarded a contract to supervise construction at Michoud and at the time of the 1964 hearings had won NASA contracts totaling $29,152,573 at Michoud. The contracts were for a fee of approximately 2.6 percent, which amounted to $734,794.[47]

The selection of Michoud was reasonable in terms of providing access to services in the New Orleans area, effective water transportation systems for moving the vehicle stages, and reasonable proximity to Marshall Space Flight Center which would supervise the work of the contractors. Again, however, one cannot help but consider that New Orleans was one of the areas passed over in favor of Houston for the Manned Spacecraft Center and, like Houston, could claim some powerful political sponsorship.

The chairman of the House Committee on Science and Astronautics was Representative Overton Brooks of Shreveport, Louisiana who died September 16, 1961. Representative Hale Boggs of New Orleans was serving as House Democratic whip at the time of the selection. On the Senate side, Senator Allen Ellender served on the NASA Appropriations Subcommittee and was the third ranking Democrat in Appropriations. Senator Russell Long of Baton Rouge, Louisiana, who became chairman of the Senate Finance Committee when Senator Robert Kerr gave up that post to take the chairmanship of the Senate Aeronautical and Space Sciences Committee, was a powerful Senator who opposed the NASA patent policies. On the other hand, the decision to place the facilities at Michoud and Slidell did not prevent Long from being one of the major opponents of the Communications Satellite Act of 1962 which NASA supported.

Locating the Testing Facilities

In addition to the fabrication plant at Michoud, Marshall Space Flight Center needed a major test facility so that the large launch vehicle stages of the

Saturn I, Saturn IB, and Saturn V could be checked out and integrated with a minimum loss of time. Because of their great bulk, there was a transportation problem with these stages. A location was needed which was not too far from Michoud and had good transportation connections with it. The facility selected also had to have a significant buffer area because of the noise and danger involved with the testing program.

On October 25, 1961, NASA announced the selection of a facility to be known as the Mississippi Test Facility (MTF). The location was approximately 30 miles from Michoud in Pearl River and Hancock Counties, Mississippi, with some part of the buffer area located in St. Tammany Parish in Louisiana. The site was only 40 miles from New Orleans. The land involved 13,500 acres for the facility and easement rights to about 128,000 acres which was to be used as a buffer zone. Farming and lumbering would be permitted to continue within the buffer zone on which NASA obtained easements.[48]

The selection of the Mississippi Test Facility (MTF) was announced six weeks after the Michoud decision and again part of the philosophy in that decision was that a government-owned and operated central test facility, located reasonably close to Marshall and to Michoud, would be in the government's best interest. It would mean that no contractor would gain control and thereby place other contractors at a disadvantage in future contract competitions. This facility had a water connection to Michoud and so was ideal for the purpose. To prepare the water connection to Michoud, NASA had to improve 15 miles of river channels and construct another 15 miles of canal 200 feet wide and 15 feet deep to provide water access to the East Pearl River and the Intra-Coastal Waterway. Coming into the facility, barges could move from the Pearl River into a canal, then through a lock up to the level of the Mississippi Test Facility, and finally to a network of canals located throughout the MTF. Water transportation was to be used not only to move hardware from Michoud to the MTF but also for moving such items within the facility.[49]

On November 28, 1961, NASA sent a reprogramming letter to Congress requesting authority to transfer $3,500,000 from the research and development fund account to the account to be used for purchasing MTF land. It was estimated at the time that the total cost of land would be about $16 million.[50]

NASA expected that the new construction requirements for the Mississippi Test Facility would total approximately $61,991,000. The two major items involved were an S-IC static test stand to be used for acceptance testing production stages at an approximate cost of $26,384,000 and an S-II static test stand for acceptance testing of production stages which was estimated to cost $20,535,000. Other requirements included $5,499,000 for a component service facility to provide laboratory and shop facilities necessary on the static testing project and an extension of the road network, as well as communication systems and maintenance, at an approximate cost of $9,533,000.[51]

Operation of the test facility did not require a very large NASA staff. NASA employment at the MTF never exceeded 150, a figure which was reached in

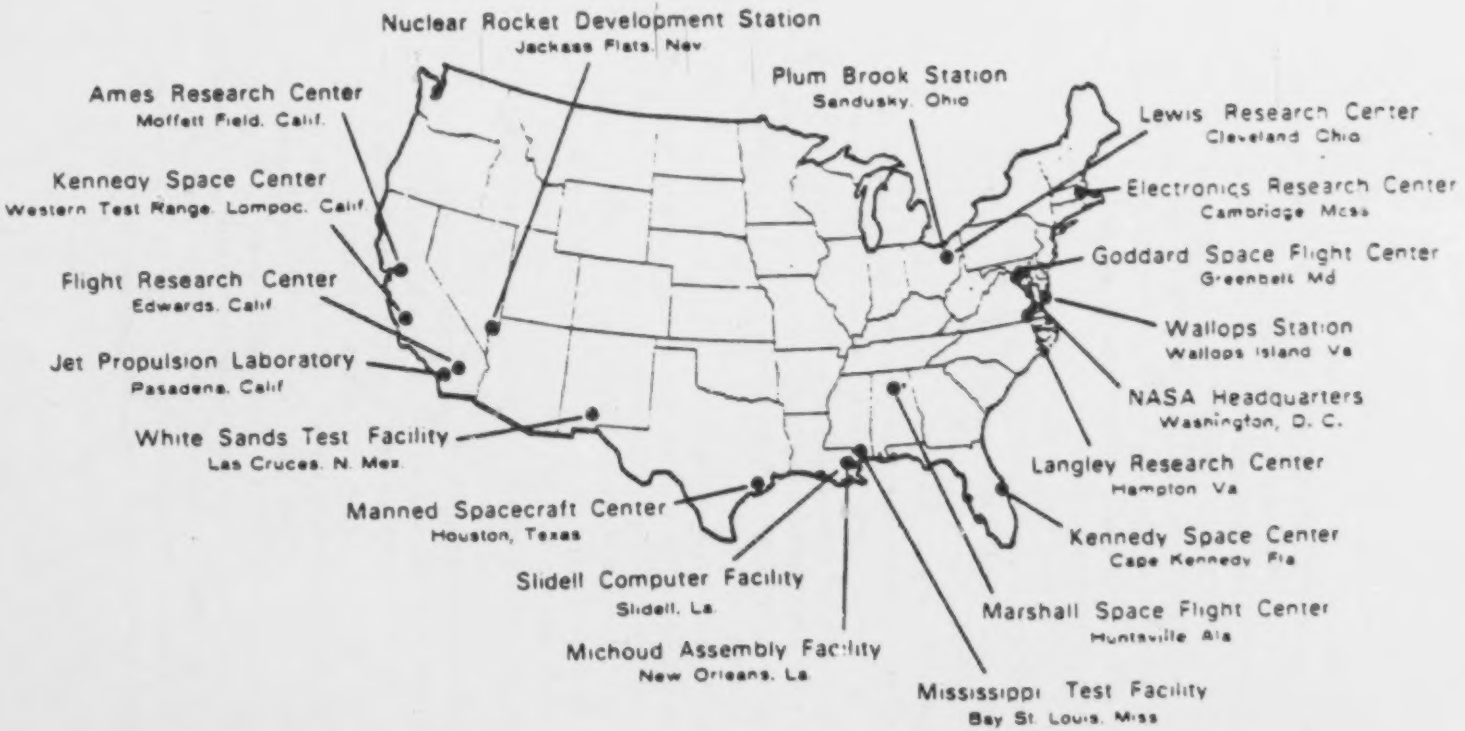

Nuclear Rocket Development Station
Jackass Flats, Nev
Plum Brook Station
Sandusky, Ohio
Ames Research Center
Moffett Field, Calif.
Kennedy Space Center
Western Test Range, Lompoc, Calif.
Flight Research Center
Edwards, Calif
Jet Propulsion Laboratory
Pasadena, Calif
White Sands Test Facility
Las Cruces, N. Mex.
Manned Spacecraft Center
Houston, Texas
Slidell Computer Facility
Slidell, La.
Michoud Assembly Facility
New Orleans, La.
Mississippi Test Facility
Bay St. Louis, Miss
Marshall Space Flight Center
Huntsville Ala
Kennedy Space Center
Cape Kennedy Fla
Langley Research Center
Hampton Va
NASA Headquarters
Washington, D. C.
Wallops Station
Wallops Island Va
Goddard Space Flight Center
Greenbelt Md
Electronics Research Center
Cambridge Mass
Lewis Research Center
Cleveland Ohio

Figure 7-1. NASA Installations January 1968. Source: Office of the Administrator, National Aeronautics and Space Administration.

1967. The peak employment at MTF reached 6,400 in August, including 1,700 construction workers. Most of the continuing employees at the facility were on the payroll of the General Electric Company. This total reached a high of 1,870 in June 1965, and dropped off to 1,180 by April 1970. General Electric had been selected by NASA in 1962 as the Apollo integration contractor. That is, General Electric was to be in full charge of the checkout, reliability assessment, and integration support for Project Apollo.[52]

This contract selection was made by the top three officials in NASA acting as a team. Associate Administrator Dr. Robert C. Seamans, Jr. stated "We have examined the companies which are not competitive in the booster business and selected General Electric because this firm has the capability of doing the inventive work needed to increase reliability through new instrumentation and storage of data from this instrumentation, and if necessary, of operating selected test facilities for NASA."[53] The basic General Electric contract was extended therefore to include responsibility for plant and test support services at Mississippi Test Facility. From 1963 through 1969, General Electric was paid $180 million under this contract for the operation of MTF.[54]

By the end of fiscal year 1966, capital investment at Mississippi Test Facility reached $216 million. This included the testing complex consisting of one Saturn V dual position test stand and two single position second stage test stands. The test support facilities included a test control center, data acquisition center, propellant facilities, water supply system, fuel transfer and storage facility, and other supporting laboratories. The navigation locks, bridges, canal system and road and railroad networks were also completed by the end of 1966.[55]

The Mississippi Test Facility was zoned into five functional areas, of which the Saturn V test complex was the most imposing. It included a dual position first stage (S-IC) test stand, two second stage (S-II) test stands, a test control center, and data acquisition facilities, as well as the fuel and water supplies. It was at this facility that the stages of Saturn V used to power the first flight to the moon were tested out by Marshall Space Flight Center with the support of the General Electric Company. Essentially, all the construction authorized for MTF was completed in 1966.

The selection of Mississippi Test Facility was logical following the selection of Michoud. The location was appropriate and the water transportation facilities were outstanding. The United States Corps of Engineers reported on the core borings that the foundations of the land were among the best on the entire Gulf Coast.[56] In addition, the area was very sparsely settled so that it would be feasible to buy easements for the necessary buffer zone at a relatively reasonable price. As in the case of the other major NASA facilities, the Corps of Engineers was called upon to secure the land for NASA and handle the construction contracts.

From a political standpoint, the key man in Mississippi was Senator John Stennis who served both on NASAs Subcommittee on Appropriations and on the Armed Services Committee. The economic advantages of the MTF area were not on as significant a scale as those which the Michoud, Marshall, Houston and Cape Kennedy facilities brought to the surrounding areas. Essentially, no contracts other than for construction were let from MTF as the work was done at Michoud or Marshall and taken to MTF solely for testing in a deserted area where the noise level and fire danger would not be a problem.

Nevertheless, there was some minor land speculation and, of course, the owners of the land which was not being used made some money from the easements which NASA purchased. Owners of sites purchased outright also made out fairly well. Nevertheless, there were a number of problems for Senator Stennis. While he was able to take credit for bringing the facility to Mississippi, on the other hand, there were some hardship cases involved in the relocation of homes and families and many of the persons affected did not feel that the prices being certified by the Corps of Engineers were equitable. This involved the Senator in numerous meetings and voluminous correspondence with NASA, but this is a standard procedure in any major federal land acquisition program.

The Nuclear Rocket Development Station

Another facility agreement entered into by NASA in the summer of 1961 involved an expansion of an earlier agreement with the Atomic Energy Commission. In 1960, NASA and AEC established a Nuclear Propulsion Office at AEC headquarters in Germantown, Maryland, but headed by a NASA employee. The work of this office related primarily to NASAs Lewis Research Center at Cleveland and to AECs Los Alamos Scientific Laboratory at Albuquerque. As the decision to go to the Moon was reached, it was not fully clear what propulsion system would ultimately be used. It was anticipated that in the long run major flights would involve nuclear technology but chemical rockets were sure to be ready earlier.

The AEC-NASA agreement was strengthened in July 1961, when the office was renamed the Space Nuclear Propulsion Office (SNPO) of AEC and NASA. The primary mission of the office was the Rover Nuclear Rocket Project. Field offices at Albuquerque and Cleveland were activated. As was the custom of both AEC and NASA, most of the developmental work on the nuclear rocket was done under contract. However, as in the case of the Apollo Project, the establishment of test facilities required special arrangements.[57]

In February 1962, NASA announced the establishment of the Nuclear Rocket Development Station at AECs Nevada test site. Another SNPO office was established at Las Vegas to manage the station, located at Jackass Flats approximately sixty miles northwest of Las Vegas. Although these facilities were being used very heavily and really did not have any surplus capacity, the nuclear rocket project was given a priority and the testing was done there to avoid the need to create a new testing facility with the attendant cost and time delays.[58]

The office at the test site was activated in October 1962, and in November, NASA found it necessary to reprogram seven and a half million dollars of research and development funds into the construction appropriation to supplement seven million dollars previously appropriated for a test stand there. The final engineering studies indicated that $14,650,000 would be the total cost. The increase was due to a decision that some of the existing equipment which NASA had hoped to use was not usable. In addition, the program advanced to the point where it was obvious that the heavy test schedule would require two fully equipped test stands.[59]

From a political standpoint, the Las Vegas area probably did not gain much from the decision to do the NASA testing there. If NASA had not moved in, the AEC probably would have done more work there so that the net effect was not too great a change. However, Senator Howard Cannon of Nevada was on the Senate Space Committee and certainly this was not a deterrent to NASA putting the facility there, although AEC was far more important to the Senator. After the decision to use the Nevada test site was recommended to NASA by the head of the SNPO office, AEC almost had to agree to make the site available. If it had not, AEC would have had a major voice in determining what other location might be used since it would have primary responsibility for implementation of the testing program developed by the SNPO and its contractors.

Summary

There are a few cases of circumstantial evidence suggesting political considerations in the facility decisions. However, there were some powerful political figures in virtually every major metropolitan area where NASA might have put a facility. The series of location decisions led to an integrated system of facilities and in none of the cases involved did NASA select an unlikely or unsuitable site. These factors must be given great weight in evaluating the extent of political influence. Houston, Michoud, Slidell and Mississippi Test Facility were all well located for the purpose and, as shown in Figure 7-1, tied in well with the Marshall Space Flight Center at Huntsville. The von Braun group had been located at Huntsville by the Army before NASA was created. The Nuclear Rocket Development Station and Cape Canaveral were facilities decisions in which NASA was heavily influenced by the prior existence of a facility and the respective projected needs of the Atomic Energy Commission and the Department of Defense.

A significant point with regard to the rapidity of the decision on Houston and the other manned space facilities was the ability NASA had to reprogram money into construction without securing specific legislation. Because of the uncertainty and lack of knowledge regarding the costs and time involved in doing some of the things NASA would be trying to do, most of which were unprecedented and therefore had no performance records, NASA was given special authority by its authorization and appropriation acts to reprogram up to three percent from its research and development budget into construction funds and to reprogram as much as five percent of its construction money into research and development. As the scope of the construction program declined, reprogramming from R&D to construction was later restricted to two percent in 1963 and to one half of one percent in 1965, but these sums still amounted to a substantial portion of the construction budget and so provided considerable flexibility. In addition, initially all money other than administrative operations money appropriated to NASA was "no-year" money which did not have to be spent within any specific fiscal year.[60]

This alone would not have been enough for NASA to do what it did. The flexibility provisions could have been interpreted to mean that additional money could be added to construction projects or taken away from them for R&D projects only if those construction and R&D projects had been specifically authorized by Congress. Instead, the approach used was that NASA could reprogram for emergency construction purposes as long as the facility in question had not previously been denied by Congress. The NASA authorization acts provided that NASAs committees had to be notified of the use of over $250,000 for construction not previously authorized. The facilities later located at the Manned Spacecraft Center at Houston and the Mississippi Test Facility had been authorized in 1961 on a "site to be determined" basis. However, they exceeded $250,000 so that NASA used its emergency reprogramming powers, subject to a veto of the House and Senate Space Committees, to move on with the projects while awaiting regular appropriation approvals.

Under the law, NASA was permitted to propose reprogramming the money provided it stated the reasons necessitating such reprogramming in letters to the Speaker of the House and the President of the Senate for transmittal to the chairmen of the two Space Committees. Those Committees were to have thirty days to review and object to the reprogramming. In the absence of such rejection, NASA would be authorized to proceed. Through its liaison contact with the Committees, NASA generally knew before the required time passed whether there would be an objection. Of 130 official reprogramming actions submitted to Congress from 1958 to 1969, virtually all were approved as submitted. All were "officially questioned" by the committee staffs or by the committee members and several were approved only after further explanations and documentation were provided by NASA.

It might also be noted that NASA evaded the potential involvement in time-consuming but potentially politically rewarding matters such as the construction and architect contracts, preferring to have these matters handled by the U.S. Army Corps of Engineers—which, of course, was more experienced and capable of evaluating the quality of such work. The Corps' strong position with the Congress was an added asset.

At any rate, NASA made these major facility decisions in a short period of time. This was possible only because of a fortuitous combination of dedicated rofessional people in NASA, White House support, strong legislators who con- inually urged NASA forward, a receptive public opinion environment, coopera- ion from the Corps of Engineers, and top management in NASA which knew how o drive forward while orchestrating these other elements of success. Politics for nce was turned into the handmaiden of professional management.

Notes

1. Stephen B. Oates, "NASA's Manned Spacecraft Center at Houston, exas," *Southwestern Historical Quarterly*, January 1964, p. 351.

2. *National Aeronautics and Space Act of 1958*, Public Law 85-568, 85th ongress, July 29, 1958; 72 *Stat.* 426; and 42 U.S.C. 2451.

3. Executive Order 10783, 23 *F. R.* 7643.

4. Robert L. Rosholt, *An Administrative History of NASA, 1958-1963,* lational Aeronautics and Space Administration, 1966, pp. 45-46.

5. Ibid., p. 47.

6. Ibid.

7. Ibid., pp. 108-123.

8. Ibid., pp. 69-70.

9. Hugo Young, Bryan Silock, and Peter Dunn, "Why We Went to The Moon," *he Washington Monthly,* April 1970, p. 34.

10. Ibid., p. 35.

11. U.S. *Congressional Record,* May 25, 1961, p. 8881.

12. Rosholt, *Administrative History,* p. 114.

13. NASA, *News Release 61-273,* December 7, 1961. See also Rosholt, *dministrative History,* p. 238.

14. Rosholt, *Administrative History,* p. 124; NASA, *Fifth Semi-Annual eport,* p. 153; and NASA, *News Release, 61-1,* January 3, 1961.

15. Rosholt, *Administrative History,* pp. 255-7.

16. Ibid., pp. 214-15.

17. National Aeronautics and Space Administration and Department of efense, *Joint Report on Facilities and Resources Required at Launch Site to upport NASA Manned Lunar Landing Program,* July 31, 1961, p. 4.

18. Ibid., pp. 10-11.

19. Ibid., p. 13.

20. Ibid.

21. Ibid., p. 11.

22. U.S. Congress, Senate, Committee on Aeronautical and Space Sciences, *earings on NASA Authorization for Fiscal Year 1964,* 88th Congress, 1963, p. 59.

The exact acreage will vary as easements expire and new boundary surveys are completed. As of 1970, NASA records indicate ownership of 83,783 acres on Merritt Island at a cost of $71,018,057. *Letter,* Ralph E. Ulmer, NASA Hq., dated April 20, 1970.

23. Rosholt, *Administrative History,* pp. 124 and 251.

24. Ibid., pp. 124 and 302.

25. National Aeronautics and Space Administration, Budget Operations Division, "Construction of Facilities Fiscal Years 1959-1970," *Status of Approved Programs,* March 31, 1970.

26. U.S. Congress, Senate, Committee on Aeronautical and Space Sciences, *Hearings on NASA Authorization for Fiscal Year 1963,* 87th Congress, 1962, p. 13.

27. Stephen B. Oates, *Manned Spacecraft,* pp. 350, 354-5; Loyd Swenson, James Grimwood, and Charles Alexander, *This New Ocean,* Washington, D.C.: NASA, 1966, p. 587; and James Grimwood, *Project Mercury,* Washington, D.C.: NASA, 1963, p. 147.

28. Oates, *Manned Spacecraft,* p. 355 and "Houston, Boomingest Town in the U.S.," *Newsweek,* June 11, 1962, pp. 78-84.

29. "Space Age Zip for Booming Houston," *Business Week,* January 27, 1962, p. 35.

30. U.S. Congress, Committee on Science and Astronautics, *Hearings on NASA Authorization for Fiscal Year 1963,* 87th Congress, 1962, pp. 888-890.

31. Ibid., pp. 884-892.

32. Oates, *Manned Spacecraft,* p. 360.

33. Ibid., pp. 365, 370.

34. Ibid., pp. 374-75.

35. Ibid., p. 372.

36. Ibid., p. 356.

37. John W. Finney, "Houston is Picked for Space Center," *The New York Times,* September 20, 1961.

38. U.S. Congress, Senate, Committee on Aeronautical and Space Sciences, *Hearings on NASA Authorization for Fiscal Year 1963,* 87th Congress, 1962, pp. 102, 163 and 475.

39. Ibid., p. 475.

40. U.S. Congress, Senate Committee on Aeronautical and Space Sciences, *Hearings on NASA Authorization for Fiscal Year 1964,* 88th Congress, 1963, p. 64.

41. Ibid. and National Aeronautics and Space Administration, Budget Operations Division, "Construction of Facilities – Fiscal Years 1959-1970," *Status of Approved Programs, Fiscal Year 1962,* March 31, 1970, p. 4.

42. U.S. Congress, Senate, *Hearings,* p. 64 and U.S. Congress, House Committee on Science and Astronautics, Subcommittee on the Manned Space Flight, *Hearings on NASA Authorization for Fiscal Year 1964,* 88th Congress, 1963, pp. 1263 and 1267.

43. U.S. Congress, House, Committee on Science and Astronautics, Ibid. and National Aeronautics and Space Administration, Office of Administrator, "Construction of Facilities," *Status of Approved Programs, Fiscal Years 1962-1965.*

44. U.S. *Congressional Record – House,* 88th Congress, 1st Session, May 6, 1965, Vol. III, Part 7, p. 9641.

45. U.S. Congress, House, Committee on Science and Astronautics, Ibid., p. 530; U.S. *Congressional Record,* May 6, 1965, p. 9641; and U.S. Congress, Senate, Committee on Appropriations, *Hearings on NASA Appropriations for Fiscal Year 1964,* 88th Congress, 1963, pp. 64 and 69.

46. U.S. *Congressional Record,* 88th Congress, May 6, 1965, p. 9641.

47. U.S. Congress, House, Committee on Science and Astronautics, Subcommittee on Manned Space Flight, *Hearings, NASA Authorization for Fiscal Year 1965,* Part I, 89th Congress, 1964, pp. 93-5.

48. NASA *News Release 61-236,* October 25, 1961 and U.S. Congress, Senate Committee on Aeronautical and Space Sciences, *Hearings on NASA Authorization for Fiscal Year 1964,* 88th Congress, 1963, p. 65.

49. U.S. Congress, House, Committee on Science and Astronautics, Subcommittee on Manned Space Flight, *Hearings on NASA Authorization for Fiscal Year 1964,* 88th Congress, 1963, p. 206.

50. U.S. Congress, Senate, *Hearings,* pp. 65-66.

51. U.S. Congress, Senate, Committee on Aeronautical and Space Sciences, *Hearings, NASA Authorization for Fiscal Year 1963,* 87th Congress, 1962, p. 102 and *for Fiscal Year 1964,* 88th Congress, 1964, pp. 528-529 and *letter* from Deputy Manager Henry F. Auter, April 30, 1970.

52. U.S. Congress, Senate, *Hearings for Fiscal Year 1966,* 89th Congress, 1965, p. 882 and *letter* from Deputy Manager of Mississippi Test Facility, Henry F. Auter, April 30, 1970.

53. U.S. Congress, House, Committee on Science and Astronautics, *Hearings on 1964 NASA Authorization,* 88th Congress, 1963, p. 3264.

54. D. D. Wyatt, Assistant Administrator for Planning, National Aeronautics and Space Administration, *Letter,* May 19, 1970.

55. U.S. Congress, Senate, Committee on Aeronautical and Space Sciences, *Hearings on NASA Authorization for Fiscal Year 1966,* 89th Congress, 1965, pp. 882-885 and *Hearings on NASA Authorization for Fiscal Year 1968,* 90th Congress, 1967, p. 240.

56. U.S. Congress, House, Committee on Science and Astronautics, Subcommittee on Manned Space Flight, *Hearings, 1963 NASA Authorization,* 87th Congress, 1962, p. 557.

57. Rosholt, *Administrative History,* pp. 254-55.

58. Ibid.

59. U.S. Congress, Senate, *Hearings on NASA Authorization for Fiscal Year 1963,* 87th Congress, 1962, pp. 62-63 and *Hearings on NASA Authorization for Fiscal Year 1964,* 88th Congress, 1963, p. 65.

60. *National Aeronautics and Space Act of 1958,* Section 307 (42 USC 2459) – no year money; Public Law 87-584, August 14, 1962 (76 Stat 382), Public Law 88-113, September 6, 1963 (77 Stat 141), and Public Law 89-53, June 28, 1965 (79 Stat 192).

10

The Atomic Energy Commission Selects A Site for the World's Largest Proton Accelerator

Harold Lasswell's definition of politics as "who gets what, when, and how" was perhaps incomplete. He could well have added "where." Following the lengthy political struggle over the location of the NASA Electronics Research Center in which the Midwest was rejected in favor of Cambridge, Massachusetts, and the Environmental Health Laboratory competition, again lost by the Midwest, the next major federal facility decision concerned the location of a proton accelerator by the Atomic Energy Commission. This would be the nation's largest scientific instrument and would "produce beams of protons with energies of 200 billion electron volts (Bev), more than six times the energy of particles produced by any existing accelerator."[1]

Parameters of the Prize

Political leaders tend to view science in a political context. Michael Faraday once appealed to Parliament for funds for the development of the electric motor, based on his discovery of electromagnetic forces and their application. He was ridiculed by Prime Minister Disraeli who asked what possible use it could serve. Faraday shrewdly replied in a political context, saying, "Sir, someday you may be able to tax it."[2] The political strength of the American scientific community was demonstrated in the decision to build the large accelerator. The attraction of the 200 Bev machine to the politicians was based not only on its intrinsic merits, but also on the pork-barrel benefits which would accrue to the area in which it was located.

The anticipated benefits from location of facilities such as the high-energy accelerator run the gamut from economic benefits to local business and real estate interests to social change and educational impact. An annual operating budget of approximately 60 million dollars for the accelerator, for example, would be spent primarily in the immediate metropolitan area. Over 20 million dollars of such a budget would be spent for salaries alone.

Economic benefits are usually the major reason that local boosters make such efforts to secure location of federal facilities in their area. In discussing the effort of the Midwestern Universities Research Association (MURA) to have the 12.5 Bev accelerator constructed at Madison, Wisconsin, Daniel Greenberg of *Science* wrote:

> . . . when a 150-million dollar research facility is planted on the countryside, all sorts of usually desirable things start to happen to the surrounding areas. New industry rushes to the area – as it is now doing, for example, at the previously barren site surrounding NASA's Manned Spacecraft Center

in Houston, and other federal agencies take to placing facilities and funds in the region, which is part of the story of the Cambridge and California phenomenon.[3]

In the race for the AEC accelerator, as in the case of NASAs Electronics Research Center and the Environmental Health Center, numerous cities and states made offers of free land and other inducements to secure location of the facility in their area. A few examples in the case of the AEC accelerator were as follows:

– Indiana's offer to give the necessary 3000 acres to AEC if it would build the accelerator near Indianapolis. This land would have been purchased with 10 million dollars appropriated by the state legislature.

– New York's offer to provide electric power at one sixth of cost.

– Nevada's offer to provide up to 100,000 acres of land without cost.

– Oklahoma's offer to build a new technical college near the accelerator if the AEC would build it near Muskogee.

– Kentucky's offer to provide a free site and to pay most of the construction costs.

– Kansas City's offer to provide the site and other support items without cost to the AEC.

– Michigan's offer to "match any other offer made by anyone, anywhere."[4]

On the other hand, those who are active in attempting to secure the facility may not fully understand the economic impact on their area. New residents not only provide additional consumers for local business but also mean new requirements for school construction, teachers' salaries, sewage disposal, highways, police protection, fire protection, health and hospital facilities and personnel, and other public service areas wherein it is difficult to calculate the additional costs. However, the net economic effect could well be negative – at least for the local governments involved. Drastic changes in the configuration of a city may not provide adequate time for proper planning of the adjustments necessary to provide a smooth transition to the new stage of development. State limitations on local government financing may in fact make it impossible for local government to do some of the things which it ought to do in reacting to the change. The economic impact of 9,000 new residents of a new area would of course be considerable. However, the influx of a large number of scientist and engineer families also would have an educational impact on the community. In the case of Cape Kennedy, for example, the area schools were forced to react to the influx of aerospace specialists by changing their curriculum to make it more science-oriented.

In commenting on the possibility of the selection of a Chicago area site for the accelerator, the Chancellor of the University of Chicago, Dr. George W. Beadle, stated that one of the long-term benefits would be the contribution the presence of such a center would make toward keeping in the Midwest some of the scientists and engineers graduated from midwestern universities. That is, it could be an antidote for the midwestern brain drain. In addition, it would have the effect of attracting more students to study in the Chicago area universities and so enable them to upgrade their programs in science and engineering.[5]

The competition for the 200 Bev accelerator was directly related to the new understanding that we are in an age where the knowledge industries are extremely important. Attracting such a cluster of talent has all the traditional impacts of economic growth from any industrial-type expansion. However, even more than the so-called "light industrial" or smokeless industries which suburban towns especially see as the panacea for their tax problems, the knowledge industry holds out the added mystery that great unforeseen benefits will accrue to the area in which such groups are located.

There is much reason for this attitude. Research holds the potential of great growth leverage far beyond the normal multipliers used by economists to predict the economic impact of new industries on an area. What is it worth to an area and a region to be on the cutting edge of laser developments, medical breakthroughs, plasma physics, or the development of new materials which can revolutionize an industry? The response varies but often the region gains as much or more than the immediate district where the new facility is built and does so without the dislocation factors inevitably experienced by the impacted district.

An example of this is contained in the response of one of the Chicago suburbs. South Barrington, Illinois, was a preferred site in the Chicago area. However, *after* it was informed that it had been selected as one of the six finalists by the National Academy of Sciences, it requested that it be removed from further consideration. A civic leader described himself as "pro-suburb and anti-particle" and indicated that residents of South Barrington had moved there because it was a suburb and did not want to be faced with the problems of growth which would result from the establishment of the accelerator in the area. There is also reason to believe that many residents feared an influx of blacks because of the attention that was paid in the debate to having equal employment opportunity and available housing for blacks. South Barrington would certainly not be a unique suburb in this respect.[6]

The senators, governor, and chambers of commerce promoting the Illinois location against the other potential sites would understandably have a different perspective. On the other hand, they would see only the advantages of the new facility to the state and would be only remotely affected by any of the real or perceived disadvantages. In any event, competition for the accelerator site would be substantial and the Joint Committee on Atomic Energy (JCAE) was certainly sensitive to the problem. The following exchange took place during the March 1965, hearings on the High-Energy Physics research program. Dr. Lofgren was the director of the Lawrence Radiation Laboratory.

Dr. Lofgren: In order to accomplish definite design studies we have used Camp Parks, an inactive military base on U.S. Highway 50, 30 miles east of San Francisco, as an example; and we have evaluated the site dependent factors in terms of this site.

Representative Craig Hosmer of California: Just a moment, Doctor. You are not trying to sell us this site?

Dr. Lofgren: No; I am using this as an example.

Representative Hosmer: I want to make that clear. There have been two areas already that have tried to jump the gun here and start a big national fight about where this thing is going to go. It is not only premature, but it is going to derogate the possibility of getting this kind of equipment if these people continue.[a]

I have two of them specifically in mind. I didn't want you to become the third. If those people continue, on my own responsibility I am going to do everything possible I can to block the possibility of their ever getting this machine, because they are not doing anything but trying to serve their own selfish interest and in so doing they are making it a lot more difficult to get this program on its feet and underway.[7]

The Madison Accelerator

Prior to the 200 Bev proposal, a group of fifteen midwestern universities, known as Midwestern Universities Research Association (MURA), had been promoting a high-energy proton accelerator, rated at 12.5 Bev, for Madison, Wisconsin. Officially, the machine was known as a fixed field alternating gradient synchrotron (FFAG). This project was estimated at $150,000,000 and was looked upon by the institutions in question as the most important single activity capable of preventing the Midwest from becoming a scientific oasis. The scientific community was split on this proposal in that the Argonne National Laboratory near Chicago had a 12.5 Bev accelerator almost constructed. The Argonne people argued that this would be adequate for the Midwest, whereas the MURA group argued that its capacity would be inadequate to serve the regional needs and that the one proposed for Madison would have greater capabilities than the one at Argonne.[8]

Most Midwest members of the House of Representatives and the Senate lined up behind the Madison proposal. What is unique about the MURA proposal is that the group was able to agree upon a specific location for the proposed facility. This is rare in industrial or university circles in that so much power and prestige as well as economic impact and professional development is related to the location of major facilities. Normally, the other regions competing with a particular region could depend upon the advocates of the facility to be able to agree only on the regional placement rather than on a specific location. In this case, a united front was presented by the universities and the political forces except those identified with Argonne. The dominant force at Argonne was the University of Chicago which looked down on the state universities involved in MURA.

---

[a] The two states referred to by Representative Hosmer were Colorado and Washington.

Approval of the MURA proposal became a major issue for the Midwest political leadership, some of whom clearly indicated that if the MURA proposal was negated, they would vote against all other accelerator proposals as a means of demonstrating their dissatisfaction. Senator William E. Proxmire of Wisconsin wrote President Kennedy's science advisor, Dr. Jerome P. Wiesner, that "The failure to approve an accelerator for the Midwest would seriously compromise the prospect for approving a $250,000,000 accelerator on the east or west coast a few years from now. I say this not with any notion that there might be some kind of political reprisal. But I say this from the standpoint of realism."[9] Proxmire had just become a member of the Senate Appropriations Committee and interestingly enough was suggesting that the Midwest would support a large accelerator for one of the two coasts if it received the Madison facility.

One of the financial realities in the proposal is that construction costs would not be too great to be handled in a federal R&D budget approaching $16 billion, but the operating costs of the facility were estimated to exceed $15 million a year by 1970 and would quadruple by 1980, making it one of the largest single outlays in the total R&D budget.[10] These factors were undoubtedly in the mind of the Director of the Bureau of the Budget, Kermit Gordon, who wrote to a number of the MURA Congressmen in July 1963,

> it will be necessary to assess carefully the requirements of high-energy physics against those of other fields of science, and to balance the requirements of science as a whole against other needs for Federal funds. . . . With regard to . . . the Middle West, I would note as a matter of interest . . . that the accelerator at the Argonne National Laboratory in Illinois, shortly to be in operation, is the second most costly project of this sort ever to be undertaken in the United States. A major factor in the selection of the Argonne site was that this machine would serve the needs of the midwestern universities, and I am confident that it will be successful in this regard.[11]

The Madison decision was caught up in a series of priority determinations related to larger accelerators. In February 1962 and again in December 1962, the Lawrence Radiation Laboratory (LRL), an Atomic Energy Commission (AEC) facility operated by the University of California at Berkeley, submitted proposals to the AEC requesting authorization to conduct design studies for an accelerator in the range of hundreds of Bev's. These and other proposals were considered favorably by the AEC and were made part of a program in high-energy physics which was the result of a study conducted by a scientific advisory panel of the President. The panel was formally made up of members from the AECs General Advisory Committee and the President's Science Advisory Committee (GAC/PSAC Panel) under the chairmanship of Dr. Norman Ramsey of Harvard.

To generate support, the Panel called for assistance from representatives of the Technical Committee on High Energy Physics of the Federal Council for Science and Technology, from the staffs of government agencies supporting high-energy accelerator physics, from representatives of the laboratories interested in new accelerators, and from a number of other individuals including people from the Bureau of the Budget. The Panel previewed its recommendations by releasing intermittent reports through the press.

The Ramsey Report (GAC/PSAC Panel), intended to be a master plan for high-energy physics and programs, was delivered by the scientific community in April 1963. It considered both the MURA proposal and the proposals for large accelerators and recommended that authorization be made within five years for the construction of a 200 Bev proton accelerator at the Lawrence Radiation Laboratory in California and also for design studies on a 600 to 1000 Bev proton accelerator to be located at the Brookhaven National Laboratory on Long Island. It indicated that these two projects should receive similar priority and went on to endorse the Madison facility as well "provided this is not expected to delay significantly the authorization of steps toward high energy recommended" for the Lawrence and Brookhaven sites.[12]

In December 1963, the FFAG facility fell victim to the budgetary constraints and doubts raised by those who felt Argonne would be sufficient and by the new President attempting to establish an economy image.[13] However, "lobbying for and against the proposal had been so fierce, and the members of a supposedly impartial panel of scientists had been so evenly divided as to its merits that the FFAG took up more of President Johnson's time – or so he later wrote Senator Humphrey, one of the FFAGs most ardent backers – than any other non-defense item in the 1965 budget."[14]

The 200 Bev Accelerator

The AEC decision to design a new 200 Bev proton accelerator was made in 1963 in response to the recommendations of the Ramsey Panel on High Energy Accelerator Physics. The purpose of the accelerator would be to provide the ability to develop additional basic knowledge of elementary particles. However, the 200 Bev accelerator has been described as "a nuclear microscope" powerful enough "to solve the riddle of how and why matter is put together,"[15] and it was not hard to understand why it was so sought after by the high-energy physicists.

One of the most readily understandable explanations of the accelerator was provided by Robert C. Cowen, natural science editor of the *Christian Science Monitor,* who wrote:

> High energies let physicists probe matter's basic nature by seeing what happens when the particles bump into other particles.

> As a rough analogy, think of two men running about an empty room in the dark. Once in a while, they bump. You can tell something about their size if you know how fast they run and how often they bump. You can tell something about their softness if you know what happens when they bump at different speeds.
>
> Physicists learn about particles in similar ways. Also, the bumping particles often make a lot of new particles. Physicists have found a zoo full of such particles. They have begun to see some order among them. They begin to grasp the natural laws that particles obey.
>
> Such things are the basic stuff of the universe. To pursue them, physicists feel they need much higher energy particles than ever before. Hence the new accelerator.
>
> In this, protons will speed around a circular tube. Magnetic forces will hold them on course. The tube diameter depends both on the protons' energy and the strength of the guiding magnets.[16]

One of the earliest participants in the 200 Bev accelerator project was Lawrence Radiation Laboratory, operated by the University of California at Berkeley. Under its contract with the University of California, AEC requested that the Lawrence Radiation Laboratory do the preliminary design of the new accelerator. In the course of this effort, the Lawrence Laboratory appointed a national advisory committee in 1964 consisting of ten physicists and chaired by a physicist from the University of Wisconsin. Four of the other members were from the East coast and four from the West. The chairman and other member were from the Midwest. The advisory committee gave strong indications that it thought the new accelerator should be built in California somewhere near Lawrence Radiation Laboratory.[17]

The first step that led to ultimate political success for the physicists was the strategy of consolidating the support of the various interest-clusters thought to have a stake in the project. This was done by bringing interested parties together early in the discussion and by maintaining that cohesion throughout the debates. This reduced considerably the possibility that the project would be rejected as a result of discord within the scientific community. Dr. Donald Hornig, testifying as the President's Science Advisor in 1964, described the coordinating process:

> The Ramsey report (GAC/PSAC Panel report) was published to get the viewpoint of the scientific community and to obtain reactions. Comments were invited from all agencies and were collected. The OST (Office of Science and Technology) sent the report to specially qualified individuals for comment. It also convened a group of young users to see how they felt about the recommendation. Personal discussions were held with Midwest scientists. Accordingly, there was made available as good an expression of the views of the scientific community as could be obtained. I believe it is correct to say that the community agreed with the Ramsey Panel that the building of higher energy accelerators was the single most important step which should be undertaken.[18]

During these 1964 hearings, the Joint Committee on Atomic Energy (JCAE) emerged as the initiator of the new accelerator. The Committee concluded that a formal national policy for high-energy physics was imperative and instructed the AEC to submit a proposal by the following spring.

The AEC authorized the Lawrence Laboratory to proceed with a preliminary design study for the proposed 200 Bev accelerator. The purpose of the study was to establish design parameters for the accelerator and in so doing not only advance the state of the accelerator art, but also provide the basic criteria upon which a reasonably accurate estimate of costs could be made. In accordance with JCAE instructions, the AEC further proceeded to draw up its "Policy for National Action in the Field of High Energy Physics." And on January 26, 1965, President Johnson transmitted the report to Congress with his approval of the guidelines developed. The policy statement was substantially the Ramsey report. Like that report, it recommended the construction of the 200 Bev accelerator with the intention that funds for the final design would be included in the fiscal year 1967 budget to be submitted in January 1966.[19]

The new accelerator was proposed as the central component of a new national laboratory to cost approximately $280,000,000 and to be staffed by over 2,000 scientists and technicians. The proton linear accelerator designed on an AEC contract by the Lawrence Radiation Laboratory was to be 500 feet long with a booster magnetic ring 650 feet in diameter. The ring magnet of the main accelerator was to be housed in a concrete structure submerged for shielding purposes and measuring one mile in diameter. This main magnet weighing over 20,000 tons was to be powered by two 1,500 horsepower motor-generator sets. The land requirement for such a facility was a minimum of 3,000 acres in a configuration of roughly three miles by one and one-half miles as shown in Figure 10-1.[20]

The Joint Committee convened its Subcommittee on Research, Development, and Radiation and proceeded to conduct hearings on high-energy physics, generally, and the policy for national action, specifically. During these hearings, it became clear the degree to which the JCAE had taken over the initiative. The Committee was concerned lest the ceiling on the total AEC budget might squeeze high-energy physics research. It observed that while the total AEC budget had a tendency to remain fairly constant, high-energy physics research costs were increasing considerably. Representative Melvin Price of Illinois suggested that the AEC make it clear to the Bureau of the Budget that the national program be considered separately from any overall limits placed on the AEC budget. The feeling was that the field of high-energy research should be considered in the light of its own national needs. In fact, the Committee went so far as to suggest that the AEC use two budget categories to solve the problem. Chairman Chet Holifield of California remarked:

> I think that one possible approach is to have the AEC bill broken up into Title I and Title II, maybe. We might take our atomic energy program in

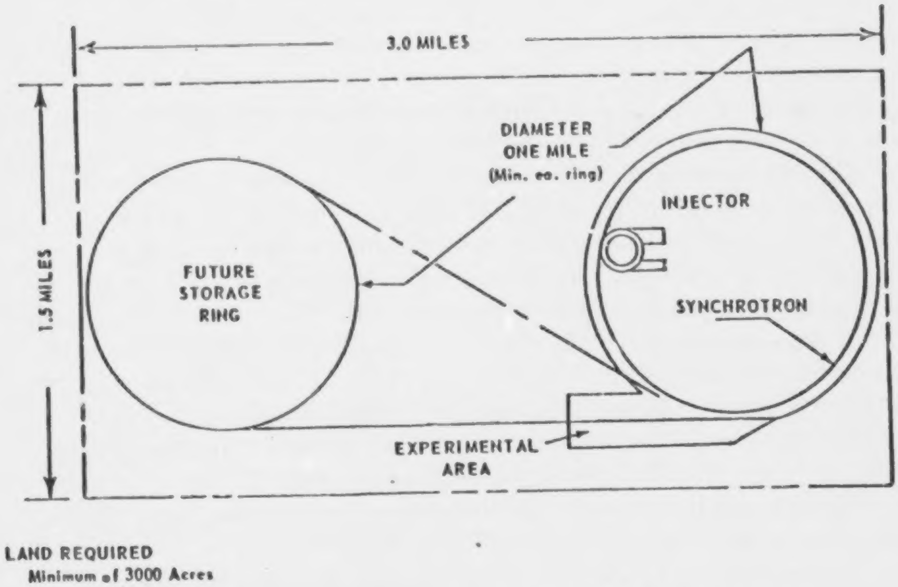


Figure 10-1. Plan Typical Configuration 200 BEV Accelerator. Source: U.S. Congress, Joint Committee on Atomic Energy, *Hearings, High Energy Physics Research*, 89th Congress, 1st Session, March 5, 1965, p. 687.

Title I and put high-energy physics in Title II to make a legislative distinction between the high-energy physics program and the rest of the atomic energy program.[21]

During the 1965 hearings, various leading scientists testified in favor of a 200 Bev accelerator. Dr. Leland J. Haworth, Director of the National Science Foundation, emphasized the importance of the decision by saying that "the next big accelerator is not going to be duplicated and there won't be a lot of large accelerators; clearly it will have to be a national effort."[22]

One justification for initiating the expensive accelerator project is that the further development of nuclear physics will be a long-range benefit to the taxpayers. For example, it will increase the efficiency of nuclear power plants which derive power from the fissioning of Uranium 235. This power can be expanded through controlled fusion reactions. The plasma research upon which development of this field depends would benefit from additional accelerator research.

Dr. S. J. Weinberg of the University of California stated in another context that to fail to build the new accelerator would be to deny the importance of discovering the laws of nature.[23] On the other hand, in an editorial the *New York Times* stated that it is "a distortion of national priorities to commit many millions now to this interesting but unnecessary scientific luxury."[24]

The Lawrence Laboratory completed its study in June 1965, and issued a two-volume report entitled, "200 Bev Accelerator Design Study." The work carried out in the study included preliminary design of the accelerator itself, its physical plant, the associated experimental areas and the support facilities, and estimates of the operating staff and budgets. The report summed up the investigation and described in detail a specific accelerator. There were solutions to all technical problems as well as definite design work, cost estimates and detail site criteria.

The total needed for construction was $348 million. It was further estimated that during the construction phase a number of R&D activities had to be in simultaneous progression. This would cost $32 million in the year of authorization. Several years after turn-on of the accelerator, the annual operating costs would be $58.7 million.[25]

At the conclusion of the 1965 hearings, the Joint Committee on Atomic Energy approved the 200 Bev accelerator project. Both the House and the Senate acted favorably on the authorization bill and all that remained was to select the site and secure Congressional approval of the necessary appropriation. As will be seen, this last step did not occur until 1967 and it involved a fairly intense controversy related to the site.[26]

Prior to the authorization of the two hundred BEV accelerator, the largest American particle accelerator was the thirty-three Bev proton accelerator at Brookhaven, Long Island, New York, operated by Associated Universities, Inc. The other major accelerators in the United States are shown in Table 10-1.

The Soviet Union has a 70 Bev accelerator nearing completion at Serpukhov, near Moscow. Upon its completion, it would be the largest in the world. Europe's most powerful accelerator, rated at 28 Bev, is under the control of the European Organization for Nuclear Research (CERN), located in Switzerland. However, the European Committee on Future Accelerators (ECFA) was studying the possibility of developing a 300 Bev machine to be completed in the mid-1970s.[28]

Although the new accelerator was to have a staff of over 2,000 scientists, engineers, and technicians, as much as 75% of the research program would be carried out by visiting scientists from all over the country. For this reason, the new facility had to be conveniently located. Statements that the site should be located near possible users actually meant that it had to be located near a major airport. The success of the whole project is contingent upon having available competent manpower to perform the necessary experiments.

This is why much attention was paid to the question of locating the new accelerator near one of the older ones. The point being made was that during the design stage of the new accelerator, physicists engaged in the design would be able to maintain currency in the field if they were located adjacent to an existing accelerator. In addition, the flexibility of being able to run experiments on two different types of accelerators in the same geographic location would be an attraction for recruiting some types of specialists. This factor of course gave a

Table 10-1
U.S. Accelerators with Energy Greater than 200 MEV
(As of Jan. 1, 1968)

Location	*Common name*[a]
OPERATING	
Lawrence Radiation Lab., Berkeley, Calif.	184-inch Synchrocyclotron
University of Rochester, New York	130-inch Synchrocyclotron
Columbia University, New York City	Synchrocyclotron
University of Chicago, Illinois	170-inch Synchrocyclotron
Carnegie Inst. of Tech., Pittsburgh, Pa.	142-inch Synchrocyclotron
Lawrence Radiation Lab., Berkeley, Calif.	Bevatron
Brookhaven National Lab., Upton, N.Y.	AGS
Calif. Institute of Tech., Pasadena	Cal Tech Synchrotron
Cornell University, Ithaca, New York	Cornell Synchrotron
Harvard University, Cambridge, Mass.	CEA
University of Illinois, Urbana	Illinois 300-Mev Betatron
Stanford University, Palo Alto, Calif.	Mark III Electron Linac
Princeton University, New Jersey	PPA
Argonne National Laboratory, Argonne, Ill.	ZGS
Cornell University, Ithaca, N.Y.	Cornell 10-Bev Synchrotron
Stanford University, Palo Alto, Calif.	SLAC
Space Radiation Effects Laboratory, Newport News, Va.	Synchrocyclotron
Under Construction	
National Accelerator Lab., Batavia, Ill.	200-Bev
Massachusetts Inst. of Tech., Cambridge, Mass.	Electron Linac

[a] AGS – Alternating Gradient Synchrotron
CEA – Cambridge Electron Accelerator
PPA – Princeton-Pennsylvania Accelerator
ZGS – Zero Gradient Synchrotron
SLAC – Stanford Linear Accelerator Center

Source: William J. Kernan, *Accelerators,* Washington, D.C.: U.S. Government Printing Office, 1968, for the U.S. Atomic Energy Commission, pp. 50-51.

decided advantage to the California, Illinois, and Brookhaven sites. High-energy user groups in the United States are obviously clustered on the two coasts, in the Northeast and in California. The only deviation from this pattern occurs in Illinois.

The Site Selection Process

Noting NASAs experience in locating the Electronics Research Center and the location problem of the National Environmental Health Center to be discussed in the next chapter, the Atomic Energy Commission took a different approach in selecting a site. To forestall the political fury that would surely be evoked by a choice of California or even if the Midwest were by-passed again for a non-California location, AEC planned to select a site before the request for funding was submitted and to do so on the basis of an objective nationwide study. The Midwest was already upset at the maldistribution of federal R&D money and there were suggestions that the Lawrence Radiation Laboratory would get the project.

Despite its intense involvement in the 3-year planning process, Lawrence Radiation Laboratory did not play a controlling role in the next stages of the accelerator decision. One clue to this might be found in the statement by Dr. Glenn Seaborg, Chairman of the Atomic Energy Commission, who described the Lawrence Laboratory as "almost completely integrated in the Berkeley campus and . . . therefore less of a national laboratory than the other laboratories." There have been suggestions that Lawrence was not as open to outside researchers as the others. Seaborg said, "I don't mean by this that . . . visiting scientists aren't welcome. I just think that in any description of that particular laboratory it is clear that it is a laboratory integrated in a single university.[29]

In April 1965, the AEC announced the details of its plan to work with the National Academy of Sciences. It entered into an agreement with the National Academy of Sciences under which the Academy agreed to receive from AEC the proposals of the various regions which AEC considered met the basic requirements and to review them in terms of their "detailed technical requirements and the scientific benefits associated with the various sites."[30]

The criteria for the site prepared by the Atomic Energy Commission were stated as follows:

> a desirable site would (1) contain at least 3,000 acres owned by, or reasonably available to, the U.S. Government; (2) have the potential of delivering a firm power load of several hundred megawatts and a minimum of 2,000 gallons a minute of high quality water; (3) be reasonably close to a commercial and industrial center which includes research and development activities; and (4) be reasonably close to communities having adequate housing, cultural and educational facilities for some 2,000 scientific and technical personnel and their families. Also, the site should be close to adequate service transportation systems and a major airport with frequent service to major U.S. cities.[31]

A detailed explanation of the criteria was appended to the press release and was later submitted by Commissioner Tape to the Joint Committee on Atomic Energy. That document entitled "Considerations Involved in Siting a Major New Accelerator" still of course makes no reference to political criteria. Relevant sections of the statement are as follows:

> It is difficult to establish priorities or weights among the various technical, economic and social criteria that can be described. Some items, of course, are absolutely essential such as acreage requirements, the availability of adequate power, the proximity of adequate transportation, etc. On the other hand, other items such as foundation requirements of deep piles versus shallow piles, one-pass versus recirculating water systems, tunneling versus cutting and filling for shielding the magnet ring, etc., cannot be categorized

> absolutely and are subject to some compromise in order to maximize the potential of each site.
>
> In other words, there will be trade-offs between the technical and other factors in order that over-all efficiencies and economies can be obtained.[32]

In response to its open invitation for an appropriate site for the accelerator, the Atomic Energy Commission received 126 proposals. Many of these proposed several sites within the same community as appropriate for the purpose. In all, there were 200 separate locations suggested in 46 states. The Atomic Energy Commission referred 85 of these proposals, involving 150 different sites, to the National Academy of Sciences in September 1965. That AEC would suggest that 85 locations met the basic criteria for site indicates it was well aware of the political dynamite in its decision. Only three states – North Dakota, South Dakota, and Wyoming – were dropped out of the competition by the AEC.[33]

The Atomic Energy Commission then established a group of teams which traveled to each of the sites indicated in Table 10-2 to collect data to supplement the proposals before the review by the National Academy of Sciences. In May 1966, the National Academy reported that it had whittled the list of 85 proposals down to six, including three sites in the Midwest. In referring the matter back to the Atomic Energy Commission, the site evaluation committee of the National Academy of Sciences reported as follows:

> . . . the Committee has selected six sites, which, on balance, are clearly superior. Five sites are principally characterized by having either the nucleus of a strong accelerator design group, or one of the nation's outstanding universities nearby. These are: Ann Arbor, Michigan; Brookhaven National Laboratory, New York; Madison, Wisconsin; Sierra Foothills near Sacramento, California; and South Barrington (or Weston), near Chicago, Illinois. The sixth site, Denver, Colorado, has neither the university strength nearby nor the existing design group that is considered desirable. However, the Committee is of the opinion that the combined advantages of excellent geology, accessibility, good climate, and an area already available which is sufficiently large to accommodate any conceivable experiment or expansion are such as to make the site merit serious consideration.
>
> The Committee's principal concerns have included the physical properties of the site, the problems of assembling an outstanding staff, and accessibility for visiting scientists who will conduct about seventy-five percent of the experiments. While none of the recommended sites is ideal, each is excellent in at least one of the most important features, and within acceptable limits with respect to others.[34]

The report also indicated in detail some of the bases for the recommendations of the site evaluation committee. It stated that there were major categories of factors which had to be considered:

> . . . The first includes the physical features, such as geology, size, configuration, climate, and availability of power, water and industrial support. The

Table 10-2
85 Sites Identified by AEC for Further Consideration by NAS Committee

Alabama
- Birmingham Area
- Eufaula
- Florence
- Greene County

Arizona
- Gila River Indian Reservation
- Phoenix and Tucson

Arkansas
- Little Rock

California
- San Francisco Area
- San Diego

Colorado
- Denver Area

Connecticut
- Plainfield

Florida
- Broward County
- Dade County
- Fort Walton Beach
- Jacksonville/Gainesville Area
- Orlando
- Palm Beach Gardens
- Tallahassee
- Tampa Bay Area

Georgia
- Atlanta Area
- Bainbridge
- Forsyth County
- Fulton County
- Savannah

Idaho
- Idaho Falls

Illinois
- Chicago Area
- Princeton

Indiana
- Clinton
- Indianapolis Area

Iowa
- Cedar Rapids (Sunbury and North Liberty)

Kansas
- Kansas City Area

Kentucky
- State of Kentucky Selected Sites (Covington and Others)
- Louisville
- Murray
- West Point
- Paducah

Louisiana
- New Orleans Area

Maine
- Sanford

Maryland
- Baltimore Area
- Dickerson

Massachusetts
- Haverhill

Michigan
- Ann Arbor and Battle Creek

Minnesota
- Dakota County
- Duluth

Mississippi
- Jackson
- Perry/Forest Counties
- Scott County

Missouri
- Joplin
- (Kansas City Area – See Kansas)
- St. Louis

Montana
- Arlee

Nebraska
- Mead

Nevada
- Las Vegas

New Hampshire[a]

New Jersey
- Millville
- Warren Grove

New Mexico
- Albuquerque
- Santa Fe

---

[a] Note: The Sanford, Maine, location has been endorsed by the State of New Hampshire, and part of the Haverhill, Massachusetts, location is in New Hampshire.

Table 10-2 (Continued)

New York
Auburn
Brookhaven
Hunter
New York State Selected Sites
(Model City and others)

North Carolina
Research Triangle Area

Ohio
Cleveland – Ravenna Arsenal
Marietta
Pike County

Oklahoma
Oklahoma City Area
Tulsa

Oregon
Willamette Valley

Pennsylvania
Crawford County
Pittsburgh Area

Rhode Island
North Kingston

South Carolina
Savannah River

Tennessee
Memphis Area
Oak Ridge

Texas
Amarillo
Austin
Dallas/Forth Worth Area
Houston
Lubbock
Odessa
San Antonio

Utah
Salt Lake City

Virginia
Norfolk

Washington
Hanford

West Virginia
Pt. Pleasant

Wisconsin
Madison

Source: Atomic Energy Commission, "AEC Asks NAS to Evaluate 85 Site Proposals for 200 Bev Accelerator," *Press Release J-68*, March 22, 1966.

second category consists of those less readily measurable factors of environment likely to affect the recruitment of resident staff and the participation of visiting scientists.

Some of these factors will have an important influence on the success of the entire program; all will influence its cost. The cost differences dependent on the various physical features of the proposed sites cannot be accurately determined without detailed studies of the design of the accelerator and the experimental program at each site. In the judgment of the Committee, however, the ultimate cost of the project depends less on the physical features of the site than on the competence and ingenuity of the physicists and engineers who will design the facility and be responsible for its operation. The cost to the project that would result from inadequate staff would far exceed the savings that might be realized through such visible considerations as cheaper land, power or water.

After assuring themselves that a given site had suitable physical properties, the Committee assigned *paramount importance* (emphasis added) to the considerations that affect the recruiting of personnel for the national accelerator laboratory, and the participation of the nation's high energy physicists.[35]

The NAS committee altered the criteria somewhat in requiring, for example, that at least 5,000 acres of usable land be available rather than the 3,000 acres in

Table 10-3
Six Finalist Sites and Factors Weighed in Final Selection

Site	*Acreage*	*Soil Conditions*	*Utilities*	*Adequacy of Transportation*	*Proximity to Universities with Key Researchers*	*Tie-ins with Other Accelerators*
Ann Arbor, Michigan	5,700 acres	bedrock at 200-300 feet	adequate	½ hour to Detroit airport +	U of Michigan –	none
Brookhaven, New York	6,000 acres purchase already underway +	Reaction of the subsoils to leading is known as a function of time and AGS operation.	adequate	NY airports 1½ hours by car	near all NY and Northeast Universities +	AGS accelerator +
Denver, Colo.	34,000 acres already government owned +	no seismic activity high strength subsurface rock	power supply not adequate –	2½ hours to Denver airport	U of Colorado at Boulder and U of Denver –	none
Madison, Wis.	5,500 acres	bedrock is near surface	power not adequate –	2 hours to Chicago's O'Hare field; feeder flights needed from Madison –	U of Wisconsin –	2½ hours by car from Argonne
Sacramento, California	4,117 acres but could be doubled	underlain by hard rock; no seismic activity	adequate	2½ hours to San Francisco. Sacramento feeder flights needed –	Stanford, U of Cal.-Davis, U of Cal.-Berkeley +	Lawrence Radiation Laboratory at Berkeley and Livermore Lab at Davis +
Weston, Illinois	5,000 acres	bedrock at 70-125 feet but deep piles needed to support the magnet ring	adequate	40 minutes to O'Hare Field +	U of Chicago Northwestern U of Illinois, Illinois Institute of Technology +	1½ hour to Argonne ZAGS machine +

Source: Data extracted from *Report of the National Academy of Sciences' Site Evaluation Committee,* Washington, D.C., March 1966, pp. 23-39.

the first AEC list of criteria. However, none of the major sites were eliminated on the basis of not having 5,000 acres available. Even Brookhaven, which is located on densely populated Long Island, had 6,000 acres available in addition to the area currently being used. The committee clearly pointed out that it did not attempt to make definitive estimates as to cost of construction and operation in the various locations. Rather, it indicated great concern with "what might be termed 'return on investment.' "[36]

The Committee strongly urged that the legislative and executive decision-makers pay attention to the "total investment that will be needed for initial construction, continuing improvement, and operation over the life of the laboratory. The value of this unique laboratory as a national asset will depend chiefly on its through-put potential: the rapidity with which first-rate experiments can be carried out."[37] This attitude accounted for the assignment of "paramount importance" to recruitment of appropriate manpower and access for visiting researchers.

Key points selected from the individual site evaluations in the NAS report are reflected in Table 10-2. Data for the South Barrington site are not shown since the civic leadership of that area withdrew its site from the competition after the National Academy report. It is clear from the Academy analysis that South Barrington was the preferred Chicago area site, although Weston is considerably closer to the Argonne Laboratory as is obvious in Figure 10-2.

Analysis of Table 10-3 indicates that all of the sites except Brookhaven, New York, and Weston, Illinois, have some serious deficiency. The weakest site seemed to be Madison, Wisconsin, which had no major plus factor, had inadequate power supply, is over two hours to Chicago airports, and had less proximity to universities with key researchers in science, physics, and engineering than all six of the sites with the exception of Denver. Like Madison, Denver had a power supply problem and also had a problem with rail transportation in that a seven-mile spur would be needed. Madison and the Sacramento site had the worst airport conditions of the six sites. Denver and Brookhaven offered the best situation in terms of land. The land at Brookhaven was already in the process of being purchased for governmental purposes and the 34,000 acres at Denver was the greatest amount of acreage available anywhere and it was already under government ownership.

The most significant factors turned out to be the availability of transportation to permit full participation by visiting researchers, proximity to existing major universities with compatible research programs, and relationships with existing accelerators sufficiently close so that the staff of the new accelerator would be able to use the facilities during the long development period of the new accelerator. Location near another accelerator would also make it possible for staff and visiting researchers to use several accelerator facilities for different purposes in the same general area.

Weston, Illinois, ranked high because of its transportation facilities, its

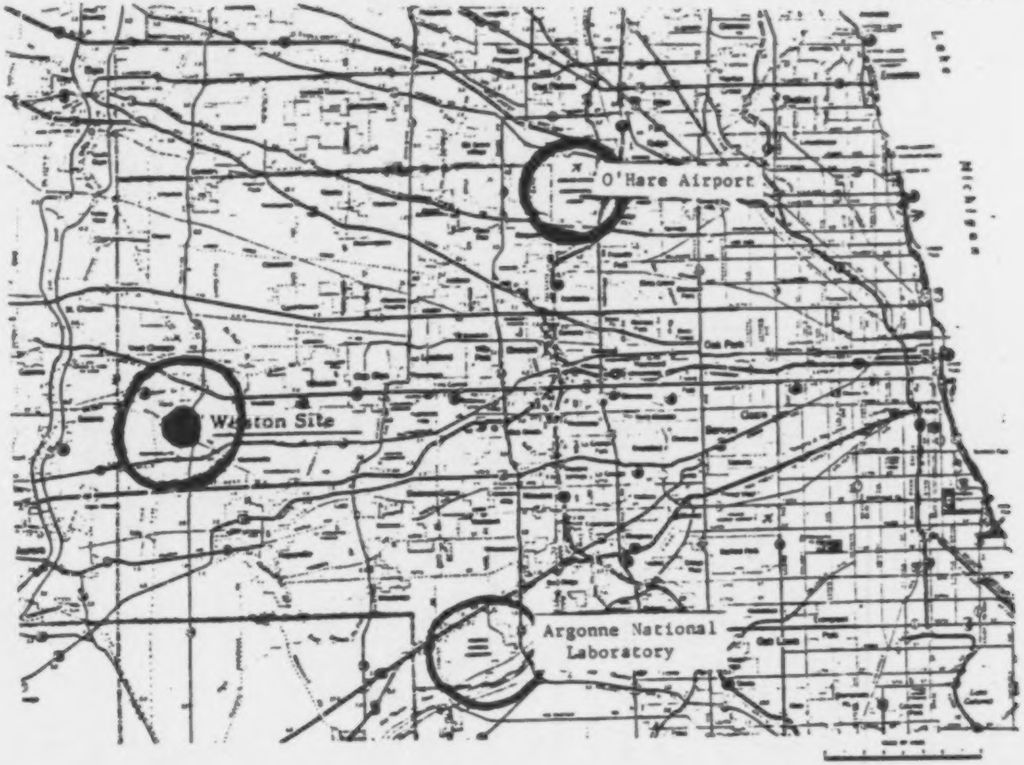

Lake
Michigan
O'Hare Airport
Weston Site
Argonne National
Laboratory

Figure 10-2. Geographic Relationship of Weston, Argonne and O'Hare International Airport, Chicago. Source: U.S. Atomic Energy Commission, *Basis for the Selection of the Chicago (Weston) Site for Location of the 200 BEV Accelerator Laboratory*, Washington, D.C., January 18, 1967, p. 11.

nearness to the University of Chicago, Northwestern University, the University of Illinois, and the Illinois Institute of Technology, and its location within half an hour of the Argonne National Laboratory. The Sacramento site could claim proximity to Stanford University, the University of California at Davis, and the University of California at Berkeley. In addition, it was within two hours driving time of the Lawrence Radiation Laboratory at Berkeley and a half hour from Livermore Laboratory at Davis. The Brookhaven Laboratory was said by the National Academy to be within reasonable proximity of all major universities of the Northeast and especially Yale, Princeton and Columbia which have active accelerator research programs. The colocation of the new accelerator with the existing 33 Bev AGS accelerator at Brookhaven would provide the best opportunity for direct tie-ins and joint use of technical staffs.

Nevertheless, the National Academy of Sciences was certifying that while none of these sites was perfect in all respects, all could be adapted to be adequate to the needs of the facility. In a sense, it provided the Atomic Energy Commission with a well-balanced opportunity to select whatever region it wished since there were three midwestern entrants, one from the East coast, one from the West coast, and Denver which should be considered as representing the Rocky Mountain region.

On the other hand, it might be argued that these research groups are clustered where they are because of the location in those places of appropriate accelerator facilities. The vicious circle of the "have-not" areas would never be broken unless such a facility were placed in an area which did not now have it. In this regard, the Chicago area separated itself from the Midwest position in that it was not among the have-not groups. This factor was one of the reasons cited for the failure of Madison, Wisconsin, to be approved as the 12.5 Bev accelerator before the search for a 200 Bev accelerator site was undertaken.

Shortly after the decision not to build the 12.5 Bev accelerator in Madison, Wisconsin, President Elvis J. Stahr of Indiana University stated that the accelerator "was abandoned, because among other things, the Midwest never quite united behind it."[38] However, he added that the major Midwest universities had agreed that they would support whatever midwestern site appeared to have the best chance of winning the 200 Bev accelerator after the initial screening process. Governor James A. Rhodes of Ohio, whose state later was to play a key role in the fight over the Environmental Health Center, stated "We have been discriminated against continually. We are not asking for something here. We are entitled to it." He was speaking not only of Ohio but of the Midwest and the fact that it had come off a poor second with California and the Northeast in the distribution of federal contracts and facilities.[39]

Although the National Academy of Sciences Committee approved sites in the Midwest as well as on both coasts, there was some serious skepticism expressed as to the objectivity of the national competition for the site. It was suggested that the Committee "knew where it was going before it go there."[40] This observation is based upon the fact that the Academy committee visited selected portions of only six of the 43 competing states and that all six finalists were in those selected portions. In addition, three of these visits occurred *prior* to the visits of the AEC evaluation team which supposedly was to furnish preliminary information to the NAS team.[41]

Although the number of sites visited by NAS and the order in which they were visited suggests that they knew "where they were going," no specific evidence exists that the Committee had preselected any of these sites. The Committee was composed of men who knew what was required to build and operate an accelerator, and so might rather easily identify those areas which would be best suited as well as those which would be unsatisfactory even without personally visiting each site.[42] Nevertheless, areas not visited and which had mobilized significant local political, economic, educational, and other support could well question whether a full review of all proposals had been made or whether they had been used to create the impression of a national search.

The congressional reaction to the choice of sites recommended by NAS to the AEC was predictable. The proximity of the finalists to the already established research centers or northern universities caused Senators John Sparkman of Alabama and Richard Russell of Georgia to engage in a bit of scriptural sarcasm with Sparkman commenting, "For unto every one that hath shall be given, and he shall have abundance . . . ," and Russell concluding, "But from him that hath not shall be taken away even that which he hath."[43] Senator Symington of Missouri and Senator Mansfield of Montana also expressed disappointment in the geographic distribution of the finalists.[44]

Senator Edward Long of Missouri contributed a detailed analysis of the sites selected by the Academy. He sent the analysis to AEC Chairman Dr. Seaborg and then placed it in the *Congressional Record*. Senator Long wrote:

> The original AEC criteria stated that 'acreage requirements, the availability of adequate power, the proximity of adequate transportation, etc.' are 'absolutely essential.' Unfortunately, it appears that the National Academy of Sciences did not agree with these criteria and instead picked sites which did not meet your high standards.
>
> *Acreage requirements.* All but two of the sites fail to have land 'owned or reasonably available to the Federal Government.' If the AEC goes ahead with the six sites, in addition to negotiating with as many as 185 private owners on a single site, it may cost the American taxpayer $2,058,000 at the Sierra Foothills site; $9,360,000 at the Weston site; $2,800,000 at the Ann Arbor site. In addition, relocation problems may occur due to the fact that at the Weston site a major railroad – the Elgin, Joliet and Eastern – crosses the site as well as a 138 kv transmission line. There may also be some problem due to the fact that the town of Weston (population 410) is included in the site area.

> *The availability of adequate power.* Even the National Academy itself admits that at the Denver and Madison sites 'A detailed study would be needed to determine whether the power transmission system which would supply the area will have sufficient capacity to insure acceptable voltage and frequency stability under all possible load conditions.' Perhaps in a re-study of all the sites you will be able to find ones which do not fail to meet the AEC's power criterion.
>
> *Proximity to adequate transportation.* The Sacramento site and the Madison site fail to offer 'Proximity to a major airport having, or planning to have frequent service to major U.S. Cities' which the 'Siting Factors' memorandum mentions. Instead we find that the site near Sacramento is 2½ hours or 116 road miles from the San Francisco International Airport and slightly less than that time and distance from the Oakland International Airport. Thus, in spite of AEC criteria, the National Academy of Sciences has selected a site which, as its report states, 'is only moderately accessible to the majority of the nation's high energy physicists.'
>
> Rail transportation appears to be inadequate at the Denver and Madison sites. The cost of a rail extension might be $400,000 to serve the Madison site and $300,000 to serve the Denver site. Seriously doubt if any railroad could absorb the cost of such extensions.
>
> Believe that the communities which proposed sites in all areas of the nation deserve to know why, even though a number of them satisfied the basic criteria, the National Academy of Sciences overlooked them in favor of sites which do not satisfy the AEC's basic criteria.
>
> May I respectfully add that I am deeply disturbed by the Academy's statement 'It has not been the purpose of this Committee to estimate the costs of construction and operation at various sites.' By failing to look into the cost factors, the Academy has fallen far short of the expectations of the Congress and the American taxpayer.[45]

AECs Final Decision

The selection of the Weston site was announced December 16, 1966, in an AEC press release. Chairman Seaborg attempted to break the news gently for the disappointed bidders. "Each proposal had many strong points," Seaborg said, "making the selection of the one site an extremely difficult task." As an added sop to the losers, he concluded that "the information developed for these proposals will be a help to the communities in attracting other industry or government projects."[46]

The estimated cost at the time of the selection was $300 million, or $20 million more than the first estimate. This increase was announced at the same time the AEC said it was reducing costs by proceeding at "a somewhat reduced scope with potential for expansion at a later date."[47] The elimination contest was summed up in the same news release as follows:

> One hundred and twenty-six proposals recommending more than 200

> locations were submitted to the Commission in 1965 for the large accelerator project. The AEC reduced the list to 85 proposals which included 150 different sites and in September of 1965 asked the National Academy of Sciences to evaluate the 85. In March of 1966 the Academy recommended six finalists which were considered by the Commission.[48]

Once again, the overriding importance of skilled manpower was acknowledged.

> The principal objective in the factors governing the choices of a site is the contribution to the capability and effectiveness of the nation's research in elementary particle physics. The site must be so located that management can mobilize and maintain the necessary specialized staff, both resident and non-resident to accomplish the research goals.[49]

The detailed press release reviewed all the criteria and the degree to which the Weston site met them. It made a strong case based on the good transportation and access to a key cluster of significant universities. With regard to the water question, the release indicated no need for dependence on the controversial Lake Michigan water:

> Ample supplies of water for the initial and future project needs can be obtained from on-site wells drilled into deep sandstone aquifers and from the Fox River, 3 miles to the west of the site, and the DuPage River adjacent to the east boundary of the site.[50]

The other questions raised in the Senate debate were also covered. In response to Senator Magnuson's complaints regarding the overlooking of sites with low operating costs, the AEC said:

> AEC staff estimates indicate that operational costs at all of the sites should be closely comparable and such differences as are indicated are well within the uncertainties of the estimates. About two-thirds of such costs are believed to be independent of site location. There are, of course, differences in electric power rates, cost of water, climate related incremental costs and in the cost of annual increments of capital improvements for each of the sites. However, these various operational cost elements offset one another to a degree that none of the six sites appear to have a notable advantage over the others with regard to estimated costs of operation.[51]

Regarding the open housing and equal employment regulations, the release reported:

> In its investigations and evaluations the Atomic Energy Commission included consideration of the climate of equal opportunity and nondiscrimination that existed in the communities containing and surrounding the six prospective sites, as well as the applicable state laws, executive orders, and local ordinances. . . .
>
> During the selection process, the AEC informed the Weston (and the other) site proposers of the thinking of the U.S. Commission on Civil Rights on civil rights criteria, and indicated that the AEC planned to seek from the

> selected site, appropriate commitments in areas of employment, housing, education, and community facilities and services as were suggested by the Commission on Civil Rights.
>
> The Atomic Energy Commission noted differing views with respect to nondiscrimination in housing, and in certain areas more distant from the site, with respect to integration in public schools, but will expect that with the leadership of the state and local governments and with the cooperation and support of citizens and community organizations in the Chicago area, a broad satisfactory record of nondiscrimination and equal opportunity will be achieved.[52]

Finally, the selection of Weston was said to be based on the balancing of all factors and criteria and that "the Weston site offers the best prospects for attaining the objective of successful mobilization and maintenance of the best possible staff to construct and utilize this unique national facility most effectively."[53]

The Senate Debate

Several weeks after the AEC decision, the appropriation of 7.333 million dollars for the design of the Weston accelerator was presented to the Senate as part of the fiscal year 1968 authorization bill of the Atomic Energy Commission. The 200 Bev accelerator was the only item questioned in the 2.634 billion dollar request. Senator John Pastore of Rhode Island, chairman of the Joint Committee on Atomic Energy, introduced an amendment to delete the 7.333 million dollar item from the bill. He based his case on four arguments questioning the appropriation of money for such a facility in general, and then two arguments with regard to the specific site selected.[54]

As to the question of need, Pastore stated "It is nothing more or less than an educational gadget for the physicists of this country who are interested in high-energy physics. . . . This Bev has nothing to do with national defense or national security."[55] He further suggested that to approve a new facility which would cost 400 million dollars for construction and perhaps 60 million dollars to operate, was a distortion of national priorities because of Vietnam and the deficit budget. He also predicted that when the budget squeeze would hit later in the year, many Senators would jump first on the antipoverty program and the Great Society programs for budgetary cuts. He suggested that the best place to start cutting appropriations was with the accelerator.

In defense of the question of whether the facility was needed, the best argument was presented by Senator Charles Percy of Illinois. He quoted testimony by Dr. Glenn Seaborg, chairman of the Atomic Energy Commission, regarding the purpose of the accelerator as follows:

> The aim is to correlate all the information sufficiently to try to bring it together in a coherent manner that will lead to better understanding of the

> nucleus of the atom and therefore of the atom itself and therefore of molecules and everything that makes up the universe.[56]

But becoming somewhat more philosophical, Senator Percy added that "if we fail now to go forward with the construction of the 200 Bev accelerator, we will be failing the promise that the great pioneers like Einstein and Fermi had set for us in their exploration of the frontiers of nuclear science."[57]

Senator Pastore also pointed out that there was some question as to why a new accelerator was needed when the one already built at Stanford University at a cost of 114 million dollars was only being used 50% of the time.[58] Senator Hickenlooper of Iowa, a twenty-one-year member of the Joint Committee on Atomic Energy and a strong supporter of the Weston selection, countered that the Committee was aware the Stanford University Accelerator was operating at 50 percent of its capacity and for that reason had recommended that an additional three million dollars be added to the Stanford University operation to optimize that facility's operation. He cited the lack of adequate operational money as the reason that the Stanford Accelerator was operating under capacity and that three million dollars would get it up to 80 or 85 percent of its capacity.[59]

Another reason advanced by Senator Pastore for delaying approval of the facility was that questions had apparently arisen as to the optimum size of the new accelerator. He pointed out that the project was first brought to the Joint Committee on Atomic Energy approximately four years ago when it was suggested that a 200 Bev costing $375 million be authorized. However, according to Pastore, the Bureau of the Budget cut back this figure to 240 million dollars by lowering the intensity and dropping important items of equipment which AEC had wanted. When this was proposed in the Joint Committee on Atomic Energy, some members of the Committee questioned why AEC should compromise on a lower intensity machine. The conclusions of special hearings on the subject was that a big machine should be built and Pastore pointed out that "the Atomic Energy Commission has not yet given its approbation."[60] Again he argued, the AEC is not sure of what it really needs and there would be no harm in waiting one year longer to be sure.

In response to the argument that 7.333 million dollars was not that much money to get the project started, he replied "If you do not appropriate the money to build the project, you are only sending this money down the dirty drain into the open sewer. The 7,333,000 dollars is tied in with 40 million dollars which the AEC will be requesting next year. Once you have your nose in that tent or you have your foot in the door, you have to begin to weigh whether or not you will lose the 7,333,000 dollars."[61]

Senator Percy compared American efforts with nuclear accelerators with the efforts of the European Organization for Nuclear Research (CERN) as well as with the Soviet efforts. He stated that witnesses before the Joint Committee on Atomic Energy had discussed the need for perhaps even increasing the size of the machine now in planning to give it a capacity of 300 Bev.

> . . . since this project has been conceived, a study project was undertaken by the Commission to determine whether or not they could accede to the Director of the Budget's request that they reduce the size of the accelerator. That was turned down by the Commission, and it was turned down by the joint congressional committee. Rather than reduce the size of it, in accordance with a request from the Budget Director, they saw the need to increase its size, because, as time goes on, this accelerator will become increasingly obsolete as others press forward. If the Soviet Union, with only a part of the gross national product that we have, and other European countries can see the urgent need to move forward in this field, how can we sit here and . . . delay this project, the need for which has been seen for the last 4 or 5 years, and is fully recognized by every top grade physicist and scientist in the United States of America, as well as in the world?[62]

Senator Pastore related that there was also a serious question as to whether the Weston site would have adequate water for the project. He referred to a suit by the attorney-general of Michigan regarding the amount of water that Illinois could take from Lake Michigan.[63] Senator Percy also responded to this point by inserting in the record a report from the chairman of the Great Lakes Commission of Illinois who had formerly been assistant attorney-general of Illinois in which capacity he handled the Lake Michigan Diversion case in the United States Supreme Court. Senator Percy quoted Mr. Albert J. Meserow as saying:

> . . . the State of Illinois is bound by the U.S. Supreme Court decree in the Lake Michigan Diversion case to limit the withdrawal from Lake Michigan for *all* purposes to 3,200 cubic feet per second. Because of this limitation in the decree it cannot withdraw any more water from Lake Michigan for Weston or for any other purpose in Illinois over and above the 3,200 cubic feet per second. The important thing to emphasize is that there is sufficient and adequate ground and stream waters in the Weston area for the atomic plant otherwise the Atomic Energy Commission would never have selected this site.[64]

The main thrust of the Pastore argument with regard to the site, and in fact the only site objection raised other than the water question, was whether or not the Weston site could provide equal employment opportunity and equal housing opportunity to minority employees and visiting scientists at the accelerator. In the initial call for site proposals, adequate housing had been listed as one of the essentials to be considered in the evaluation process.

Senator Percy pointed out that "neither the original site selection criteria submitted to the Joint Committee in April 1965 by the Commission nor the National Academy of Sciences' report to the Commission recommending consideration of six sites for the accelerator in March 1966 specifically mention the criteria of civil rights or non-discrimination, much less the matter of open housing legislation."[66]

Senator Pastore took issue with this statement and referred to a page from the Hearings of the Joint Committee in which the Commission reported:

July-December 1966. The Commission considered the 200 Bev accelerator site selection question at 18 meetings. Primary concerns of the Commission were electric power supply, civil rights, and non-discrimination.[67]

The question arose because Senator Pastore argued that he had a considerable amount of information from persons in Illinois that equal employment opportunities did not exist at the Argonne facility because of the absence of open housing in the vicinity of the installation. Specific information submitted to back up this claim was drawn from a survey made by NAACP in January 1967 as follows:

> We determined that there were 259 minority group people working at the Argonne Installation on that day, and of the 259, 225 were Negroes. Of the 225, 223 found it necessary to commute approximately 70 miles per day round trip between the Argonne Installation and the City of Chicago, as well as the City of Gary, Indiana.[68]

Pastore argued that it would be improper for the Senate to ignore Title VI of the 1964 Civil Rights Act which provided,

> No person in the United States shall, on grounds of race, color, or national origin, be excluded from participation in, be denied the benefits of, or be subjected to discrimination under any program or activity receiving Federal financial assistance.[69]

There were some interesting responses to this approach. Senator Dirksen in arguing for the facility stated "I can show the Senator thousands upon thousands of people who live where I live, thirty miles from Washington, who fairly congest the highways in the Washington area in order to get to work." Senator Brooke of Massachusetts asked, "But they are not denied the right to live in an area closer to their jobs, are they?" Senator Dirksen responded "Thirty miles away is an opportunity in this country, as we see it."[70]

Senator Sam Ervin of North Carolina asked Senator Percy if he could imagine any better way to destroy the integrity of the federal system

> . . . than for Congress to tell the States of this Union, "If you do not pass the kind of laws relating to local matters Congress desires you to pass, then you will not be able to participate to the full extent in American life, and you will not be permitted to receive Federal installations."
>
> Would that not be destroying the States of this Union for Congress to undertake to coerce the States into passing laws which Congress might think a State should pass and which a State refuses to pass?[71]

Senator Spessard Holland of Florida asked "In the event such a precedent as this were set, how would it operate in future instances, such as for instance, in choosing the location of a great center such as the Kennedy Space Center at Cape Kennedy?"[72] He went on to list a variety of other NASA and Atomic Energy facilities and ended up by answering his own question as follows:

> This kind of precedent if once adopted would be a great expansion of Title VI of the Civil Rights Act so as to operate against future federal investments of great size in any state unless that state bowed to the will of that group which wants open housing established throughout the Nation but has not passed a federal law on that subject.[73]

Senator Pastore argued:

> We have passed a higher education law, and we have passed the spirit of Title VI of our civil rights law. We do not give out money for educational purposes if there is segregation, and I do not believe anyone challenges that. This is because we feel that when you are dealing with the whole complex of the student society of this Nation, you have to treat them all equally and give them all a fair opportunity.[74]

Senator Brooke added that the amendment proposed was not an attempt to move a facility which was already operating but rather was an attempt to establish a policy consistent with the new Civil Rights Act of 1964. In referring to the Atomic Energy and NASA Centers offered as other examples of places where facilities would not have been built if the policy proposed in the Pastore Amendment were approved, Brooke added:

> . . . these projects which are so frequently mentioned were all constructed long before the statement in the 1964 Civil Rights Act that the Federal Government would no longer tolerate discrimination in projects for which it was responsible. We are not attempting to correct past mistakes. We are attempting to insure that these mistakes will not be repeated in the future.[75]

Senator Pastore asserted that he thought it somewhat remarkable that of the six states remaining in the final competition, only one did not have an open housing law and, given the concern of AEC with equal employment and housing opportunity for its employees and visiting scientists, that this was the site which was selected. He went on to point out that his position was slightly different from that of Senator Brooke in that he did not insist upon a state open housing statute but rather "did insist that the vicinity around the project should be of such a character that no one would have any trouble at all in finding any housing because of religion, race or color."[76]

Senator Brooke joined Pastore in saying that the federal government should not be in the position of rewarding racial discrimination by making federal grants available to a community unwilling to provide equal housing opportunity. He suggested that there was nothing wrong with the federal government using its own economic power to secure compliance with the law of the land.[77]

Senator Percy quoted from a resolution passed by the national board of the NAACP in 1966 regarding this question which stated as follows:

> Whereas we recognize that racial discrimination exists in each of the six communities now under consideration, we also believe that the degree of difference, if any, between such discrimination in one as opposed to the

other is not great enough to justify the NAACP in taking sides in the matter; now therefore, be it

Resolved That the NAACP expresses no preference with respect to the question of selection of a site for the atom smasher planned by the Atomic Energy Commission.[78]

It is clear that when AEC settled on the Weston site it had to receive certain guarantees from local and state governmental authorities to make the project feasible. Among these guarantees AEC included securing substantial evidence that there would be no housing discrimination affecting AEC employees or visiting scientists. This became such a serious matter that Chairman Seaborg and three commissioners of the Atomic Energy Commission travelled to Springfield, Illinois, and met with Governor Otto Kerner and state legislative leaders. At this meeting on April 12, 1967, Seaborg told the Illinois leaders:

> There is no question in our minds but that the issue of housing discrimination will be debated when the Atomic Energy Commission authorization bill goes to the floor of Congress within the next few weeks. Those offering and defending authorization of the project *will need all of the evidence of positive action to eliminate discrimination in housing they can obtain.* Frankly, at the moment, the AEC has little to offer. The commitments sought from communities in the Weston area have failed to materialize. . . . You may also ask if it is actually feasible to move the site from the state of Illinois. Political issues aside, it certainly is feasible to move the site.[79]

Following that meeting, appropriate legislation was passed by the Lower House of the Illinois Legislature but was rejected in the Illinois Senate. Regarding this point, Senator Jacob Javits of New York suggested:

> . . . I do not believe that we need to allow the Legislature of the State of Illinois to act in a way which it knew would jeopardize this program on the floor of the Senate because apparently its deep feeling about open housing was superior to its concern for the presence of a great Federal installation in that State.[80]

When the authorization bill containing funds for the establishment of the AEC facility at Weston passed the House of Representatives on June 29th (by a vote of 104-7), no Illinois city or county other than Weston had yet taken steps to pass an open housing ordinance. Weston's population was 350 and it would be dissolved as a jurisdiction if the facility were built. On June 29th, the town of Wheaton, the county seat with a population of 26,000 located six miles from Weston, passed an open housing ordinance and on July 11th, Joliet, a town of about 60,000 located twenty miles from the site, also passed legislation supporting open housing. Several other communities were opening parts of their area for occupancy by persons from minority races.[81]

In response to this, Senator Pastore said that he was pleased that such actions were being taken and so,

> I must say to my good friend, the senior Senator from Illinois [Senator Dirksen], that we should give this good feeling a chance and postpone this project for another year. I am not for changing the site from Weston. I have said that in the Hearings. . . . I want the project to be in Weston. I do not want to look for an alternative site, but I am saying now: You have raised this issue Mr. Atomic Energy Commission. You stick by your guns, and don't retreat.[82]

Senator Brooke added that the NAACP resolution of 1966 was outdated. On June 16, 1967, Mr. Roy Wilkins, Executive Director of the NAACP wrote to the chairman of the Atomic Energy Commission reflecting the NAACP attitude that the rejection of open housing legislation by the Illinois legislature caused a change in NAACP policy. The Wilkins communication read:

> Since Illinois Senate today voted to kill fair-housing bill, NAACP calls upon Atomic Energy Commission to revoke award of atomic installation to Weston, Illinois on ground the state has no guarantee in law against racial discrimination in the sale or rental of homes.[83]

The debate then shifted to the point of whether the proposed new policy could be consistently applied. Senator Percy asked Senator Brooke if he intended to introduce an amendment to the bill under discussion to remove the authorization of funds for other facilities located in states which do not have open housing legislation. He cited specifically facilities at Los Alamos, New Mexico; in Nevada; and in Richland, Washington; since those three states are among the thirty which had not passed fair housing legislation at the time of the debate. Senator Brooke responded that he did not desire to interfere with additions to existing projects but only to establish a policy on major new facilities.[84]

Shifting the basis of argument and betraying the regional bias that was present in the entire debate, Senator Percy introduced some statistics on the percentages of nonwhites residing in the various congressional districts of the six final sites. Five of the six congressional districts had populations under 6.2% nonwhite. On the other hand, Ann Arbor, Michigan, had a nonwhite percentage of 41.5. To this Senator Percy responded, "if we are to base our judgment on this one criterion, then the situation is clear: The project ought to stay in the Midwest and it ought to go to Ann Arbor, Michigan."[85]

By contrast, Senator Phillip Hart of Michigan sided with Senator Pastore in believing that the facility should be delayed until equal employment opportunity could be ensured. In his statement, he pointed out that scientists in Michigan were pressuring him to approve the authorization for Weston and characterized their attitude as regretting "that Ann Arbor was not selected, but they are pleased that it is proposed to place the facility in the Midwest."[86]

Senator Bourke Hickenlooper of Iowa also took a regional viewpoint asserting, "I was very disappointed that a site which had been offered for this facility in the state of Iowa was not selected. . . . Nevertheless, I believe that the midwestern region of our country generally will benefit by placing the accelerator in the Illinois site."[87] Elsewhere in the debate Senator Hickenlooper said, "We do not get anything in the state of Iowa. The Federal Government has passed us up quite consistently. Every time we want anything, we just do not get it, that is all. But we do want it to be located with some proximity to where our people can use it."[88]

Other regional allusions in the debate included a statement by Senator Javits of New York, home of the Brookhaven site which was among the final six, that "My state and other states sought to have this installation and they failed. That is the end of that. It is finished, so far as I am concerned. So far as my State is concerned, the verdict is in but I do not believe that we should compound that verdict and the disappointment of other States by yielding on the matter of other major considerations."[89]

Senator Pastore added "Naturally, there is a little disappointment with some because the project did not go to Brookhaven. I understand all the reasons for it, and I am not seeking to change the Weston site. It is only a question of changing decisions that are complex, irritating, and disturbing to many people."[90] Senator Hickenlooper responded, "I do not believe that the desire for geographical location alone should override the considered judgment of the scientific fraternity of this country engaged in the Atomic Energy program."[91] He could say that since AEC had proposed a Midwest location.

The deep-seated feeling of the midwestern delegation held firm and it was clear that they did not really believe, despite the protestations, that the Easterners wanted only to delay the facility for a year. They felt it was an attempt to secure a full reconsideration and to take the facility away from the Midwest. Senator Hickenlooper made the point once more when he commented:

> Within a radius of approximately 250 miles from the location of this accelerator, which was considered to be the best location in the United States for it, nearly 50 percent of the Ph.D.'s are produced in the United States each year. They go elsewhere simply because the east coast and the west coast have succeeded, by some legerdemain, in capturing these rather important installations and sometimes, I may say, not to the advantage of the scientific programs. But for some reason or other they have succeeded in claiming those installations for their areas.[92]

One other regional dissatisfaction was voiced, although wrapped in an introduction of administrative analysis. Senator Warren Magnuson of Washington made a speech questioning whether the National Academy's selection team had done an adequate job. He asserted, "While I am not complaining about what is happening now, I want the record to be put there that this was the most ill-advised group for making selections that I have ever encountered – and I have had to deal with scientists quite frequently in the years I have been a Senator."[93]

Magnuson argued that the National Academy of Sciences had made a mistake when it eliminated five or six sites currently involved with atomic energy, one of which was in Hanford, Washington. His argument was that one of the prime criteria was to be the availability of low-cost power and that the Academy did not really give this the weight it said it would. He introduced into the record a consultant's report showing that the operating costs of the facility would be much more important over a long run than construction costs and that they would be much cheaper at a number of the AEC facilities, especially Hanford. He added that AEC had consistently underestimated operating costs at its facilities and was now doing it again.[94]

In a strike at the scientific establishment and the procedures followed by the National Academy of Sciences, Magnuson said,

> I think I know how these six sites were selected. I know the Academy people quite well. I know the people on the so-called selection committee. They finally ended up by throwing off the effects of the costs. I think the construction costs are pretty accurate and would apply perhaps to wherever the accelerator was constructed. The committee ascertained the personal desires of the people who might be involved as to the location. That should be a criterion, too. But we are dealing with a cost figure of an annual operation that will be just as important as the initial construction cost.[95]

The vote was 37 for the Pastore Amendment to delete funds for the Weston accelerator and 47 to reject the Amendment. The vote reflected a combination of regional, ideological, and party loyalties which were fairly predictable. The 16-0 vote by Southern Senators was the critical bloc in the failure of the Amendment.[96]

Conclusion

To say that politics had not been involved in the decision would be naive. However, this was not a political payoff to any one Senator, Congressman, or state. It appears that the Midwest made a strong regional campaign. According to Daniel Greenberg, the "elder statesmen of high energy physics" believe that if the Midwest had not set off such a political storm after the 12.5 Bev accelerator was not built at Madison, Wisconsin, the 200 Bev would probably have been built near Berkeley, California.[97] Given a predisposition to put it in the Midwest, the Weston site was so well located that it was difficult to justify not using it.

Even the timing of the decision was determined by political factors. Early in 1966, Representative Melvin Laird (R-Wis.) had predicted that the site for the atom smasher would not be selected until after the 1966 election. He said that Congress had been told that the accelerator was given high priority, that we needed to move rapidly because of anticipated advances Russia was expected to make in the field, but that there had been a deliberate slowdown in the selection of the site for political reasons. Since the decision was announced in December following the election, Laird was probably correct.[98]

Perhaps the most effective politics in the entire case was that practiced by the high-energy physicists. They succeeded in pushing through a major expenditure at the height of the Viet Nam War. In many respects, they held the high cards.

During the heat of the site selection debates, a related problem came up. This involved the question of who was to manage the accelerator and under what arrangement. In January 1964, while the formal proposal for the accelerator was being developed, a group of 26 university presidents met without public announcement and agreed to create an association of universities that would together seek to assume responsibility for the accelerator. At this same meeting, it was proposed that the Committee should offer its advice on site selection and other technical matters to the AEC. The group also resolved to broaden its base of support by inviting other major universities; to incorporate as an independent organization; and to offer its services to the AEC as manager of the proposed facility.

In his June 18, 1965, editorial comments in *Science*, Daniel Greenberg noted that while he agreed with most of what the group was doing, he thought it unfortunate that the group had organized without public notice or discussion. "Traditionally, that's the way of doing business at the summit of the scientific community, but it is not the way to inspire public or Congressional confidence, especially when the principal business of the association is expected to be the administration of a taxpayer-supported project."[99]

The Joint Committee on Atomic Energy agreed with the sentiments and during the High Energy Physics Research Hearings (March 2-5, 1965) asked some rather pointed questions of Dr. Seaborg, Chairman of the AEC, about the organization and how the AEC was involved with the group. The Committee was upset because it had not been apprised of the group and suspected that the AEC might be trying to use the group to insulate itself from Congressional control. As Representative Hosmer said,

> it seems to me that the high-energy fraternity has created a model of the best of all possible worlds with everything in it that they would like. It is kind of like a bunch of business tycoons sitting down and figuring out what taxes they are going to pay, and writing the laws to fit them.[100]

The Committee's fears were temporarily assuaged when they were assured that there had been no intention to bypass their authority; that the group would not play a part in site selection; that the group's function was purely advisory with no policy commitments; and that the JCAE would be subsequently apprised of all similar meetings and consultations.

Eventually, the AEC gave the management responsibility to the 48-university group named Universities Research Association, headed by Dr. Norman Ramsey of Harvard who had chaired the GAC/PSAC Panel which recommended the 200 Bev accelerator.[101] After all, there was no other group able to do it. There is a rule in politics that you can't fight something with nothing. The physicists have learned their politics well.

Notes

1. National Academy of Sciences, Site Evaluation Committee, *The Report of the National Academy of Sciences' Site Evaluation Committee,* Washington, D.C., March 1966, p. 1.

2. S. P. Thompson, *Michael Faraday, His Life and Work,* Cassell & Co. Ltd., London, 1901.

3. Daniel S. Greenberg, "Hi-Energy Physics: Major Fight Brewing as Midwestern Legislators Take Stand on MURA Accelerator," *Science,* October 11, 1963, p. 210.

4. "Scientific Prize of Century," *U.S. News and World Report,* January 3, 1966, pp. 56-57 and Mayor Ilus Davis of Kansas City, Missouri, *Letter* to Senator Edward Long, May 12, 1966.

5. "Gift from the AEC: Weston, Illinois, Selected as Site of World's Largest Atom Smasher," *Business Week,* December 24, 1966, pp. 17-18.

6. B. Nelson, "200 BEV Accelerator Moving Into Wasp's Nest?" *Science,* June 30, 1967, pp. 1713-1716; D. S. Greenberg, "200 BEV: The Academy Committee Knew Where It Was Going," *Science,* April 15, 1966, pp. 326-329; and "Europe's Giant Atom Smasher," *Business Week,* July 8, 1967, p. 64.

7. United States Congress, Joint Committee on Atomic Energy, Subcommittee on Research, Development, and Radiation, *Hearings on High Energy Physics Research,* February 1965, p. 67.

8. Daniel S. Greenberg, "High Energy Physics: Major Fight Brewing as Midwestern Legislators Take Stand on MURA Accelerator," *Science,* October 11, 1963, pp. 208-210 and Greenberg, *The Politics of Pure Science,* New York: The New American Library, 1967, pp. 213-4 and 223-4.

9. U.S. *Congressional Record* – House, February 27, 1964, p. 3928.

10. U.S. Atomic Energy Commission, "AEC Selects Site for 200-BEV Accelerator," *Press Release No. J282,* December 16, 1966, p. 6.

11. Daniel S. Greenberg, *The Politics of Pure Science,* pp. 253-4.

12. Ibid., pp. 241-3.

13. Ibid., pp. 261-7 and Atomic Energy Commission, *Press Release G-14,* January 20, 1964.

14. Spencer Klaw, "The Nationalization of U.S. Science," *Fortune,* September 1964, p. 160 and Greenberg, *The Politics of Pure Science,* pp. 209-210 and 267.

15. "200 BEV – Beginning or End?" *Science News,* December 31, 1966, p. 571.

16. Robert C. Cowen, "Studies Cut Cost of Atom Smasher," *Christian Science Monitor,* November 20, 1967.

17. Daniel S. Greenberg, "Hi-Energy Politics," *Science,* March 19, 1965, pp. 1423-1426.

18. U.S. Congress, Joint Committee on Atomic Energy, *Hearings, Authorization Legislation for Fiscal Year 1965,* Part III, p. 1478.

19. U.S., President's Science Advisory Committee and General Advisory Committee of the Atomic Energy Commission, *Report of the Panel on High Energy Accelerator Physics* (TID-18636), April 23, 1963.

20. U.S. Atomic Energy Commission, *Status Report on 200 BEV Accelerator*, April 24, 1965.

21. U.S. Congress, Joint Committee on Atomic Energy, Subcommittee on Research, Development, and Radiation, *Hearings on High Energy Physics Research*, February 1965, p. 347.

22. Ibid., p. 10.

23. "Plea for Atom Smasher," *Science News Letter*, February 6, 1965, p. 85.

24. "U.S. Scientific Luxury," *New York Times*, July 16, 1967, Section 4, p. 12.

25. Lawrence Radiation Laboratory, *200 BEV Accelerator Design Study*, University of California (UCRL-1600) June 1965.

26. U.S. Congress, *Congressional Record*, 89th Congress, May, 1966, pp. 10033 and 10169 and 90th Congress, June 1967, pp. 17895-17921 and July 1967, pp. 18507-18544.

27. Victor J. Danilov, "Federal Contract Research Centers," *Industrial Research*, April 1967, pp. 78-84 and U.S. Congress, Joint Committee on Atomic Energy, Subcommittee on Research, Development, and Radiation, *Hearings on High Energy Physics Research*, pp. 51-53 and 108.

28. J. Walsh, "CERN: The Strong Focus is on the 300 BEV Machine," *Science*, February 3, 1967, pp. 544-546 and "200 BEV-Beginning or End?" *Science News*, December 31, 1966, p. 571.

29. Daniel S. Greenberg, "Hi-Energy Politics," *Science*, March 19, 1965, p. 1423.

30. U.S. Atomic Energy Commission, "AEC-NAS Interagreement on Evaluating Sites for a Proposed New National Accelerator Laboratory," *Press Release No. H95*, April 28, 1965, p. 2.

31. Ibid., pp. 1-2.

32. Ibid., attachment to p. 1.

33. United States Atomic Energy Commission, "AEC Selects Sites for 200 BEV Accelerator," *Press Release J-282*, December 16, 1966 and listings supplied by the Atomic Energy Commission; "AEC Asks NAS to Evaluate 85 Site Proposals for 200 BEV Accelerator," *Press Release H-207*, September 15, 1965; "Wide Geographic Distribution Shown in AEC List of Proposals for 200 BEV Accelerator," *Press Release H-160*, July 9, 1965.

34. National Academy of Sciences, *The Report of the National Academy of Sciences' Site Evaluation Committee*, Washington, D.C., March 1966, pp. i and ii.

35. Ibid., p. 7.

36. Ibid., p. 3

37. Ibid.

38. Daniel S. Greenberg, "New Accelerator-Wide Open Race," *Science,* June 18, 1965, p. 1571.

39. "Scientific Prize of Century," *U.S. News and World Report,* January 3, 1966, pp. 56-57.

40. Daniel S. Greenberg, "200 BEV: The Academy Committee Knew Where it Was Going," *Science,* April 15, 1966, p. 327.

41. Ibid.

42. Ibid.

43. U.S. Congress, *Congressional Record,* 89th Congress, March 29, 1966, p. 6929.

44. Ibid.

45. Ibid., May 9, 1966, pp. 10107-10112 and pp. 13239-13240.

46. U.S. Atomic Energy Commission, "AEC Selects Sites for 200 BEV Accelerator," *Press Release J-282,* December 16, 1966, p. 1.

47. Ibid., p. 2.

48. Ibid.

49. Ibid.

50. Ibid., attachment p. 4.

51. Ibid., attachment p. 5.

52. Ibid., attachment pp. 6-7.

53. Ibid., attachment p. 2.

54. U.S. Congress, *Congressional Record,* 90th Congress, July 12, 1967, pp. 18507-18544.

55. Ibid., p. 18508.

56. Ibid., p. 18525.

57. Ibid.

58. Ibid., p. 18509.

59. Ibid.

60. Ibid., p. 18510.

61. Ibid.

62. Ibid., p. 18522.

63. Ibid., p. 18508.

64. Ibid., p. 18522.

66. Ibid.

67. Ibid.

68. Ibid., p. 18511.

69. Ibid., p. 18515.

70. Ibid., p. 18538.

71. Ibid., p. 18525.

72. Ibid., p. 18519.

73. Ibid.

74. Ibid., p. 18524.
75. Ibid., p. 18534.
76. Ibid., p. 18536.
77. Ibid., p. 18533.
78. Ibid., p. 18526.
79. Ibid., p. 18533.
80. Ibid., p. 18511.
81. Ibid., p. 18510.
82. Ibid.
83. Ibid., p. 18533.
84. Ibid., p. 18534.
85. Ibid., p. 18524.
86. Ibid., p. 18515.
87. Ibid., p. 18530.
88. Ibid., p. 18531.
89. Ibid., p. 18511.
90. Ibid., p. 18533.
91. Ibid.
92. Ibid., p. 18531.
93. Ibid., p. 18527.
94. Ibid.
95. Ibid.
96. Ibid., p. 18543.

97. Daniel S. Greenberg, "200 BEV: Illinois Chosen in Competition for New Accelerator," *Science,* December 23, 1966, p. 1528.

98. "Atom Smasher Sites," *Congressional Quarterly Weekly Report,* October 1, 1965, p. 2011.

99. Daniel S. Greenberg, "Editorial Comments," *Science,* June 18, 1965, p. 1573.

100. U.S. Congress, Joint Committee on Atomic Energy, Subcommittee on Research, Development, and Radiation, *Hearings on High Energy Physics Research,* 89th Congress, 1st session, March 1965, p. 127.

101. U.S. Atomic Energy Commission, "AEC Awards Contract for 200 BEV Work," *Press Release L-10,* January 23, 1968.

TOWARD THE ENDLESS FRONTIER

History of the Committee on Science
and Technology, 1959–79

By KEN HECHLER

COMMITTEE PRINT

U.S. House of Representatives

U.S. GOVERNMENT PRINTING OFFICE
35-120 O WASHINGTON : 1980

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, D.C. 20402

GEOGRAPHIC DISTRIBUTION OF RESEARCH CONTRACTS

The authorization bill which was passed on May 6, 1965, included an amendment which Representative J. Edward Roush (Democrat of Indiana) had inserted in the committee markup of the bill:

> It is the sense of Congress that it is in the national interest that consideration be given to geographical distribution of Federal research funds whenever feasible and that the National Aeronautics and Space Administration should explore ways and means of distributing its research and development funds on a geographic basis whenever feasible and use such other measures as may be practicable toward this end.

The location of NASA installations and the geographic distribution of research contracts were issues which were intensely debated within the committee from the start of the space program. Bobby Baker, in his book *Wheeling and Dealing* alleges that he worked through Senator Robert Kerr (Democrat of Oklahoma) and Vice President Lyndon Johnson to persuade NASA Administrator Webb to intervene on behalf of North American Aviation for the multibillion-dollar Apollo-Saturn contracts, thus enabling Baker to install his Serv-U automatic vending machines in North American plants. No proof of this allegation has ever been forthcoming. But the awarding of large contracts was frequently accompanied by intense argument over whether certain sections of the country were being favored.

Every member of the committee, with varying degrees of success, vigorously represented his own district and State when it came to the awarding of contracts or the funding of programs. Thus it was not unusual to see Miller and Bell active on behalf of some California projects, Mosher raising the flag for Lewis Research Center and Plum Brook, Downing standing up for Langley Research Center, Teague and the Texans plugging for Houston and Dallas, while Fuqua, Gurney, and Frey were interested in pushing everything which happened at Cape Canaveral-Kennedy.

Perhaps the most fascinating story about geography, Congress, and the space program occurred with respect to the location of the Manned Spacecraft Center in Houston. Keith Glennan, NASA's first

Administrator, reported that soon after taking office he had a telephone call from Representative Albert Thomas (Democrat of Texas), chairman of the Independent Offices Appropriations Subcommittee handling NASA's appropriations. Thomas said to Glennan:

> Doctor, I just want you to know how grateful I am that you're willing to come down here and take on the duties that you're taking on, and I want to be as helpful as I can. Now, Doctor, you're going to need some more research and development. I just want to tell you that down there in Houston, there's an institution known as Rice.

Glennan said: "Albert, I know Rice very well indeed."

At this point, Thomas mentioned that Rice had a sizable chunk of land, adding:

> I know Rice would give this land to the Government as a location for a research development laboratory.

Glennan replied:

> But Albert, we don't need any more laboratories. We have all that we need at the moment, and we're building one which was started before I came * * * out at Beltsville.

On subsequent visits to Capitol Hill, Glennan dropped in to see Thomas, and "each time he would bring up this same matter and I would turn it off."

Glennan then reported:

> Finally, I had a call from him one day, and I would have to guess this would be in the spring of 1959.
>
> "Doctor, about that research center matter down in Houston."
>
> I said, "Albert, you know, we've been over this several times and I have told you very frankly that I can see no reason for spending money for this and until there's a need—there may be at some point in time—I'm just not going to think about asking for money for a research center. We're going to finish the one that we've started."
>
> (Thomas responded): "Now, Doctor, let's stop all the horseshit. I've got your budget in front of me." I've forgotten the number but I think he said: "There's $14 million in there for Beltsville."
>
> I said, "Well, you know more about it than I do. I don't remember it in that detail."
>
> (Thomas): "Well, let me tell you, Bud, you won't get a nickel of that unless you put a research center at Houston."
>
> And I had sense enough to react by laughing and saying: "Now, Albert, I think it's about time you and I went out and had a drink."
>
> Well, that ended it, and I never did have any more arguments with him about that.
>
> Now, time passes and I go back to Case in Cleveland and Jim Webb takes over * * *.
>
> So the word was out that there was to be a manned space center someplace, and the Governor of Ohio called me one day and asked me if I would undertake to * * * put together a story nominating Ohio as the site for this center. And I just broke out laughing. I said: "You know, I suppose that there are 25 States doing just this at the present time, and I'll lay you a year's salary that that center is going to Houston."

When Webb succeeded Glennan as NASA Administrator, Thomas enlisted the aid of his old college roommate, George R. Brown, a heavy contributor to Lyndon Johnson's campaigns and a close personal confidant of Johnson. Brown, head of the big Houston engineering-construction firm of Brown & Root, had already been one of the most active consultants for Johnson in the Vice President's mission to prepare a space program prospectus for President Kennedy. On May 23, two days before the President's personal appearance before Congress to announce his Moon decision, Webb wrote Vice President Johnson:

> In other discussions with Congressman Thomas, he has made it very clear that he and George Brown were extremely interested in having Rice University make a real contribution to the effort, particularly in view of the fact that some research funds were now being spent at Rice, that the resources of Rice had increased substantially, and that some 3,800 acres of land had been set aside by Rice for an important research installation. On investigation, I find that we are going to have to establish some place where we can do the technology related to the Apollo program, and this should be on the water where the vehicles can ultimately be barged to the launching site. Therefore, we have looked carefully at the situation at Rice, and at the possible locations near the Houston Ship Canal or other accessible waterways in that general area.

Webb went on to say that California, Chicago, and the research triangle in North Carolina ("in which Charlie Jonas as the ranking minority member on Thomas' appropriations subcommittee would have an interest") were other candidates for space installations. Webb wrote Johnson that "I am convinced, and believe you should consider very carefully, that the merit of this program will attract the kind of strong support that will permit the President and you to move the programs on through the Congress with minimum political infighting."

Thus when many States and communities other than Ohio began to burn the midnight oil to put their proposals together, they had no Keith Glennan to break out laughing and bet: "I'll lay you a year's salary that the Center is going to Houston." Not until after President Kennedy's May 25, 1961, decision to go to the Moon was clearance received for budget approval of the funds. On September 19, 1961, Webb confirmed the fact that Houston had been selected in preference to 20 other cities submitting proposals. Glennan's scenario was accurate.

On April 5, 1962, Teague's Manned Space Flight Subcommittee held a public hearing which included a review of the Houston land deal. As Teague pointed out during the hearing, Humble Oil Co. conveyed most of the land to Rice University free of charge, on condition it would be reconveyed to NASA. When NASA found the need for additional acreage, NASA purchased additional land from Rice which had been conveyed through Humble Oil Co.

PART VII

CONGRESSIONAL AND EXECUTIVE ROLES IN NATIONAL SCIENCE POLICY

BACKGROUND AND SUMMARY

The role of Congress and the Executive in national science policy has assumed greater importance since World War II as the federal government has become the principal patron of scientific research in the United States. How these roles have evolved and how Congress has interacted with the executive branch and the scientific community is the subject of the selections in Part VII.

The first selection, "Science and Politics: The United States of America" provides a broad overview of the political structure of science policy. Lyons observes that instead of contributing to the shrinkage of the domain of politics (as Robert Lane argued in the article reprinted Part III of this reader), "politics has increasingly influenced science as much as science has influenced politics. The 'politization' of science has come about in two ways. In their role as advisors of consultants, scientists have often been drawn into political controversies, however, 'neutral' and 'value-free' they have tried to be in presenting their evidence. . . . At the same time, scientists have been forced into the political position of advocating public support for basic research as the federal government has become a major patron of science. . . . Scientists have, in sum, become 'politicized' actors in the policy process no less than politicians have become 'scientified' by their growing need to call upon the information and analytical skills of the scientific community" (pp. 26–7).

Lyons notes that there are three themes that dominate relations of science and politics in the United States: decentralization; the "scientification" of politics; and the "politization" of science. After reviewing these themes in terms of the structure and functioning of the executive branch and Congress in policy formulation for scientific research, Lyons uses Harvey Brooks' distinction (see Part IV of this reader) between "policy for science" and "science in policy" to disaggregate the roles of science and technology in American politics. (The former refers to policy for the support of fundamental scientific research, while the latters refers to research findings in the formulation of national policies and the performance of agency programs). Lyons argues that "four sets of variables—the state of knowledge, the perception of expertise, the nature of the policy issue, and its political salience—combine to determine the role of science and technology in matters involving 'science in policy.' But they are also applicable to 'policy for science' because "[e]ven though the federal government has accepted a primary commitment to support basic research in all the sciences, the level of support and the distribution of resources among the different disciplines and fields are subject to continuing debate and controversy" (p. 36).

Lyons concludes his analysis with three observations about the American system of science policy making: First, "the impulse toward a short-range view of research planning . . . can be detrimental to the long-range needs of basic research and obscure the long-range implications of science and technology in matters of social economic or foreign policy." Second, "both the American political system and the multifaceted nature of science policy, bar highly centralized solutions to the problem of long-range planning. Finally, there are multiple points in the system—and not just one central focus—at which the case for a long-range perspective can be argued: within the presidency; in the bureaucracies; and in the congressional committees" (pp.36–7).

In the second selection, Sanford A. Lakoff's "Congress and National Science Policy," the involvement of Congress in science policy is addressed specifically. Lakoff begins by observing that the "role of Congress in setting and maintaining science policy has never been very defined or delimited. . . . In general, . . . the pattern and direction of federally supported research and development has been set in the executive branch and embodied in the budget presented annually to Congress" (p. 589). But because the deployment of scientific resources is a critical instrument of government action, Lakoff speculates (in 1974) that Congress is likely to assert more authority in science policy in the future.

According to Lakoff, two conditions are propitious for a stronger congressional role in science policy: the decline in the executive apparatus for setting science policy and a shift in public opinion from a frustration with the uses to which science has been put toward a recognition that science might solve important problems like the energy shortage and environmental pollution. Congress in turn has strengthened its ability to assert more control over science policy through internal reorganization and capacity-building moves like the creation of the Office of Technology Assessment (OTA) and the Science Policy Research Division of the Congressional Research Service. But, Lakoff cautions, for Congress to take a broader role in science policy it will need to depart from its past practice of focusing on one issue at a time.

After briefly reviewing the record of congressional involvement, Lakoff concludes that there are several structural weaknesses in the way Congress addresses issues of science policy, principally its fragmentation. Because the Congress is a far less unified branch than the executive, it has been unable "to develop and maintain a continuous, comprehensive, and interally consistent approach to the variety of elements that make up science policy" (p. 597). One way to encourage such a comprehensive view, according to Lakoff, would be to enact a statutory national science policy which would strengthen the nation's commitment to the development of science. Whether Congress chooses this approach is less important, Lakoff argues, than the fact that it will undoubtedly assume a larger role in issues of science and technology: "The case for a larger role for Congress rests on the essential principle that decisions concerning the uses of science and technology are so vital to the citizenry that they should not be made in secret or bureaucratically or by the executive alone. The fact that this principle has been so egregiously violated in recent years should spur rather than deter congressional efforts to subject major policy issues to informed legislative scrutiny" (p. 610).

The third selection in this section, Patrick W. Hamlett's "A Typology of Technological Policy making in the U.S. Congress," focuses upon two dimensions of public policy that affect the coherence of long-term technological policies made in Congress. The first dimension is a continuum of "policy discontinuity," which "assesses the degree of deviation from established practices and politics in the area under consideration. The policy option chosen by Congress may vary from a simple ressertion of existing policies, with no change, to relatively modest modifications of existing policies, to truly radical departure from the established situation" (p. 33).

The second dimension is a continuum of "technological integration" which "assesses the relative degree of concentration or decentralization in the network of social, economic, and political institutions and relationships surrounding a technology, that is, the 'technological infrastructure' " (p. 34). Given the interests of the groups in a established network surrounding a technology—engineering designers, manufacturers, retailers, consumers, and so forth—Hamlett observes that such a technological infrastructure "will support the continuation of favorable public policies affecting their technology, and oppose change in policy perceived as potentially disruptive" (*ibid.*). But technology infrastructures vary as to their degree of internal concentration or decentralization. A concentrated infrastructure will have relatively few interested groups and firms while decentralized infrastructure (like railroads), which often develop around nature, standardized, non-exotic technologies, involve a large number of suppliers and customers scattered around the nation. "The importance of technological integration for congressional decisions derives from the differing political tactics that characterize decentralized versus concentrated infrastructure. Each kind of infrastructure presents a different set of political opportunities and constraints to congressional policymakers" (*ibid.*).

Hamlett combines the two continuums into a four cell idealized typology and provides some examples from the areas of defense, power, transportation, and environmental policy to illustrate each type. He observes that concentrated infrastructures, because they have fewer participants, offer Congress a greater opportunity to make and implement long-term technology management decisions. "Because virtually any decision about an existing technology affects various groups differently, the greater the number of participants in the decision process, the greater the difficulty for Congress in defining technologically responsible policy goals and in structuring programs to achieve those goals. This is especially true when Congress must choose to switch technologies, reducing or eliminating programs favorable to one technology in favor of another. Such decisions become necessary as technological developments and breakthroughs, changes in constituent demands, fiscal constraints, or other political changes press upon existing policies" (p. 35).

Dealing with concentrated infrastructures allows Congress a degree of policy insulation from cross cutting political pressures to make long-term technology management decisions. "Relatively free of pressure politics, Congress may be able to create policies more congruent with emerging technical options, policies which may be more beneficial to the majority of people even if temporarily harmful to those attached to the existing policies" (p. 37). On the other hand, decentralized infrastructures, although promoting sacrifices in policy coherence and long-term planning, "offer many opportunities for individual members of Congress to display their sensitivity to constituent demands." They allow members of Congress, an attractive opportunity "to develop and maintain a reputation of protecting local interests" (p. 38).

Hamlett concludes by noting an important irony in American technology policymaking: "Because concentrated infrastructures have fewer participants whose diverse demands must be somehow integrated into a single policy, they are better policy partners for those members of Congress interested in long-term policy management. However, they are also frequently less accountable to the public specifically because they have fewer participants in the decision process. Hence, they often lack safeguards against manipulation by special interests, and sometimes produce policies benefiting only infrastructure members. Their potential for long-term management for the common good may often be subverted by their undemocratic character" (p. 39).

In the fourth and fifth selections in this section, the focus shifts to executive branch. The fourth selection, Willis H. Shapley's "Executive Leadership for Science and Technology," addresses the issue of how best to organize science advice to the president. Shapley's basic thesis is that the current (circa 1980) concept of the Office of Science and Technology Policy (OSTP) "is generally good and working well, but that there is additional need for a strong focus of central leadership in Federal science and technology which should not and does not need to be in the Executive Office. . . . [T]he goal should be a minimal Science Advisor's office in the Executive Office supported by a heftier 'Agency for Science and Technology' which, in addition, would perform central planning and leadership functions and bring together a group of federal agencies directly concerned with science and technology" (p. 165).

Shapley begins his discussion by describing OSTP in 1980—its role, projects, and achievements since its reestablishment during the Carter Administration. He asks: "Have we now reached a viable long-term organizational pattern for advice and leadership on science and technology in the Executive Branch?" (p. 167).

After concluding that "OSTP is doing a first-rate job in meeting the general expectations to the degree possible within the external constraints," he adds that in some respects it falls "short of the expectations of the congressional authors of the legislation mandating its recreation" (p. 167). This is because several of the functions "spelled out in great detail in the law, have been down-graded or eliminated entirely. The initial reluctance of the Carter Administration to accept any science and technology presence in the Executive Office, because of a strong commitment to reducing its total size and suspicion of special pleading for science and technology, was overcome only at the price of restricting the size of OSTP and of removing some of its statutory functions" (*ibid.*).

Shapley argues that it seems doubtful that OSTP as now conceived (in 1980) can fully meet the needs of the Executive Branch for central leadership on science and technology. That "job is too big for a staff office in the White House. There is a need for dealing with more than just the most pressing attention-demanding problems" (p. 168). Arguing that "the present arrangement is inherently unstable" because it is too dependent "on the personal understanding among the principals in the Executive Office and on the acceptance of OSTP by the other [science and technology] bureaucracies there entrenched" (p. 170), Shapley states the case for the creation and institutionalization of "a new, permanent, strong, independent agency" in addition to OSTP. With broad central responsibilities for advice and leadership in science and technology in the Executive Branch, such an agency, which Shapley would call the Agency for Science and Technology (AST), would be headed by a Presidential appointee of Cabinet rank who "would be the principal official in the Executive Branch responsible for policy, planning, advice, and general leadership on matters involving science and technology that are not clearly within the scope of responsibilities of other departments and agencies" (*ibid.*). In addition to assuming many of the responsibilities currently performed or expected of OSTP, the AST would have greater capabilities to provide advice and leadership to other science and technology agencies in the executive branch without duplicating or interfacing with their activities and responsibilities. Such a role, Shapley concludes, is needed and consistent with several previous ideas looked upon favorably by Congress.

A somewhat different view of science policy making in the executive branch is provided in the selection by James E. Katz, "National Science Policy and the Office of Mangagement and Budget" from his book, *Presidential Politics and Science Policy*. Katz provides "an analysis of the authority of the OMB over science policy: when, why, how and by whom . . . the OMB's power [is] brought to bear on the scientific research enterprise" (p. 62).

Katz begins his analysis by observing that "the OMB is more distinctly and systematically involved in the process of science-policy construction, implementation, and evaluation than the rest of the presidential offices" (p. 62). This is in large part because the OMB has unique responsibility for the management and supervision of all government-sponsored research operations: "It is through the OMB . . . that the general expression of the leadership of the president is translated into specific science policy on a broad-based operational basis" (p. 65).

Katz notes that although some responsibility for issues of science policy and programs is scattered around divisions within OMB, the broadest responsibility for dealing with matters of science policy lies with the Division of Science, Space, and Energy Technology. It is OMB's job to "to single out for scrutiny policies and programs which appear to have significance under the president's priorities. In this context, 'significance' means research with putative negative or positive payoffs for presidential power, influence, and prestige or support positions expressed in presidential statements and directives" (p. 66). As a result, the OMB is a pivotal political player in science policy issues, particularly with respect to relations between the executive branch and Congress, between the White House and the executive branch agencies, and within and between the agencies themselves. In addition, "[p]olicy regarding the procedures involved in science research, grant management, and budgeting guidelines are also under OMB's authority. The OMB has a sizable influence in creating guidelines for allocating indirect costs to research grants; developing principles of cost sharing for grants to universities; and establishing common systems for auditing grants dispensed by various federal departments" (p. 68).

At a broader level, OMB serves as a broker to: 1) "maintain a balance between various discipline and styles of scientific research; 2) ensure comprehensiveness of program activities, to guard that no lacunae develop in important areas and also prevent overlap between agencies' research activities; 3) evaluate research, both in terms of adequacy of research funding and quality of research effort in the laboratory; and 4) attempt to maintain a high level of significance in government-sponsored research" (p. 69). In doing so, OMB staff meets occasionally with groups of scientists, representatives of scientific associations and agency officials. Although "OMB rarely involves itself with the internal methodologies and arguments of scientific research, . . . it is concerned that the research undertaken does have a redeeming value and is not simply wasting funds on 'useless' research or vitiated by inadequate research techniques" (p. 73).

OMB exercises its greatest influence over science policy by setting budget priorities for science. The major portion of OMB science division manpower is assigned to the program-evaluation-and budgeting process. Although OMB's power of the purse is usually exerted negatively to reduce agencies' budget requests, it also shapes the general content of programs to reflect presidential priorities. In both roles, OMB examiners are bound to frustrate agency advocates in the bureaucracy and in Congress.

After reviewing the role of cost/benefit analysis and other tools to rationalize the decision-making process in OMB, Katz turns his attention to the "politicization" of OMB after its reorganization in 1970. Katz cites Heclo and others who argue that since its reorganization OMB "lost the appearance of 'neutral competence' and instead represents the short-term political interests of the president, and these short-term plans can be very destructive to the long-range operation of many agencies." Yet the rationale behind politicization—"to make the OMB more responsive to the president's political dimension" (p. 96)—Katz argues, is also an important function of the agency. In the area of science policy, the clearest example of the politicization process is the OMB's sharpened attack on the peer-review system, for better or worse.

Katz concludes by observing that OMB has been one of the most important mechanisms in government for determining and regulating the scope and level of national policy for scientific research. In the future, Katz believes that OMB will go in two directions at once: the budget-and-management process will be increasingly rationalized, and OMB will become even more politically responsive to the White House. Because OMB receives encouragement to take even greater responsibility for guiding the national scientific research effort, Katz expects that it will continue to gain power. However, as it does so, some questions will need to be addressed: To whom is OMB accountable and responsible? "Is it only to the "political" president the particular needs of a particular man in power? Or is it ideally to the agencies the OMB examines as well (and by extension, the people and constituencies these agencies reflect)? [Finally,] what tradeoffs are necessary to achieve a balance between these responsibilities, and where should the fulcrum of the balance be placed?" (p. 97).

A. CONGRESS' ROLE: OVERVIEW, HISTORY, APPRAISAL

Science and Politics: United States of America

Gene M. Lyons

It has frequently been said that decisions about science and politics in the United States are made through a process characterized by pluralism and decentralization. This is true and it is a good starting-point for discussion. But these characteristics only help orient us to the way to think about science and politics in the United States. They are descriptive of American politics more generally and not just the interface with science. They do not, for that matter, really tell us very much about how decisions are made and how much influence scientists have in 'who gets what, when and how' (to use Lasswell's cryptic definition of politics).

It has also been said that the imperatives of science and technology have increasingly influenced the nature of politics in the United States. The President makes increasing use of expert advisers; the Congress, less systematically and more slowly, has called on scientists to serve on committee staffs and testify before congressional committees on a broadening variety of legislative issues; and interest groups, in their efforts to influence federal laws and budgets, make prominent use of scientific and technical consultants. But we are a far cry from the 'knowledgeable society' that Robert Lane spoke about a decade ago, in which 'the political domain is shrinking and the knowledge domain is growing, in terms of criteria for decisions, kinds of counsel sought, evidence adduced, and nature of the "rationality" employed'.[1] In matters such as energy policy, economic stability and an international régime for the oceans, there is every evidence that, granted the importance of science and technology, the 'political domain' is as broad and tumultuous as it ever was.

There is, of course, the reverse side of the coin. Politics has increasingly influenced science as much as science has influenced politics. The 'politization' of science has come about in two ways. In their role as advisers or consultants, scientists have often been drawn into political controversies, however 'neutral' and

---

Gene M. Lyons is Dean of the Faculty of Arts and Sciences and professor of political science at Dartmouth College, Hanover, New Hampshire. He is a former director of the Department of Social Sciences of Unesco and has published extensively on various problems of policy. He edited the proceedings of a conference organized by Dartmouth College and OECD, published under the title of Social Research and Public Policies *(1975).*

International Social Science Journal

Int. Soc. Sci. J., Vol. XXVIII, No. 1 1976

'value-free' they may have tried to be in presenting their evidence. Their expertise has been subject to various interpretations: either because research findings are incomplete and thus their testimony partly speculative; or because the issues involve the social and economic consequences of their knowledge rather than the knowledge itself. At the same time, scientists have been forced into the political position of advocating public support for basic research as the federal government has become a major patron of science. They have had to make a politically acceptable case for supporting basic science and defend the principle of peer review as a procedure for deciding which projects to fund. Scientists have, in sum, become 'politicized' actors in the policy process no less than politicians have become 'scientified' by their growing need to call upon the information and analytical skills of the scientific community.

These then are three themes that dominate the relations of science and politics in the United States: decentralization; the 'scientification' of politics; and the 'politization' of science. They are themes that interact in a variety of complex ways. They create tensions in decision-making processes. 'Scientification', for example, tends to encourage centralized, rational planning which is then diluted by 'decentralization' and the 'politization' of science. The fact is that nothing that can be strictly termed 'science policy' emerges from the American political system. But, for many, nothing much is lost in the process. Nothing much is lost because science policy is not a singular phenomenon but, if anything, a pluralistic enterprise. The whole thrust of American politics militates against total solutions to complex problems.

The limits of centralization in the executive branch

Early in 1973, President Nixon abolished the complex of agencies that had grown up within the White House staff: the President's Special Assistant for Science; the President's Science Advisory Committee (PSAC); and the Office of Science and Technology (OST). The President explained that this machinery of scientific advice and co-ordination was no longer necessary. Scientific bureaux were now well established within all the major executive departments and agencies and scientific advice was available through the normal channels of budgetary and programme clearance. When he needed special counsel, he went on, he could turn to the Director of the National Science Foundation whom he designated as a presidential special assistant for science, but without a real place in the White House or staff support in the Executive Office of the President.

A scientific presence in the White House is now being restored by President Ford although the new office will be smaller and less central than the earlier OST-PSAC arrangement. In a recent statement before a congressional committee, Vice-President Nelson Rockefeller explained that the President would create a new

Office of Science and Technology Policy (OSTP) that would consist of a director and professional staff of about fifteen. No continuing group like PSAC is planned, though the OSTP could constitute *ad hoc* panels to assist in its work. The major responsibility of the office, according to the Vice-President, will be 'to be available for participation in broad reviews of policies and programs of the departments and agencies having technical responsibilities and thus to assist in the formulation of national policy on technical and scientific matters'.[3]

The abolition of OST-PSAC by President Nixon and the setting up of a less elaborate OSTP by President Ford, illustrate the limits of centralization in scientific and technological planning in the United States. The fact is that the most important reason for representing science in the White House is not to have a central structure for planning scientific research and overseeing its application in government programmes. The major reason is to permit the scientific community to have direct access to the President in order to solicit his political support for the interests of research and to provide him with alternative sources of scientific advice for contending with social and economic problems. The purpose is to influence the President in a governmental system in which initiative and cohesion largely emerge from the exercise of presidential authority.

Influencing the President does not, however, come automatically to scientists (or any other group) from being established, in a formal sense, in the Executive Office. A President cannot be influenced if he does not want to be. The earlier White House arrangements were first created in 1958 in response to the needs of then-President Eisenhower for direct scientific advice in matters dealing with defence and space exploration. At the time, weapons and space developments were being carried on by the three military services, the army, navy and air force, in a rivalry for a dominant role in American military strategy. There was as yet no strong central direction in the Office of the Secretary of Defense and no channels through which independent scientific and technological advice could reach the highest levels of government.

The President thus took a series of actions: he strengthened the centralized mechanisms of the Pentagon to co-ordinate weapons development, created a new National Aeronautics and Space Administration (NASA) to take charge of the space programme, and established, in his own office, a Special Assistant for Science and the President's Science Advisory Committee (PSAC). The President's Assistant became chairman of PSAC, as well as the Federal Council on Science and Technology, the latter made up of the research directors of government departments and agencies and thus, in theory, a mechanism for research co-ordination.

The system for presidential science advice was augmented in 1962 when President John Kennedy established the Office of Science and Technology (OST) to provide staff support for the Special Assistant and PSAC. One of the most important functions of OST was the assistance it rendered to the then-Bureau of the Budget (now designated the Office of Management and Budget) in reviewing the research estimates of the executive agencies before they were incorporated into the President's annual budget presented to the Congress. It was the Budget Office, more than the Federal Council on Science and Technology, that provided co-ordination, or at least a basis for co-ordination of research planning.

OST and PSAC began to expand the scope of their interests beyond weapons development and the space programme early in the 1960s. PSAC reports, for example, covered a range of subjects from the needs of higher education and scientific manpower to the world food problem, health care and the effective use of the seas.[5] The composition of PSAC also changed over the years. At first made up largely of physical scientists and engineers, biological scientists were added to the committee and, by the late 1960s, social and behavioural scientists as well. By the time it was abolished by President Nixon, the OST-PSAC structure covered the full spectrum of research activities that had developed in the federal government since PSAC's establishment fifteen years earlier.

As OST-PSAC grew and expanded, however, its influence in presidential decision-making diminished until, by 1973, the access of the Special Assistant for Science to the President was so limited that he was not even consulted on the deletion of his own office.[6] I do not mean to suggest that there is any natural correlation between an expansion of function and a reduction of influence. There is, however, a point to be made. So long as OST-PSAC had a specified purpose in an area of presidential need—scientific and technological advice on weapons and space developments—the scientists and engineers from whom the President sought advice, unusually had important influence. During the early years of OST-PSAC, the President's Special Assistant for Science and the advisers and consultants on PSAC and his staff also had a virtual monopoly in being able to tap the scientific community for government assignments. These three elements, taken together, were important factors in ensuring that their advice was taken seriously and was often decisive: clear aims, presidential need and domination, if not a monopoly, over the sources of expertise.

By the mid-1960s, at least, all three of these elements became diffused. The aims of OST-PSAC and the needs of the President for scientific advice widened over a larger number of government programmes, involving a greater set of political complexities and political interests. The demands of defence and the space race with the Soviet Union, previously powerful directing forces, become more ambiguous with an increasing recognition of the limits of military power in a more pluralistic and less sharply bi-polarized world. Domestic problems, such as urban rebuilding, income distribution and health benefits, began to crowd the agenda of Lyndon Johnson's administration and bring pressures on new and more controversial fields of research than the physical sciences and engineering. The environmental issue began to emerge as substantial in American politics, bringing with it a serious questioning of technological change and the benefits of scientific progress. Finally, scientific and technological units were strengthened throughout the executive departments, creating competing groups of experts, tied to the bureaucratic interests of their own agencies rather than the broader presidential interests which the PSAC and OST represented.

Under these changing conditions, PSAC and OST no longer made up a cohesive group of specialists, sharing a common background in the physical sciences, a common set of professional relations, and a single aim in strengthening national security. Over a broader range of public programmes and with a more broadly-based group of scientists among advisers and staff, consensus became more difficult to achieve. The presidential advisory system became less effective by the mid-1960s than it had been earlier, less effective, that is, in influencing the President and, through the President, national policies.

There were other considerations as well. The Viet-Nam war became a divisive element in the scientific community, no less than in the society as a whole. Public stands against the war by scientific and other academic groups tended to create tensions between official scientific advisers, who suffered divided loyalty, and Presidents Johnson and Nixon. These tensions were exacerbated when PSAC took positions that were contrary to administration policies in the case, first, of the anti-ballistic missile system and, later, of the supersonic transport.[5] Finally, in the waning years of the Nixon White House, scientists, like many others, became shut off from presidential decision-making processes which became increasingly closed and restrictive.

The conditions of the Nixon White House were, hopefully, an aberration in American political life. They were certainly not decisive in the weakening of the presidential science structure; weakening had already set in. More important were the broadening of the structure, the difficulties of gaining consensus among advisers, and the bureaucratization of science and technology throughout the executive branch. Just as fundamentally, the nature of the problems involving science and technology had changed. As Edward David, the last presidential science adviser under the old system, put it:

> Science and technology were the controlling factors in military and space programs; they animated the whole decision process. But civilian research and development—for energy, transportation, health resources and so on—involve much more. In these cases, science and technology must share the driver's seat with economic, legal, and social factors, political considerations, and—most of all—human sensibilities. Science and technology are only part of the story.[6]

The effects of the committee system in the Congress

As problems change, so does the role of science and technology. The present is still, in many respects, a period of transition from a preoccupation with the pressures of a bipolar world to the more complex imperatives of interdependence in world politics and increasing concentration on domestic affairs. This change, in itself, encourages tendencies toward decentralization in American science policy. Still another factor that weakens centralization, however, is the impact of the congressional committee system on the executive branch. This, again, is a general phenomenon in American government but it has operated in the field of scientific and technological research in a somewhat different way than in other policy areas.[7]

The committees of the Congress, in both the House of Representatives and the Senate, run the gamut of public programmes and are the crucibles in which legislative proposals are tested and prepared for final decision. Up against the impossible task of dealing with all matters that are brought up for vote, congressmen and senators concentrate on a limited number of committee assignments. These usually conform to the particular interests of the state or district they represent or to special opportunities that they offer to enhance their political advancement within the Congress or more generally as national leaders. Thus, representatives from farming states seek seats on the agriculture committee and senators with presidential aspirations give high priority to seats on foreign relations or defence committees.

In such circumstances, any effort to consolidate or rationalize the committee structure of the Congress is usually doomed to frustration. Any substantial change is likely to imperil the position of enough senior members to bring about its defeat. Committees are more likely to be added than eliminated and new subcommittees created within committees to accommodate political ambitions, particularly of those who are members of the majority party. These tendencies toward fragmentation are compounded by the duplication of committee structure in the two Houses of Congress that harbour jealousies of each other's prerogatives and have only infrequently agreed to create joint committees to deal in common with legislative matters.

Multiple decision centres in Congress affect the executive branch in a variety of ways, all of which are relevant to policies for science and technology. However coherently the President may offer a science programme to the Congress, it will be carved up for review before the committees of both Houses. Through processes of addition, contraction and compromise, the President's programme is likely to emerge not only different than originally proposed, but more piecemeal. Once legislation comes up before the Congress, moreover, alliances begin to operate between specialized bureaucratic interests in the executive departments and the congressional committees with responsibility for their programmes and budgets. Over the years, the growth of the National Institutes of Health was as much attributable to the support of powerful committee chairmen as to that of Presidents Eisenhower, Kennedy and Johnson. A major task of the White House staff is to ensure that departments do not pursue their special interests with congressional committees at the expense of, or in contradiction to, the over-all purpose of the President's programme. As an example, a department's interest in increasing its budget for research and development must not be so aggressively pursued that it runs counter to the President's stated aim to maintain a limit on government expenditures as part of his anti-inflationary policy.

The congressional system leads, in some respects, to a kind of centralization within one or another House when a single committee holds a virtually exclusive jurisdiction over a policy area and its views are generally followed by the rest of the House or Senate. But this very condition only reinforces decentralization in matters dealing with science and technology. Science and technology are ubiquitous, linked with matters of health, defence, energy and the environment that are dealt with and coveted by different congressional committees. No committee will give up control over the research programmes and budget applicable to its own area of jurisdiction.

At the same time, committees in both the House and Senate that specifically focus on matters of science and technology have limited scope and only a narrowly construed jurisdiction over the totality of federal research activities.

The Joint Committee on Atomic Energy, for example, for years emphasized the exploitation of nuclear power to the point of minimizing the potentially serious consequences of such policies and ignoring research into other sources of energy. New committees in both Houses created in the late 1950s were almost exclusively devoted to oversight of the space programme. Even when the House Committee on Science and Astronautics developed a broader set of interests (and changed its name to the Committee on Science and Technology) its investigations were limited to organizational studies and recommendations and did not expand to substantive issues of scientific research which remained under the control of the functional committees of Congress, like agriculture and armed services. New co-ordinating tendencies in the Congress have come through the strengthening of central staff services, especially a new congressional budget office, the Science Policy Division of the Congressional Reference Service and the Office of Technology Assessment. But whatever centralized pressure these services may exert, it needs to be translated into political action through the congressional committee system which continues to favour a more fragmented approach to science and technology.

Under such circumstances, administrators of scientific research programmes and their professional constituencies find it imperative to maintain the support of key congressional leaders whose political power is based in a multiplicity of strong committee positions. The balance of loyalty between the President and Congress is often precarious. Administrators are beholden to the President who has appointed them and who can remove them from office or cut them off from the sources of his own political power. But they cannot alienate influential members of the congressional committees which monitor their programmes and budgets. Their ability to manoeuvre between these two foci of power on which they depend for authority and resources, is enhanced by less, rather than more, centralization in both branches of government and, most especially, in the executive. The decentralized pattern that exists permits administrators to conform to presidential policies, but still offers them margins of flexibility to meet congressional pressures without being caught in a fierce confrontation between the President and Congress.

Decentralization, in many respects, helps make the American constitutional system work. By the same token, it is the constitutional separation of powers between the President and Congress that enhances bureaucratic decentralization in the first place. The cost, at least in the case of science and technology, would appear to be a difficulty, if not the impossibility, in developing coherent, long-range national policies. But this is a real cost only if a national science policy is thought to be a value in itself. There has, over the years, been a consistent call for a national science policy, but, if one looks carefully, the call has always been fairly muted. Proposals for a central Department for Science and Technology, for example, came up year after year but never gained even minimal support in government or in the scientific community. And today, the scientists who criticized the Nixon decision to abolish PSAC-OST, seem quite satisfied with the response of President Ford though his proposed OSTP is more modest than the Council for Science and Technology that some of them had proposed.[8]

There is, of course, a difference between a national science and technology policy that is planned and coherent and a centralized structure for policy-making for science and technology. There is, in American attitudes, no less than in the American constitutional system, a long and deep distrust of centralized structures. At the same time, the complexities of contemporary social and economic life have created pressures for coherent national policies and an expansion of presidential power as a source of their development. The result has been a continuing tension between the quest for coherent policies, in all fields of public concern, and the decentralized system through which policies evolve.

Science policy disaggregated

Satisfactory definitions of science policy are hard to come by. Some years ago, a convenient formula developed for dealing with science policy in two ways: first, as 'policy for science', that is, policy for the support of fundamental scientific research; and second, as 'science in policy', that is, the use of research findings in the formulation of national policies and the performance of agency programmes. This distinction is a step in disaggregating science policy, but only a first step. More recently, there have been other useful efforts in going further to disassemble the parts of science policy.

During the past year, for example, the American Association for the Advancement of Science (AAAS) prepared a 'white paper' in response to the abolition of the White House science office. In describing the broad field of 'science policy', the AAAS paper made explicit a management function and elaborated on the 'policy for science' and 'science in policy' functions in terms of 'short-term' and 'long-term' considerations. But the AAAS statement went on to discuss how different functions are carried out at different levels of government, providing another dimension to science policy, in effect.[9] The AAAS, as had other scientific groups, advocated that a Council of Science and Technology Advisers be established in the Executive Office of the President. (But the AAAS acknowledged that a 'single presidential appointee was a viable alternative' where a President might find a council unwieldly and slow-moving, and would prefer a simpler arrangement. What the AAAS was saying was that the specific structure for science advice in the Executive Office was less important than effective working relations with the President.)

Under the AAAS proposals, the Council (or single adviser) would be principally concerned with the long-term aspects of 'policy for science' and a broad integrative level of 'science in policy' in co-operation with presidential planning agencies, like the National Security Council and the Domestic Council. The management function, dealing with the effectiveness and co-ordination of research and development programmes, would be carried out through a separate unit working with, or under, the Office of Management and Budget. Finally, the AAAS identified an 'advocacy' role to argue the case for continuing support for basic science; this level of 'policy for science' would be focused in the National Science Foundation, and other science agencies. Taken together, the AAAS proposals (whatever the disposition of the precise recommendations) offer a perspective on science policy that breaks the field down into a series of separate roles—advisory, management and advocacy—that are performed at different levels of government and with both short-term and long-term considerations.

The AAAS paper looks at science policy as a pluralistic enterprise that is, itself, diffused. There is also a tendency to limit the autonomy of science policy, if not to question whether science policy really exists as a separate field of public policy. In this context, science and technology are largely construed as means to other ends, ends that are subsumed in social, economic, defence or foreign policies. This way of thinking leads to an integration, on the one hand, of policies for science and technology and, on the other, socio-economic policies, with science and technology policy the dependent, rather than the independent variable. If there is a place for an autonomous science policy, it is at the far end of the spectrum of scientific and technological activities where support for fundamental research is involved. But even here, science policy is not isolated from educational or other social interests. In all other matters, questions on science and technology need to be taken up as part of broader policy issues in which the scientific and/or technological components might carry different degrees of importance.[10]

Pressures toward decentralization in the American political system tend, in general, to invite limits on the autonomy of science policy. There was, however, a brief period, in the late 1950s and early 1960s, when quite independent policies for science evolved. Spurred on by the space race with the Soviet Union, influential scientists argued successfully that the capacity of the United States to meet the challenges of a science-based world lay in a vast expansion of the educational and research structures of the country. Attention was on the physical sciences and engineering, though not exclusively. The incentives that came from the space race were reinforced by breakthroughs in health care (like the polio vaccine) which gave impetus to a buildup of support for the biomedical sciences, as well.

This was the lush period for science and technology in the United States. It was an important period in providing a vital injection of funds and manpower. In some instances, it encouraged universities and research institutions to expand beyond their legitimate capacities and resulted, by the early 1970s, in a surplus of trained personnel in some fields and a strain in sustaining research facilities. But, over-all, I would argue that this massive government programme guaranteed the quality and scope of American science for some time to come. Science had emerged from the Second World War with considerable prestige and promise of support. But government science and technology programmes had lagged through the late 1940s and early 1950s except for those narrowly related to national security requirements. This space-generated effort was not only considerably larger in scope, but it also incorporated an important commitment to basic research that persists though it is frequently vulnerable in the face of short-term political pressures.

There is no doubt that the American political system, with its decentralizing forces, encourages a short-term, problem-oriented approach to research planning. But the relative weight given to science and technology in different policy decisions and the importance attached to basic research cannot be reduced to a simple short-term/long-term dichotomy. They are the result of a more complex interaction of several variables.[11] The first relates to the field of science applicable to the issue area: the state of knowledge, the degree of professional consensus on paradigms, and the power of proven methodologies, for example. Of these several conditions, the degree of professional consensus has particular significance in the policy process. Technical disagreement and arguments among scientists in the same field are certain to bring doubts to the minds of politicians over whether there really is an objective basis in facts for their political decisions.

The effect of professional disagreement leads to a second set of variables that is closely related to the state of knowledge. This is the policy-maker's perception of the field of science, for it is his perception which will determine, in many respects, his receptivity or resistance to expert advice. Speaking very broadly, there is, in the United States, a general view that the physical sciences are the most advanced and the most reliable of the sciences, the biological sciences less so, and the social sciences even less. Such generalizations do not take into account the range of reliability from physics to meteorology and the basic elements of life that still elude the biological sciences. But that is not really the point. The point is that, all other things being equal, there will be greater questioning and scepticism by policy-makers, of social science advice, for example, than of advice from biologists, zoologists or chemists.

But this is only a first generalization; the problem is more complex. The vocabulary of the social sciences is more familiar to policy-makers and this can lead either to greater questioning and doubt, or, under some circumstances, to greater confidence in the expert advice because it is understandable. Few policy-makers, on the other hand, can follow a highly technical discussion in the physical or biological sciences. They are then in a position of having to act on a kind of faith that they may not have or are not willing to acknowledge, especially if there is disagreement among specialists. They may discount scientific advice because they cannot understand it.

Two further sets of variables in measuring the importance of scientific advice to policy-making relate to the issues of policy rather than the state of knowledge involved. The first deals with the nature of the issue, whether, for example, it is one in which the scientific or technical aspects can, at least at first examination, be isolated, or whether they clearly and immediately affect, or are affected by, social and economic factors. In the first instance, the technical factors can be overriding; in the second, they may be no less important, but their obvious links with socio-economic factors can create uncertainties and provoke the attention of vested political interests. Finally, there is the political salience of the issue. How critical is it that a firm decision be made? Are there pressures of time? How much is public opinion roused by the issue? How does it affect the political fortunes of the policy-makers? Is it a matter that touches on important partisan differences?

These four sets of variables—the state of knowledge, the perception of expertise, the nature of the policy issue, and its political salience—combine to determine the role of science and technology in matters involving 'science in policy'. But they are also applicable to 'policy for science'. Even though the federal government has accepted a primary commitment to support basic research in all the sciences, the level of support and the distribution of resources among the different disciplines and fields are subject to continuing debate and controversy. The importance that policy-makers will give to special fields and thus the level at which they will be funded, will depend on these same variables: the state of the disciplines; the credence that policy-makers give to research needs as they are presented by scientists; the broad public purposes that appear to be served; and the political importance of these purposes at a given moment.

When American science policy is thus 'disaggregated' and related to the realities of American politics, three conclusions emerge. The first has already been suggested: the impulse toward a short-range view of research planning that can be detrimental to the long-range needs of basic research and obscure the long-range implications of science and technology in matters of social, economic or foreign policy. A second conclusion is that both the American political system and the multifaceted nature of science policy, bar highly centralized solutions to the problem of long-range planning. Finally, there are multiple points in the system—and not just one central focus—at which the case for a long-range perspective can be argued: within the presidency; in the bureaucracies; and in congressional committees. There is no reliance on a single person or a single group. Defeat within one arena does not mean the end of the game. The case can be taken up in other arenas in relation to different issues.

There are certainly key arenas and the presidency is the most significant. The political resources of the President are powerful and his view the most comprehensive and far-reaching. Reinstituting a science presence in the White House was thus of prime importance. But even here, the bureaucracies and congressional committees have interests of their own that need to be furthered with or without presidential leadership. They provide not only alternative sources of support in the absence of presidential leadership, but also alternatives to undue presidential domination of political power. Matters of science policy, like other matters of important social decision, are enmeshed in basic questions of democratic government.

Notes

---

[1] Robert E. Lane, 'The Decline of Politics and Ideology in a Knowledgeable Society', *American Sociological Review*, Vol. 31, No. 5, October 1966, p. 658.

[2] 'New White House Science Office: Small in All Ways', *Science and Government Report*, Vol. V, No. 12, 15 June 1975, p. 1.

[3] For a list of the public reports of PSAC, see David L. Beckler, 'Science in the White House', *Daedalus*, Summer 1974, p. 132–4.

[4] ibid., p. 127.

[5] ibid., p. 124.

[6] Quoted in 'Science Advice in the White House', *Technology Review*, January 1974, p. 14–15.

[7] For a full and excellent discussion of the congressional role, see Sanford Lakoff, 'Congress and National Science Policy', *Political Science Quarterly*, Vol. 89, No. 3, Fall 1974, p. 589–611.

[8] See *Science and Technology in Presidential Policy-making*, Report of the *ad hoc* Committee on Science and Technology, National Academy of Sciences, Washington, D.C., June 1974.

[9] 'Organization for Science and Technology in the Executive Branch', *Science*, Vol. 187, 7 March 1975, p. 810–14.

[10] For very useful discussion of the integration of science policy with other policy decisions, see *Science, Growth and Society*, Report of the Secretary-General's *ad hoc* Group on New Concepts of Science Policy, Organization for Economic Co-operation and Development, Paris, 1971.

[11] The following discussion is adapted from a method of analysis for examining the influence of experts on policy-making, developed by Robert S. Friedman in *Professionalism: Expertise and Policy Making*, New York, General Learning Press, 1971.

Congress and National Science Policy

SANFORD A. LAKOFF

The recent efforts by Congress to reassert some of its constitutional prerogatives, which began with the reaction against involvement in Vietnam and continues in the wake of the Watergate affair, have made the role of the legislative branch a more lively issue than it has been at perhaps any other time in this century. The degree of legislative involvement affects many areas of government policy, including the complex of issues and activities that together make up "science policy." The role of Congress in setting and maintaining science policy has never been very well defined or delimited. Congressional influence has often been felt in specific cases, such as the development of atomic energy, the Apollo project, and controversial and expensive weapons systems. In general, however, the pattern and direction of federally supported research and development has been set in the executive branch and embodied in the budget presented annually to Congress.

As the deployment of scientific resources becomes a critical instrument of government in virtually every area of public concern, science policy has come to be regarded as one of the vital levers of social action. For this reason, it is entirely possible that Congress may take advantage of the present climate of political uncertainty to try to assert more authority in this area of public policy than it has had either the will or the opportunity to do since the end of World War II.

SANFORD A. LAKOFF, currently at the University of Toronto, will shortly become chairman of a new department of political science at the University of California, San Diego. He is the author (with J. S. Dupré) of *Science and the Nation: Policy and Politics* and editor of *Knowledge and Power: Essays on Science and Government*.

Political Science Quarterly Volume 89 Number 3 Fall 1974

Opportunities for Increased Congressional Influence

In at least two respects, conditions for improvement are propitious. First, the executive apparatus for setting science policy, laboriously built up since the sputnik crisis of 1957, has been dislodged and dismembered, though not destroyed. In the aftermath of that crisis, President Eisenhower made two moves aimed at improving the links between the White House and the nation's scientists. At the suggestion of James R. Killian, Jr., he created the post of special assistant to the president for science and technology. Along with it, he created the President's Science Advisory Committee (PSAC), to which he appointed leading nongovernment scientists, mainly from the universities. In time, a third element was put in place when the staff which served both the special assistant and PSAC was converted by a reorganization plan into the Office of Science and Technology (OST). In 1973, however, President Nixon decided to abolish both PSAC and OST and to replace the special assistant with a science adviser located in the National Science Foundation (NSF) rather than in the White House. In place of OST, a new Science and Technology Policy Office (STPO) has been created within the NSF.

The relocation of the science-policy apparatus not only deprives its directors of a relatively exalted place in the executive hierarchy but also of whatever small degree their predecessors could claim of that precious quality known in Washington as political "clout." In theory, the special assistant and PSAC had direct access to the president, and in practice the staff offices of OST were located in the Executive Office Building along with the president's other advisers and the Office of Management and Budget (OMB). The new STPO reports only indirectly to the president, is housed with the other offices of the NSF, and must attempt to influence the activities of other agencies of government from a notch in the bureaucratic hierarchy considerably lower than that occupied by OST.

The termination of PSAC cuts even the tenuous connection to the presidency previously enjoyed by the leaders of the scientific community. For the past several years, moreover, many of these same leaders have found Congress more receptive than the executive on such matters as arms control and the ABM issue. In a limited but significant sense, therefore, a power vacuum has been created in science policy. It is conceivable that Congress may now seek to enter that vacuum, as part of a larger effort to challenge the expansion of executive power and to recover some of the ground the legislative branch had lost.

The second reason for anticipating an increase of congressional concern with science policy has to do with the well-known sensitivity of members of the legislative branch to shifts in public opinion. A few years ago, when there was considerable popular frustration over some of the uses to which science had been put, as well as the unforeseen social and environmental problems brought about by the introduction of new technology, congressmen were apt to be sympathetic to the charge that science was more a part of the problem than of the solution. Now, however, with the energy shortage causing hardships, and with the association between research and the so-called military-industrial complex somewhat attenuated, there is reason to think that congressmen and their constituents will once again seek counsel and practical help from scientists and engineers.

Several signs clearly indicate serious congressional interest in asserting more control over science policy. In both houses, proposals have appeared calling for the declaration of a statutory national science policy, coupled with appropriate administrative machinery. One of these proposals, Senate Bill 32, passed the Senate by a margin of 70 to 8 late in 1972, but failed to reach the floor of the House. Nevertheless, this particular bill may have served its purpose—to warn the administration to do something about the deteriorating state of nonmilitary research or else Congress would take the initiative. In its proposed 1975 budget, the administration recommended an increase in appropriations for civil research of 20 percent over the previous year's expenditures. This will probably be enough to appease the present Congress, but it is not out of the question, if the November elections should produce a harvest of new Democratic seats, that S. 32 will be revived and passed, given the proclivity of liberal Democrats for declared commitments and positive action by the federal government.

Congress has already taken one potentially important step, which in itself could well be considered positive action, in creating a congressional Office of Technology Assessment (OTA). Technology assessment, in the words of the first director of the new agency, former Congressman Emilio Q. Daddario, is supposed to provide an "early warning system to protect man against his own inventions." The new office was established after many congressmen had come to feel a need for expert guidance in considering such new departures as the proposal to subsidize development of a supersonic civilian airliner. OTA presumably would identify likely new technological opportunities and investigate them to determine both benefits and costs, including the external costs that usually are neglected by their promoters but in the end must be borne by the public.[1]

Other developments may also affect the ability of Congress to pronounce on matters of science policy. These include the reorganization proposals which have now been tabled by a Select Committee on Committees in the House and the recently adopted Budget Reform Act of 1974 designed to enhance congressional influence over the budgetary process, even to the extent of creating a new legislative office to serve as a counterpart of the executive's budget agency.

---

[1] Anne H. Cahn and Joel Primack, "Technological Foresight for Congress," *Technology Review* (March/April 1973), 41.

Are these developments likely to coalesce to the extent that they will enable Congress to define and develop a lasting role in science policy? To answer this question we need to consider what role Congress has actually played in relation to science policy and then to consider whether, in view of the formidable obstacles to the reversal of the recent tendency toward the accretion of executive power, these new opportunities are likely to prove meaningful or largely illusory.

Congress' Past Record

Since the turn of the century, when the United States first began to assume a role as a world power, the presidency has become the focal point of popular demands for action in one area after another. To be able to respond to these demands, the president had to be provided with the capacity to control his own expanding administration. Minimally, this required that the president be in a position to set the legislative agenda, submit a budget reflecting his view of national priorities, and use his appointive power to assure compliance with his policies. Congress became the instrument, however reluctantly, through which much of the president's control was established and maintained. On the whole, the activities of Congress with respect to science policy serve to illustrate this general tendency.

Indeed, an unsympathetic critic reviewing the performance of Congress in connection with science policy might well decide that in this area as in others, Congress has been suffering from a kind of constitutional amnesia. In fairness, it should be noted that at least one disinterested appraisal does not reach such a negative conclusion. In its 1968 review of the American effort to cope with the challenges posed by advances in science and technology, the Organization for Economic Cooperation and Development (OECD) found reason to praise the achievements of the legislative branch. Congress, the OECD report declared, "has gone further than any other Parliament in seeking to respond to these challenges by equipping itself with a new set of bodies, most of which have brilliantly performed the role assigned to them by taking advantage of the instruments made available to them by Congressional institutions." In particular, the report singled out the accomplishments of the Joint Congressional Committee on Atomic Energy, citing an American study rating it as "probably the most powerful Congressional Committee in the history of the nation."[2]

In 1966, when the OECD examiners made their inquiry, the American way of managing and supporting science was still considered the best in the world. The "American model" had become a standard for worldwide emulation. Because the United States was devoting 3 percent of its gross national product to research and development, this became a target figure everywhere else. The high ratio of support allotted to development in the United States was thought to be responsible for America's ever-increasing lead in high-technology industry, and more often than not, the fact that the bulk of this expenditure had only indirect and sometimes distorting effects on the civilian sector of the economy went unnoticed.[3] The key to America's success was thought to be its political, economic, and institutional pluralism, which encouraged a wholesome competition in scientific creativity and technical inventiveness, as opposed to the stultification that plagued more highly centralized systems. The American legislature, standing free of the executive, and organized internally so as to allow for a decentralized examination of the particular aspects of science policy, seemed a perfect illustration of this dynamic pluralism at work.

The role of Congress must have seemed all the more impressive when contrasted to the role of parliaments in systems of government where the separation of powers is not the constitutional norm. Set alongside the inherently limited role open to parliamentary bodies in which the center of gravity is anchored firmly in the cabinet and the leadership of the party in power, the performance by Congress of even a modest proportion of its potential constitutional authority was bound to seem relatively significant.

The OECD appraisal points up one of the fundamental dilemmas in any assessment of the role of Congress in general, and with respect to science policy in particular. Congress actually is assigned two different and sometimes contradictory roles in the American scheme of government. One of these is to share in the setting of public policy, a function which it performs by exercising its power to legislate and appropriate. The other is to check the executive by seeing that the president and the agencies for which he is responsible follow the legislative mandate of Congress and do not administer the laws in an arbitrary, partial, or corrupt way. Congress does this by invoking its authority to interrogate administrators and to investigate allegations of wrongdoing and partiality. To appraise the performance of Congress in terms of its role in the American system, rather than in comparison with the role played by other legislative bodies operating according to a completely different constitutional principle, it is necessary to ask how well both of these functions actually are performed.

---

[2] OECD, *Reviews of National Science Policy: United States* (Paris, 1968), pp. 86–87. The comment on the JCCAE is from Harold P. Green and Alan Rosenthal, *Government of the Atom* (New York, 1963), p. 226.

[3] For a fuller discussion of the impact of the American model, see Sanford A. Lakoff, "The Vicissitudes of American Science Policy at Home and Abroad," *Minerva*, XI, no. 2, (April 1973), 175–190.

The case of the Joint Congressional Committee on Atomic Energy is a good example of how an overemphasis of one function can lead to an atrophy of the other, but the problem it illustrates is widespread. It is a common tendency for congressional committees to develop close relationships with the executive agencies they are supposed to oversee. The Joint Committee is so closely involved in the day-to-day management of the Atomic Energy Commission that Congressman Melvin Price, the present committee chairman, once described it as a kind of board of directors for the AEC.[4] Even in the early years of the committee's existence, Senator Henry M. Jackson went still further, observing that in the field of atomic energy, "the Committee made the decisions, with the advice and consent of the executive branch."[5]

Behind the pluralistic façade, in other words, lies the reality of an arrangement whereby members of the legislature have been virtually coopted into the executive. This arrangement has enabled the committee to exert very strong, even determining influence in the development of atomic weapons and atomic power, and it has also made it a target of criticism from opponents of AEC policies who point out that the intimacy of the committee and the commission makes the former less the AEC's congressional watchdog than its "Siamese twin," as Ralph Nader has called it.[6] The result, the critics contend, is that instead of reviewing and monitoring the AEC's behavior, the committee is always ready to defend it from attack, whether against those who find its standards for radiation safety inadequate or those who question its encouragement of duopoly in the industrial development of atomic power.[7]

[4] Subcommittee on Science, Research, and Development, House Committee on Science and Astronautics, 91st Congress, second session, *Toward a Science Policy for the United States* (Washington, D.C., 1970), p. 109 n.

[5] Henry M. Jackson, "Congress and the Atom," *Annals of the American Academy of Political and Social Science* (November 1953), 76. Cited in Harold Orlans, *Contracting for Atoms* (Washington, D.C., 1967), p. 161.

[6] Testimony before the Joint Congressional Committee on Atomic Energy, Washington, D.C., January 28, 1974.

[7] For such criticism see H. Peter Metzger, *The Atomic Establishment* (New York, 1972); and Richard S. Lewis, *The Nuclear Power Rebellion* (New York, 1972).

Critics of the way Congress works have often complained that such relationships vitiate the intention behind the separation of powers. The committees acquire an overly protective interest in the agencies and so long as the administrators take care to mend their fences on Capitol Hill, they can expect to be defended against any drastic budget cuts the president may seek to impose and against reorganization efforts which could weaken or eliminate the agency. The more narrow the jurisdiction of the committee, or the more its role is to supervise a particularly important agency, the more threatening such assaults are likely to be viewed by the members of the committee, for they come to feel that their own status and power are tied to the fortunes of the agencies they supervise. In the postwar struggle over unification of the military services, for example, the House and Senate Naval Affairs Committees were as protective of the organizational position of the navy in the proposed unified Defense Department structure as were the admirals and the Navy Department civilian officials such as Secretary James Forrestal.[8] On the other hand, it is easier for a committee to influence policy if it develops the kind of specialized expertise the members of the Joint Committee have acquired because of their unique access to the AEC.

Nor is this the only difficulty the committee system poses for the ability of Congress as a whole to fulfill its two major functions. Another problem is that the very diffusion of responsibilities among the committees and their powerful chairmen, which appears to have so impressed the OECD examiners, hinders Congress from performing a role as a corporate body, whether as partner or critic of the executive. The fragmentation of responsibility among committees and subcommittees results in such a wide distribution of the law-making and overseeing functions that the strings cannot be drawn together very easily.

With respect to science policy, the result is that Congress has taken no position whatever, either constructive or critical, except insofar as it is called upon to endorse the work of the committees. Since concern for science and technology is parceled out to the committees—and is in most cases only an incidental concern—there is no practical way for Congress as a body to make comparative judgments or to become involved with imbalances. Instead, some projects flourish while others languish, and in general the fate of particular projects requiring large-scale support is determined not in some comprehensive assessment but in accordance with the specific policy concerns of the committees and of powerful individual members of Congress. The fact that the United States had not even begun to pursue a broad program of research on energy until it was seized by an "energy crisis" is due not simply to a general lack of foresight in government; it also is due partially to the fact that the only committee assigned a primary concern for energy is the one devoted to a particular kind of energy, the Joint Congressional Committee on Atomic Energy. Nor is it wholly in response to the "felt needs" of the electorate that Congress should authorize billions for a space shuttle but only a pittance for mass transit. There is a Committee on Science and Astronautics in the House and on Aeronautical and Space Sciences in the Senate, but none with a comprehensive responsibility for all transportation technologies.

---

[8] See Demetrios Caraley, *The Politics of Military Unification* (New York, 1966), especially chap. 7.

Congressional Initiatives: The Problem of Particularism

It would not be correct to conclude that Congress has taken no initiatives in science policy; however, the initiatives that have been taken usually have been in isolated cases rather than in some broad comparative context. Certain highlights stand out in the record. In the immediate postwar period, Congress did not simply put into law the proposals for the control of atomic energy advanced by the president. Under the leadership of Senator Brian McMahon, Congress made its own inquiry, listened to the views of the scientists who had been associated with the Manhattan Project and wrote its independent conclusions into the Atomic Energy Act. Years of debate and deadlock preceded the creation in 1950 of the National Science Foundation, which also bore the stamp of congressional scrutiny and compromise, not to mention congressional hostility and suspicion. During the cold war paranoia of the 1950s, Congress may not have exercised a particularly benevolent role with respect to science and scientists, but it certainly made its views felt on the subjects of loyalty and security in government-sponsored research, ignoring the scientists' complaint that an obsession with negative security was counterproductive because it undermined the positive security that could come only from the encouragement of researchers. But the same Congresses which demoralized and alienated some scientists encouraged and supported others, notably by endorsing Edward Teller's crash program to develop the hydrogen bomb and Admiral Hyman Rickover's preference for nuclear-powered submarines over conventional naval vessels.

The record is replete with other examples of specific initiatives. In certain areas of health research, especially in connection with diseases likely to affect people of their own age, congressmen have been known to vote more financial support than the agencies have requested. During the "sputnik crisis," it was Congress, under the leadership of then Senator Lyndon Johnson, which took the initiative in creating the space program at a time when the administration was more anxious to balance the budget than to compete with the Russians in a contest that one official derided as a "celestial basketball game." The Space Act, as George Reedy has observed, "was one of the few examples in the last forty years of a major statute originating on the Hill rather than in the White House."[9] The role of a bipartisan group of mid-western congressmen in overcoming President Johnson's reluctance to approve plans for the 200-BEV accelerator, now located in Illinois, is well documented.[10] More recently, Congress has played an important role in the debate over the antiballistic-missile program and in the rejection of the proposal to subsidize development of the supersonic airliner. In adopting the Mansfield Amendment to the Military Procurement Act of 1970 (which prohibits the military services from supporting research unless it is directly relevant to a "service mission"), Congress responded to the objections of academic critics to the channeling of funds for basic research through the defense agencies. Typically, however, Congress did not at the same time direct that these funds be reassigned to the NSF and other nonmilitary agencies. The result was that in answering one need it created another.

Compensating for Structural Weaknesses

No one reviewing this record would be justified in concluding that Congress has been entirely subservient to the executive in matters involving science and technology. Clearly, the weakness in congressional efforts has been the failure to develop and maintain a continuous, comprehensive, and internally consistent approach to the variety of elements that make up science policy. Because the Congress is a far less unified branch then the executive, which is itself fragmented to no small extent, it cannot easily develop such a comprehensive perspective. The difficulties that confront a president in developing and imposing a single set of national policies are easier to overcome than those facing Congress. The president can formulate his objectives and take steps to assure that they are observed by the agencies his appointees direct. With the considerable help of the Office of Management and Budget, he can expect to maintain a reasonable degree of control and integration, especially by the level of funding he assigns to different programs in his annual budget. Since he is usually in office for a fixed term, and possibly for as long as two terms, the president can set relatively long-range objectives. Because he and his staff have direct access to information from the agencies, they need not be handicapped by ignorance of the real conditions as the agencies perceive them.

---

[9] George Reedy, *The Twilight of the Presidency* (New York, 1970), p. 54.

[10] See Daniel S. Greenberg, *The Politics of Pure Research* (New York, 1967), pp. 207–265; and Allan Rosenbaum, "Decisionmaking, Expertise, and the Processes of Federal Policy Formation: The Atomic Energy Commission, Congress and the National Accelerator Laboratory," forthcoming in Sage Professional Papers in Policy and Administration

Congress, on the other hand, has no effective way of integrating the work of its committees. The legislation and appropriations arrived at during the legislative session are inevitably the work of many hands. In effect, the results seal the bargains among interests, regions, and powerful legislators. Unlike the policies set down by the executive, they do not reflect a single point of view or a single source of responsibility. To be sure, interest groups often exert pressure on the executive as well, and there is a certain amount of "horse trading" among the agencies, but at least the presidency provides a single focus of authority—from which ultimate decisions can emanate, and to which appeals can be carried. There is no such single focus in either house of Congress. Budgetary responsibility is divided among the appropriations committees and the authorization committees, and is split off from the legislative authority of the substantive committees. Although powerful appropriations committees in each house must assemble the budget, neither of them can give it the detailed and informed scrutiny the OMB imposes in reviewing agency submissions.

The only committee with a general responsibility for examining expenditures is the Joint Committee on the Economic Report, but it is not authorized to exercise any controlling influence over the powerful appropriations committees in both houses. In contrast to the ability of the executive to conceive of long-term program objectives, Congress is virtually a prisoner of the annual budget cycle. This is particularly unfortunate in the case of research projects, which usually require long-term commitments and are sensitive to sharp fluctuations in funding. As Philip Handler, president of the National Academy of Sciences, has rightly pointed out, "There is no relation between the useful life of a laboratory or scientific project and the time required for the Earth to complete one orbit around the sun."[11] Far from having the easy access of the executive to information, Congress has to pry information loose whenever the agencies suspect it may be used against them. When the president cloaks the activities of his staff with the mantle of executive privilege, inquisitive congressmen must rely on informal briefings or leaks that may or may not yield genuine information.

There are also other reasons why it is virtually impossible for congressmen to devote themselves to the determination of any broad national interest in the fullest and most objective sense. Congressmen are bound

[11] Quoted in *Toward a Science Policy for the United States*, p. 37.

to be especially sensitive to the interests and needs of their particular constituencies. Not only does this color their perception of the public welfare, but it also forces them to spend a good deal of their time providing services to constituents.

No group is more aware of all these difficulties than congressmen themselves. Some seem to have concluded that the best way they can influence the framing of public policy is not so much by making policy as by improving the decision-making system in the executive branch. This tendency was expressed with rare clarity as it pertains to science policy in an exchange between Congressman Ken Hechler and the last director of OST, Edward David, Jr., during a hearing to consider the president's reorganization plan abolishing the office. Dr. David warned that governments are inevitably tempted to concern themselves with short-run returns rather than with the long-term strength of scientific and educational institutions. Congress, he added, ought to be apprehensive about what might happen to these long-term considerations now that the functions of OST were to be transferred to the STPO in the NSF. Congress, and in particular the committee before which he was testifying, might well wish to monitor the activities of the new office, he said, to make sure that these interests were well served. Congressman Hechler responded:

> You are very charitable in using the word "monitor." We have the power of suggesting, occasionally, and we pass legislation, which results frequently in impoundments when there's a philosophic difference with what we do. I think you're attributing a great deal of power to this committee when you say we are to monitor this situation. *What the committee wants to do is to try to see that the structure is sound in the first place so that we don't have to spend all our time monitoring it.*[12]

The idea that the best way for Congress to influence the executive is by concerning itself with organization has the endorsement of at least one leading student of Congress. James A. Robinson even urges that Congress "concentrate on finding organizational devices to encourage the Executive to plan ahead in such a way as to represent the kind of values Congress favors."[13]

Critics of the "imperial presidency" may consider this advice a counsel of abdication, but they might bear in mind that Congress has found it difficult thus far to do even this much. In practice, Congress has deferred to the wishes of the president at every stage in the organization and reorganization of the administrative structure for science policy. By presidential decision, the post of Special Assistant to the President for Science and Technology was established within the executive branch in 1957, followed shortly afterward by the appointment of the President's Science Advisory Committee (PSAC). Congress had no direct role in either decision. In 1962, in what was described at the time as a move designed to give more permanent status to the role of the science adviser and his staff, the Office of Science and Technology was established within the executive branch. Although this was done at least partly in deference to the congressional desire to be able to summon the science adviser to testify, it was accomplished by a presidential reorganization plan. In 1973, in accordance with the principle that what the president can create he can also destroy, the office was abolished by another reorganization plan. The use of this device is another of the techniques developed by presidents to bypass Congress, or at least to reverse the ordinary legislative process. In using this procedure, as Michael Reagan has pointed out, the president "does not just *propose* a statutory change in administrative organization, but he *declares* that the change and his decision become law without further legislative approval. Only if Congress (currently, a majority of either house of Congress) disapproves and vetoes the President's action does it fail to become law. Thus the burden of proof is shifted. . . ."[14]

---

[12] House Committee on Science and Astronautics, Hearings, *Federal Policy, Plans, and Organization for Science and Technology* (Washington, D.C., 1973), p. 143. Emphasis added.

[13] James A Robinson, "Decision Making in Congress," in Alfred de Grazia (ed.), *Congress: The First Branch* (Washington, D.C., 1966), p. 292.

Venturesome committees which have shown a serious interest in fundamental issues of science policy have not been taken altogether seriously by their congressional colleagues, as is illustrated by the fate of the Subcommittee on Government Research of the Senate Committee on Government Organization. Chaired by Senator Fred Harris, this subcommittee was appointed in 1966 and spent a busy three years reviewing and analyzing some of the most important questions affecting the government's programs in science and technology. Valuable reports were produced on a number of issues, including the payment of indirect costs in sponsored research, the usefulness of social indicators, and the future of biomedical research. The subcommittee deliberately "chose to serve as a forum of debate and to encourage the discussion of high level policy issues with profound moral and social implications. . . . The main intention was to discuss the long-term implications of scientific research with those who came before the Committee." In 1969, this subcommittee was disbanded, ostensibly as a result of a decision to economize, but also because the

---

[14] Michael D. Reagan, "Toward Improving Presidential Level Planning," reprinted in Thomas E. Cronin and Sanford D. Greenberg (eds.), *The Presidential Advisory System* (New York, 1969), p. 267.

Senate had lost interest in science policy,[15] and because Senator Harris had become unpopular with the senatorial establishment. Alternatively, we may explain its demise as a result of the inability or unwillingness of either its parent committee, or Congress as a whole, to make use of its findings.

Another case in point is provided by the experience of the Subcommittee on Science, Research, and Development of the House Science and Astronautics Committee. Under the leadership of Representative Daddario, this equally active subcommittee produced a series of major proposals in 1971.[16] The subcommittee recommended that Congress formally enact a National Science Policy, to be drafted by a special task force, as a matter of statutory law. In addition, the subcommittee proposed that OST be upgraded so that it would no longer serve merely as the staff for the science adviser, PSAC, and the Federal Council for Science and Technology (FCST). Instead it was to be given the responsibility for formulating science policy, under the broad terms of the new statute, and of monitoring the activities of the federal agencies in all appropriate areas. It was also to prepare an annual report on the scientific state of the nation. In the same report, the subcommittee endorsed the proposal, which had been approved after much detailed consideration, for a National Institute for Research and Advanced Studies (NIRAS) to encompass the NSF. None of these proposals was acted upon, in part because Congressman Daddario vacated his seat in the House, but also because the committee's recommendations do not carry great weight compared to proposals from other more powerful committees.

Significantly, the one recommendation which has come to fruition is the proposal for a congressional Office of Technology Assessment. Congress, it would seem, cannot foist a scheme of executive organization on an unwilling president, but it can reorganize itself to improve its own ability to contend with executive policy proposals. The same motive led earlier to the establishment of the Science Policy Research Division in the Congressional Reference Service (CRS), which was created to answer the common complaint of congressmen and their staffs that they were unable to cope with many of the technical issues that came before them. Both the CRS and the General Accounting Office (GAO), the other external arm of the legislature, have been specifically requested to provide not only information but "policy analysis" as well. Congress has also called upon the help of the National Academies of Science and Engineering, a development which, if expanded, could vastly improve the congressional capacity to act with more confidence in areas requiring scientific expertise. The new Office of Technology Assessment will add another important resource. The preamble of the act establishing OTA stresses the need for Congress to obtain "timely and unbiased" information from a source "independent" of the executive agencies;[17] it is clear from the language that the intention behind the establishment of OTA is to reduce the need for congressmen to rely upon the administrators and industrial lobbyists, who are usually more anxious to stress the benefits than the costs of the innovations they are promoting, for information on new technologies. It remains to be seen whether the new agency will be permitted to tackle highly controversial issues or whether, as skeptics anticipate, it will be compelled, in order to survive, to perform studies which will not risk offending powerful interests or competing with the investigative jurisdictions already well staked out by congressional committees.

---

[15] Dennis W. Brezina, "Rise and Demise of the Senate Subcommittee on Government Research," *Federation Proceedings*, 29, no. 5 (September–October 1970), 1829.

[16] *Toward a National Science Policy*.

Internal reorganization may soon be taken still further in the House, where consideration will shortly be given to a set of reorganization proposals drafted by a Select Committee on Committees, chaired by Representative Richard Bolling.[18] The draft report contains four recommendations which could enhance the ability of the lower chamber to deal with issues of science policy. One of the proposals would transform the present Committee on Science and Astronautics into a Committee on Science and Technology, presumably to signify a reduced concern for the space program and an increased concern for the general state of science and technology, including, for the first time, the military applications, which have previously been the exclusive preserve of the Armed Services Committee. A second element is contained in the proposal to reorganize several other committees. Thus, the Interior Committee would become the Committee on Energy and Environment, the Committee on Public Works would become the Committee on Public Works and Transportation, the Committee on Interstate and Foreign Commerce would become the Committee on Commerce and Health. A third element is that each House committee would be required to create a subcommittee on "oversight and forward planning," the second function to enable the committee to undertake "on a continuing basis, futures research and forecasting. . . ."[19] The fourth relevant innovation is the proposal for a new

---

[17] Technology Assessment Act of 1972 (Public Law 92–484); see especially preamble, "Findings and Declaration of Purpose." Reprinted in Senate Committee on Rules and Administration, *Technology Assessment for the Congress* (Washington, D.C., 1972), p. 44.

[18] House Select Committee on Committees, *Committee Structure and Procedures of the House of Representatives*, Committee Print (Washington, D.C., 1973).

[19] Ibid., p. 7.

Budget Committee designed to serve the function of comprehensive policy planning which, as previously noted, has been the major weakness in the organization and operation of Congress as a legislature. This new committee would be expected to "develop criteria of choice among alternative uses of Federal funds in terms of collective policy preferences and priorities. . . ."[20] Specifically, the committee would be charged with reviewing the five-year projections that must be presented by the president, in accordance with a provision of the 1970 Legislative Reorganization Act, with every request for funds to create or expand a program. The Bolling Committee explicitly urges appointment of such a committee as a way of increasing the ability of Congress to share in the setting of general national policy by undertaking "periodic assessment of national goals, priorities, and future program costs."[21]

Future Prospects: The Campaign for a Statutory National Science Policy

Some of those keenly concerned with the federal government's science policies, having failed to persuade the current administration of the wisdom of declaring a national commitment to the support of science and technology, are turning to Congress in the hope that in its current mood of self-assertion, it will impose such a formal commitment upon the executive. This campaign has had an initial, if as yet incomplete, success with the passage by the Senate of the bill (S. 32) introduced by Senator Edward Kennedy, "The National Science Policy and Priorities Act of 1972." The Kennedy bill had three key provisions.[22] One of these would have declared a statutory national science policy providing that expenditures for nonmilitary research and development should grow at the same rate as the GNP and that they should reach a level of parity with expenditures for military research and development. The second would have greatly expanded the traditional emphasis of the NSF on the support of basic research, especially in the nonbiomedical areas, to make the foundation responsible for establishing priorities in nonmilitary research and development and for devising programs to assist industries in converting to high-priority activities. The third key provision calls for the creation of a "Civil Science Systems Administration" to be located in the NSF. This new agency would be charged with promoting the development of technology aimed at meeting public needs in such areas as transportation, health care, and sanitation.

---

[20] Ibid., p. 89.

[21] Ibid.

[22] In this and the ensuing discussion I have drawn on the valuable report by Claude E. Barfield, "Congress Moves to Reset Priorities in Federal Research and Development," *National Journal*, 4, no. 40 (September 30, 1972), 1524–1533.

The Kennedy bill was introduced in response to a belief fairly widespread within the scientific community that the nation's scientific capabilities had been so heavily committed to military programs and to space exploration that other vital public needs were being neglected. The bill evoked considerable support, largely because of concern over the acute unemployment among scientists and engineers caused by cutbacks in government contracting with the aerospace and other defense-related industries. An analysis prepared for the Kennedy subcommittee projected the creation of 200,000 new jobs through programs to be fostered by passage of the bill. (Whatever the exact figure, there can be no doubt that if the key provisions of the bill were to be adopted, there would indeed be a great expansion in nonmilitary research and development. Even in fiscal 1974, the budget for military research and development is estimated at $9.3 billion, whereas nonmilitary programs, excluding space, are estimated at $5.8 billion.)

Objections to the Kennedy bill were lodged by sixteen federal agencies whose representatives complained that NSF was being asked to take on responsibilities already assigned to them. The president's science adviser, Dr. David, argued that the setting of priorities should be done at a higher level than that of the NSF, and that the administration was already taking steps to give greater emphasis to civilian research and development. The director of the NSF, H. Guyford Stever, expressed misgivings over the broadening of the foundation's responsibilities, lest it be diverted from its primary role as the overseer and supporter of basic research.

Supporters of the bill responded that too little had been done by the administration to redirect scientific capabilities to civilian needs. They pointed out that the budget for research and development had declined from 2.6 percent of the total federal budget in 1963 to 1.6 percent in 1971. If the bill had been in force, the percentage decline would have been lower and funds saved because of cutbacks in the space budget would have been transferred to other types of research. The bill's major draftsman, Ellis Mottur, of Senator Kennedy's staff, defended the call for parity between military and nonmilitary research funding as a "symbol . . . of the necessity for reordering national priorities."[23]

The campaign for the declaration of a single coherent national science policy, to which the Kennedy bill was a response, has been led by many of the leading spokesmen for the scientific community and institutions of higher education. They began to urge this course even when research budgets were expanding, both because they knew that the day would come when the budgets would have to level off and perhaps even decline, and also because they thought it was a bad idea to allow priorities in science and technology to be determined in response to particular pressures or dramatic and popular needs. Now that there is more intense competition for smaller allocations, it is said to be even more imperative that the government adopt a declared and carefully constructed national science policy. A Washington magazine writer recently concluded an article on the unhappy relations between scientists and the federal government by summarizing what he had learned after interviewing a number of leading scientists: "Most of all, the federal government must for the first time in history frame an overall policy that eliminates crash-basis science, erratic funding, and submission to faddish enthusiasms, and that substitutes consistency, continuity, balance between research and application, and long-range planning relating science/technology to national needs and goals."[24]

---

[23] Ibid., p. 1529.

Because this campaign for a national science policy is now being directed almost exclusively at Congress, and has a reasonable chance of success, it is appropriate to ask whether this should be thought of as a logical next step in the extension of congressional authority over science policy. We have already noted that Congress is not well organized to make comparative and comprehensive judgments in this or other areas of policy. This remains a serious objection which would not be vitiated simply by the passage of a bill declaring such a policy to be in existence. The policy would still need to be put into effect by a fragmented and unequally powerful set of committees. Would the Senate Labor and Public Welfare Committee, the parent of the Kennedy subcommittee, prove willing and able to enforce the legislation it has sponsored? Would it have the power to secure the cooperation of the appropriations committees in both houses to undertake the rather massive shifts of funding the policy would require? And if an administration opposed in principle to such legislation chose not to apply it with utmost diligence, would Congress be ready to compel stricter compliance? It can be argued that in this case as in others to pass legislation which risks being treated as an empty gesture is no way to reassert congressional authority.

There are other objections to the very concept of a single coherent national science policy which Congress would do well to ponder. The belief that such a policy would provide an answer to the shortcomings of the present ad hoc approach can arouse excessive expectations which almost surely would not be fulfilled. It is not possible by declaring a fixed set of priorities to remove science and technology once and for all from the continuing struggle to determine these priorities that is after all the essence of democratic politics. Even those who have advocated such a policy do not generally harbor ecstatic visions of what it could accomplish. Most of these advocates do not couple their support of the principle with a preference for a highly centralized method for assigning research priorities and for managing the government's extremely diversified system for supporting and performing research. Very few have declared support for a cabinet-level Department of Science and Technology; even those who do support this proposal acknowledge that many important scientific programs are best left to be managed by the mission-oriented agencies they now serve. They are well aware that the present system, despite the criticisms it has received, enables scientists to appeal to a variety of agencies for support and to exercise a certain degree of control over the pattern of allocations by serving as advisers to the agencies and in some cases by supervising the distribution of grants through peer-review panels. A system which would centralize control over allocations and reduce the role that specialists in particular fields now play therefore holds no great appeal for the scientists, especially since a single agency would be highly vulnerable to "political accidents and personalities in the administration or the Congress."[25] Paradoxically, therefore, many of the same scientists who would prefer to see the declaration of a national science policy are not enthusiastic about the appointment of a single scientific czar or a single committee of national science policymakers. They seem to want to retain the pluralistic system at the same time as they decry the fragmentary, ad hoc character of the policy that results from the operation of this system.

---

[24] Vernon Pizer, "Who Unplugged America's Science Machine?" *Washingtonian* (February 1974), 108.

There is little if anything to be gained by belaboring the inconsistency in this tendency to argue for a national science policy but against the machinery to create and enforce it. A few of the advocates do hold with both the principle and the practice. The others might explain that it is not necessarily inconsistent to believe in the development of a single national science policy by means of a diversified system of decision making and administration. It is not so much the discrepancy between theory and practice which is a cause for concern as the validity of the theory taken in its own terms. Some of the difficulties with it have frequently been pointed out, if not always in testimony before congressional committees. Several years ago, the secretary-general of the OECD convened a group of leading scientific administrators for the purpose of reconsidering the meaning of science policy in the light of experience. Their report admits candidly that "some observers have even questioned whether it makes sense to conceive of a science policy as a distinct and separate area of policy."[26] Especially in recent years, they observe, it has become increasingly hard to divorce policy for science from public policy generally. For better or worse, public support of research and development is shaped by the public perception of social needs. Since science and technology have become instruments of public policy, science policy must be thought of not as an independent, but as a dependent, variable of other public policy.

---

[25] Harvey Brooks, "The Physical Sciences: Bellwether of Science Policy," in James A. Shannon (ed.), *Science and the Evolution of Public Policy* (New York, 1973), p. 129. The view cited is that of the dissenting minority on the National Science Board commenting on the recommendation for a National Institution for Research and Advanced Studies.

The OECD conferees do not concede that it is altogether meaningless to think of science policy as an autonomous issue. Science and technology, they contend, need to be considered "in their entirety as a set of means to other goals, having some coherence within itself across the functions that it serves."[27] This redefinition considerably narrows both the scope and the significance of the concept of science policy. If it is to be understood as a means rather than an end, there is no justification for assigning it a fixed level of support or for making the level proportional to the size of the GNP. The level might as well approach zero or rise considerably higher, depending upon social objectives. Nor does it make sense to set an ideal ratio for the division of funds into such categories as basic research, applied research, and development, or between military research and all the varieties of nonmilitary research. Ideally, "big science" projects in different fields would not be compared with each other but with other program elements in the sector of social need which they are designed to serve.

The resulting disaggregation of science policy into a set of instrumental sectoral policies rests on the recognition that science and technology are too pervasive and too differentiated to be treated as a single coherent entity. The result, as Herbert E. Carter, chairman of the National Science Board, has candidly admitted, is that a national science policy becomes "at best an umbrella that covers a wide variety of policies, all of which affect the course of scientific activities, but many of which are directed to quite different ends."[28]

In an effort to meet such objections, a distinction has often been drawn between two aspects of science policy, usually referred to as "policy for science" and "science in policy." The first refers to the effort to develop and maintain a set of capacities in science and technology; the second refers to the effort to make use of this capacity in achieving social objectives. The trouble with this distinction is that the two aspects are often so intimately related as to make it impossible to treat one in isolation from the other. It is not easy to launch a grand national program aimed at improving health care—which would be one example of "science in policy"—if the medical schools have not previously been enabled—by "policy for science"—to produce enough doctors. Conversely, the funds made available for socially useful projects will influence and to some extent determine the directions pursued by researchers and the careers chosen by students. This relationship has been most dramatically evident in the important fields of engineering, mathematics, and the physical sciences known collectively as "EMP." As Harvey Brooks has pointed out, the expansion of university enrollments and staffs in EMP in the 1960s was influenced strongly by the employment opportunities and the availability of research support reflecting the high priority established by the federal government for military and space technology. Now that the size of the commitment has been significantly reduced and EMP faces a "glum future," the universities have found themselves overextended in these fields.[29] If science were an undifferentiated entity to be mobilized for a given set of purposes one year and a completely different set the next, the distinction between the two aspects of science policy would be a helpful one, but as this is hardly the case, it cannot rescue the concept of a single coherent science policy from its critics.

---

[26] The Secretary-General's Ad Hoc Group on New Concepts of Science Policy, *Science, Growth and Society* (Paris, 1971), p. 37.

[27] Ibid., p. 38.

[28] Quoted in *Toward a Science Policy for the United States*, p. 27.

In view of its difficulties, the suspicion is bound to arise that the main reason this distinction has been introduced is to support the proposition that the needs of basic research and higher education should be provided for on a continuing and stable basis, while practical projects are treated more flexibly. The distinction, then, serves to narrow the effective meaning of the call for a national science policy to a concern for a "policy for science"—a policy that makes basic research and the education of scientists safe from the budget cutters. Even if we agree that the objective is desirable, we cannot conclude that such a conception of science policy is what it claims to be—namely, a better-balanced and more rational way of ordering the ensemble of activities in science and technology than short-sighted, pragmatic politicians are inclined to follow. Still worse, it will almost surely antagonize industrial researchers who are likely to see it, with reason, as a device for promoting the special interests of university scientists to the detriment of a genuinely balanced national program.

---

[29] Brooks, "The Physical Sciences," p. 111.

Alternative Roles for Congress

If, for all these reasons, it would not be advisable for Congress to embrace the campaign for a declared national science policy, there are alternatives open to congressmen anxious to meet some of the serious concerns that have inspired this campaign. One such alterntive would be to recognize that the call for a national science policy is perhaps better understood as a plea for an improved system of establishing the social priorities which science and technology are thought to serve. No single institutional device will satisfy this need, but it would be helpful if the executive were to create an office—preferably one that would be independent of the OMB, and possibly one that would serve as a kind of common staff and unifying agent for the existing councils—which would serve a general policy planning function. The purpose of such a general policy planning staff, which has been proposed before,[30] would be to undertake fairly long-term planning so as to complement the necessarily more immediate focus of the OMB. Congress could perform a useful service by persuading the executive of the need for such an office, which might be thought of as a broadened Office of Science and Technology to serve at the level of the executive office. Such an office would be staffed by people in a variety of disciplines. Like the Office of Technology Assessment, it could seek outside assistance. OST, despite the best efforts of its staff, may have been too narrowly construed to serve such a general purpose. Similarly, PSAC could be reconstructed with a broader membership to restore communication between the academy and the executive and to provide the president with relatively disinterested counsel on the technical merits of proposals advanced by the executive departments and agencies.

On a more continuing and less institutional level, congressmen might also consider that they perform an essential and unique function in the American system simply by serving as the national platform for the debate of public issues concerning science and technology. Congress, precisely because it is designed more for deliberation than for action, and because it is a cockpit for contentious and ambitious politicans, is ideally suited to serve, as John Stuart Mill put it in describing the role of legislatures, as a "congress of opinions." The success of Congress in performing precisely such a function in the ABM debate and in the inquiry that led to the rejection of the SST could serve as a valuable precedent. The executive is almost bound to enlist experts who share its ruling perspectives. Congress, on the other hand, ought to aim at regularizing and institutionalizing the adversary process. As more and more issues require resolution in the light of the best technical advice rather than by compromise among interested groups, the effort to publicize and to come to terms with the conflicting views of experts becomes more and more imperative. Congress offers an ideal forum for the exposure of public issues involving conflicting expertise.

---

[30] Among a number of such proposals is one calling for a presidential office for "strategic policy assessments" which emerged out of discussions in a Government Policy Forum at the Woodrow Wilson International Center for Scholars, Washington, D.C., in 1973. For a similar proposal, see also the report of an ad hoc committee appointed by the National Academy of Sciences, "Science and Technology in Presidential Policymaking: A Proposal" (Washington, D.C., June 1974), pp. 34–35.

At present, as in the recent past, most such debates are likely to revolve around the role of science and technology in relation to the two key concerns of national policy: defense and the economy. In these areas, it is expected that the executive will exercise initiative and Congress will serve as consultant and critic. By encouraging thoughtful debate on new technologies, Congress performs an essential democratic function. And if we take seriously the predictions of those who argue that the preoccupation with national security and economic growth is being superseded by a concern for the quality of life, then it may well follow that the role of Congress will become even more significant in the life of the nation than it has been in recent years. If the issues of national security and economic management are debatable, as they surely are, the issues that arise in defining what is meant by the quality of life are even more debatable. Defense and the economy, moreover, are areas of policy which involve vital international issues in very complex ways—ways which may elude the grasp of congressmen whose careers are mainly dependent on their attention to domestic needs. The quality of life, however, is far more amenable to treatment in the light of domestic values and needs than these other issues.

The role of Congress in science policy has to be seen in the light of the larger role of Congress in the American political system and in the light of the critical importance of science and technology to the achievement of social objectives in virtually every area of public policy. The case for a larger role for Congress rests on the essential principle that decisions concerning the uses of science and technology are so vital to the citizenry that they should not be made in secret or bureaucratically or by the executive alone. The fact that this principle has been so egregiously violated in recent years should spur rather than deter congressional efforts to subject major policy issues to informed legislative scrutiny. Scientists should not expect Congress to take on a responsibility for leadership that is best exercised by the executive,[31] but they ought to insist that Congress improve its performance as a representative assembly and watchdog of the executive.*

---

[31] G. B. Kistiakowsky, "Presidential Science Advising," *Science*, 184, no. 4132 (April 5, 1974), 40.

---

* An earlier version of this article was presented at the annual meeting of the American Association for the Advancement of Science in San Francisco, February 27, 1974. For research support, the author is indebted to the University of Toronto, the Canada Council, and the Woodrow Wilson International Center for Scholars. Colleagues at the center, John Logsdon in particular, made helpful comments on an earlier draft.

A Typology of Technological Policymaking in the U.S. Congress

Patrick W. Hamlett

In a rapidly developing research field, broad theoretical analysis often fails to keep pace with the growth of independent and unconnected research. At some point, it becomes fruitful to stop, to reassess what has been accomplished in the field, and to attempt to develop the broader conceptual coherence needed for theory formation. Science and technology policy studies have grown dramatically in the last few decades, and, as books, articles, and scholarly journals on this subject increase, the need for some coherent theoretical grounding grows accordingly.

One initial step in theorizing is the creation of typologies of phenomena, which can then be used to generate testable empirical hypotheses. And so, this essay describes a typology of technological policy decisions made by the U.S. Congress, a typology that may prove useful in integrating such decisions into a theoretically coherent whole.

A typology itself is not an empirical proposition but a more or less useful "filing system" for grouping or categorizing similar phenomena. In consequence, typologies are judged by their logical coherence and conceptual breadth. If the dimensions selected for a typology are designed well, then the typology itself will suggest empirically verifiable hypotheses, the results of which may modify, extend, or enrich the original classification.

The categorizations presented rest upon an explicit conception of the scope of technological policymaking which is a good deal broader than traditional emphasis upon "high tech" hardware, moon missions, or new laboratory products. Technological policies include all of these, of course, but they also include governmental decisions in other areas. Decisions affecting national transportation, communications, or energy-production systems, for instance, often involve a large technological component, or may affect an existing technological system, so also do decisions about safety regulations, risk-assessment, resources management, and many other areas. Technology policy analysis assumes that, in all of these diverse policy domains, certain discernible regularities may be attributed to the technological components of the specific policy under consideration. Although the policymakers who are pursuing other goals may have regarded the technology affected by a policy decision as having only secondary importance, in many cases, the technology selected or altered will have affected that decision. Research in technological policymaking, then, often moves through a variety of domains—e.g., energy policy or transportation policy—as it follows the theoretical connections between decisions.

The influence of the U.S. Congress in the creation of new programs, in continuation of funding, and in legislative oversight make it a natural center for research in technological policy analysis.[1] This typology focuses upon two policy dimensions that are especially sensitive to the likelihood of short-term constituent pressures affecting the coherence of long-term technological policies made in Congress. The first dimension of the typology—*policy discontinuity*—assesses the degree of deviation from established practices and policies in the area under consideration. The policy option chosen by Congress may vary from a simple reassertion of existing policies, with no change, to relatively modest modifications of existing policies, to truly radical departure from the established situation.

Patrick W. Hamlett is Assistant Professor of Political Science in the Department of Social Sciences at the University of Missouri–Rolla, Rolla, Missouri 65401.

© 1983 by the Massachusetts Institute of Technology and the President and Fellows of Harvard College. Published by John Wiley & Sons
Science, Technology, & Human Values, Volume 8, Issue 2, pp. 33–40 (Spring 1983)
CCC 0162-2439/83/020033-08$02.60

The latter option may occur through the introduction of techniques that either create new technical capacities or improve existing capacities in significant ways, or through a major alteration in the structure, support, or utilization of an established technology. This dimension is a continuum, from moderate change through radical alterations.

The second dimension of the typology attempts to capture certain characteristics of the social, economic, and political environment in which the policy changes will occur. This variable—*technological integration*—assesses the relative degree of concentration or decentralization in the network of social, economic, and political institutions and relationships surrounding a technology, that is, the "technological infrastructure." Except for a technical breakthrough made by someone unconnected with an existing infrastructure, most technologies will be associated with organizations committed to their production and use. Surrounding each modern technology are networks, small and large, of primary providers—engineering designers, manufacturers, contractors, suppliers, wholesalers, retailers, consumers, and so forth—whose livelihood depends upon the existing technology and the public policies bearing on it. In many cases, a variety of ancillary groups derive considerable income from servicing the primary providers, as in financing, fuels, and parts for the industry, and in housing, food, consumer goods, and services for the workers. Congressional decisions about renewing, altering, or terminating existing policies are of critical importance to all members of the related technological infrastructure. (And, in many cases, the decision may be of greater importance to the service firms than to the primary providers, whose resources and size may permit them to weather policy changes more easily than small, local firms.) An established technological infrastructure thus will support the continuation of favorable public policies affecting their technology, and oppose changes in policy perceived as potentially disruptive.

Technological infrastructures vary across a variety of dimensions. For well-established technologies, in use for years or decades, the infrastructure may include national organizations, such as trade associations, professional councils, lobbyists, or political action committees. As such, these infrastructures are part of the panoply of interest groups participating in American politics. Other infrastructures may lack significant national organization, or any formal organization at all. These tend to be the newer technologies seeking entry into the market, although some older technologies also lack extensive organization.

Of more significance to this analysis is the relative degree of internal concentration or decentralization within the infrastructure. A concentrated infrastructure will have relatively few primary providers and relatively few service firms dependent upon them. Quite often, the primary providers will be large, integrated corporations involved with the manufacture or development of an array of technologies, often applicable in many different businesses. A concentrated infrastructure can also provide a technology for a limited number of customers, as illustrated by the relationship of the civilian nuclear power industry to the public utilities. In some cases, consumers may be scattered throughout the country, as in the communications and computer industries. And in other cases, concentrated infrastructures deal with only one customer, for example, government purchase of large weapons systems.

Decentralized infrastructures are composed of many primary providers, frequently scattered about the country, a large number of service firms are associated with the primary providers, entire communities may be economically dependent on the primary providers. Decentralized infrastructures often develop around mature, standardized, non-exotic technologies and involve a large number of customers also widely scattered around the nation. Railroad transport, both passenger and freight, is an example of a decentralized infrastructure.

The importance of technological integration for Congressional decisions derives from the differing political tactics that characterize decentralized versus concentrated infrastructures. Each kind of infrastructure presents a different set of political opportunities and constraints to Congressional policymakers.

Some Examples

The two dimensions of the proposed typology—policy discontinuity and technological integration—intersect to create four major technological policy types (see Table 1). Each dimension is a continuum, moving from lesser to greater, rather than a fixed, discrete variable. Thus, the four policy types should be regarded as idealized forms that blend into each other, rather than as rigidly sep-

Table 1. Policy Discontinuity

Technological Integration		Moderate	Radical
	Decentralized	Moderate Decentralized	Radical Decentralized
	Concentrated	Moderate Concentrated	Radical Concentrated

arated constructions. For moderate-decentralized policies, the degree of policy change contemplated by Congress represents only a marginal variation in existing policies, concerning a technology possessing a decentralized infrastructure. Radical-decentralized policies, on the other hand, involve significantly discontinuous changes in an established policy, or significant alterations in the technology itself, affecting a decentralized infrastructure. Likewise, moderate-concentrated policies involve only modest policy changes of concern to a concentrated infrastructure, while radical-concentrated policies represent substantial policy changes affecting a concentrated infrastructure.

Moderate- and Radical-Concentrated Policies

Concentrated infrastructures, because they have fewer participants, offer Congress a greater opportunity to make and implement long-term technology management decisions. Because virtually any decision about an existing technology affects various groups differently, the greater the number of participants in the decision process, the greater the difficulty for Congress in defining technologically responsible policy goals and in structuring programs to achieve those goals. This is especially true when Congress must choose to switch technologies, reducing or eliminating programs favorable to one technology in favor of another. Such decisions become necessary as technical developments and breakthroughs, changes in constituent demands, fiscal constraints, or other political changes press upon existing policies.

Over the years, Congress has sometimes intentionally kept some technological infrastructures concentrated, to maintain close control of technologies considered too important for either broad public dissemination or broad public participation in the policy process. Most weapons procurement decisions, for instance, fall within the concentrated infrastructure categories—that is, few primary providers and service firms and only one primary consumer. Of course, in nearly all cases, executive agencies develop the technologies and recommend them to the legislature, which must then decide about authorizing and appropriating additional production funds. Because of the time and previous funding invested in bringing new weapons technologies to production readiness, there is an inevitable pressure to continue onward with existing plans. In consequence, many weapons decisions made by Congress fall on the moderate end of the policy discontinuity scale. Most are modifications of existing technologies rather than radical changes in design or capabilities. The decision to purchase one kind of wide-body airframe over another, to serve as troop and material transports, is usually not a decision involving radically different technical designs. Most decisions concerning other weapons involve similar predictable technical advances upon existing technologies.

However, truly radical policy decisions have been made regarding weapons systems. The choices to develop the atomic and hydrogen bombs, of course, were radical-concentrated policy decisions—so radical that Congress felt the need to create a new mechanism for monitoring those decisions, the Joint Committee on Atomic Energy (JCAE). Another series of radical-concentrated decisions resulted in the introduction of the intercontinental ballistic missile, the MIRV'd ICBM, and the Cruise missile into the American nuclear arsenal.[2]

Congressional decisions to support physics research using particle accelerators, the development of the civilian nuclear industry, and the development of a domestic synthetic fuels industry are additional examples of both moderate- and radical-concentrated policy decisions. In each case, a small number of primary providers and ancillary concerns are involved, along with relatively small numbers of customers for the technology involved.

Sponsorship for particle accelerator research began with the Manhattan Project to develop the first atomic bomb, and continued after the end of the war. At first, the technology of such research represented a radical departure in physics. However, as the technology standardized, later decisions to support their construction amounted to only moderate alterations of existing policies. In fact, eventually, particle accelerator funding became the target of traditional distributive policy competition, as midwestern research universities organized MURA (Midwestern Universities Research Association) to wrest their "fair share" of accelerator funding away from the control of such institutions as the University of California at Berkeley and the Massachusetts Institute of Technology.[3]

Congressional involvement in the creation of a civilian nuclear power industry is an archetypal example of a radical-concentrated policy decision. The technology of using atomic fuel to generate electricity was surely a radical departure from the standard gas, oil, and hydroelectric generating systems. The technology was in the sole hands of the government, administered by the Atomic Energy Commission (later the Nuclear Regulatory Commission) and overseen by JCAE. Following President Eisenhower's famous "Atoms for Peace" speech in the United Nations, Congress enacted the 1954 amendments to the 1946 Atomic Energy Act, which made the commercial use of atomic power the official policy of the government. Ironically, the technology's prospective customers, the public utilities, at first resisted introduction of the new process. Risks attended the manipulation of radioactive materials, large capital outlays were needed to build the new facilities, and the potential losses from accidents were great. All these aspects served to undermine the utilities' enthusiasm for the new system. Not until Congress promised to provide research and development funding for safety research, and capital assistance in the building of facilities and, most importantly, passed the 1957 Price-Anderson Act limitations on utility accident liabilities, did the public utilities "get on board" the nuclear energy bandwagon.

During the infancy of the new industry, Congress sought to preserve its decisions from too much uninformed public participation. The concentrated nature of the existing nuclear infrastructure, the complexity of the technology itself, and the optimistic projections about "energy too cheap to meter," all contributed to a strictly limited public access to the overall decision process. The commitment to a nuclear future for America meant that the technology had to be managed with long-term goals in mind. To allow short-term needs and pressures to pull the program off track, it was felt, would only waste very valuable resources.[4]

In a similar fashion in 1980, the Congress chose to invest heavily in another energy technology, synthetic fuels. In this case, the government was not the sole possessor of the technology; synfuel processes were known within the private sector. Development costs, however, deterred many companies interested in synfuels from undertaking the capital investments. The 1980 Energy Security Act was aimed at providing investment capital and other incentives to private firms to develop the technologies to a level of commercial readiness. The Act called for a $20 billion investment in synfuels technologies, perhaps as much as $88 billion in time. The technology was to be in the hands of a relatively small group of primary providers, and was intended for a relatively small number of consumers. The technology itself represented a radical departure from conventional natural petroleum recovery techniques. And, just as in the case of nuclear energy, the existing petroleum infrastructure resisted the introduction of such a radical technological process. Even after the passage of the Act, few major petroleum corporations became deeply involved in the program. To manage the program, Congress also created a new administrative agency, the U.S. Synthetic Fuels Corporation.[5]

Moderate- and Radical-Decentralized Policies

There are examples of both moderate- and radical-decentralized policy decisions as well. An example of a radical-decentralized policy was the 1979 effort to cut Federal subsidies for Amtrak passenger rail service by about 43 percent. Although a Federal corporation (Amtrak) coordinates and administers rail passenger services, it must do so through the facilities of the private railroads whose tracks Amtrak trains must use. Moreover, the service companies in the thousands of communities served by Amtrak are numerous and dispersed throughout the United States. The 1979 cutback in Federal subsidies constituted a radical change in transportation technology policy and, not surprisingly, it prompted widespread political opposition from the existing decentralized infrastructure.[6]

Most technology-forcing environmental protection legislation also falls into either moderate- or radical-decentralized policy categories. The "polluter pays" provisions of such enactments as the Clean Air Act (and its 1977 amendments), the Clean Water Act, the Solid Waste Disposal Act, the Toxic Substances Control Act, the Surface Mining Control and Reclamation Act, and several others, sometimes require significant initial technical steps on the part of widely scattered manufacturing, public utilities, energy corporations, or city governments. There is no centralized concentration in the provision of the various environmental technologies in question, and a wide dispersion of consumers.

Discussion

For Congress, the key to making long-term technology management decisions is to gain a degree of policy insulation from cross-cutting political pressures. Only through insulating such decisions from conflicting and disruptive constituent demands does Congress gain the flexibility to review existing technology policies dispassionately, to determine if they still contribute to (or hinder) the broader public weal. Relatively free of pressure politics, Congress may be able to create policies more congruent with emerging technical options, policies which may be more beneficial to the majority of people even if temporarily harmful to those attached to the existing policies. Dealing with concentrated infrastructures allows a greater—although never absolute—degree of policy insulation. The early years of the civilian nuclear power industry illustrate the extensive efforts of Congress, through a concentrated technological infrastructure, to develop and implement long-range policy. The considerable literature reviewing the early history of nuclear power is unanimous in detailing how both the AEC and the JCAE pursued policy insulation in directing the evolution of nuclear technology, in the support for the light water reactor (LWR) over other reactor designs for both military and civilian uses, and in setting technical standards for reactor siting decisions.[7]

Similar tight controls mediated through concentrated infrastructures was the management mechanism of choice for several other recent technical developments. The authorization and appropriations for chemical and biological warfare systems, for managing nuclear wastes, for developing the neutron "bomb," antiballistic missile systems, and many others, demonstrate the utility of concentrated infrastructures in achieving policy goals. Of course, the management of technology through concentrated infrastructures does not always work smoothly. Even moderate policy shifts (e.g., a new tank design based on "normal" extensions of existing designs) can generate intense political pressures as various private manufacturers compete for lucrative contracts. Yet even here the concentrated infrastructure permits relatively efficient management: all parties concerned support the general direction of policy, while they may vie with each other over specifics.

However, concentrated infrastructures sometimes fail to maintain the policy insulation needed to permit long-range technological decisions to proceed. In two recent examples, the supersonic transport funding issue and the proposed antiballistic missile system,[8] the policies seemed at first to be quite ordinary developments within established programs. Each proposed program possessed both Presidential endorsement and extensive public support from the infrastructure members. Both the SST and the ABM represented long-standing commitments to "high tech" solutions to national problems—in the one case, a dwindling private aircraft industry; in the other, national defense. In both cases, a diverse array of non-infrastructure participants—sometimes noted scientific and technological experts, at other times non-expert citizens—were able to penetrate the decision process so successfully that Congress eventually made decisions about each technology which were substantially different from those initially expected. In both cases, the non-infrastructure participants based their objections to Congressional support for the SST and the ABM largely upon scientific and technical arguments, as opposed to, for example, more traditional economic issues. The SST was resisted because of noise and air pollution problems; the ABM, because of doubts about its technical feasibility and the fear of becoming a target for Soviet missiles if ABM facilities were constructed near population centers. Economic arguments were made by the infrastructure supporters who were likely to benefit from Congressional funding.

Another example of a radical-concentrated policy unable to sustain policy insulation was the 1980 Energy Security Act, which created the U.S. Synthetic Fuels Corporation. In this case, a combination of political pressures overcame a concentrated infrastructure's opposition to a radical change in petroleum policy. Heavy Federal investment in the new technologies of synthetic fuels—oil shale, tar sands, and coal gasification, for example—succeeded in 1980, although it had failed as recently as 1975, because of a confluence of gasoline shortages and price increases precipitated by the Iranian crisis and the accompanying public demand that the government do *something* about the energy crisis. Synfuels supporters in the Senate also proved willing to enlarge the original modest bill passed by the House by adding on nine unrelated energy technology programs. The original $3 billion synfuels bill from the House ballooned into a massive $122 billion program, with new initiatives in solar, geothermal, biomass conversion, conservation, and acid rain research technologies added to maintain the fragile coalition. Infrastructure members fought the omnibus bill, and were joined by a variety of environmental groups and affected state governments. But their objections, often backed by extensive scientific analyses, fell on deaf ears.[9]

Although concentrated infrastructures offer greater policy insulation to policymakers interested in long-range technology management, when the policy change is sufficiently radical in nature, the policy insulation may be undone by the concerted efforts of other affected groups. Often, such groups do not have a direct economic stake in the contemplated policy decision, but instead represent larger social values, such as the need for environmental protection. Also, non-infrastructure groups must often resort to unconventional political tactics to penetrate the screen of policy insulation.

Concentrated infrastructures may then, under certain circumstances, fail to provide adequate policy insulation for policymakers. Decentralized infrastructures, however, operate generally through intense political pressures upon policymakers in Congress. Decentralized infrastructures can make long-term technological planning more difficult because of the greatly enlarged roster of affected interests whose concerns must be taken into account in whatever policy emerges. In addition, dispersed infrastructure members can be adept at lobbying the Congressional representatives in their home districts. As John Kingdon and many others have pointed out,[10] no other influence shaping Congressional decisions is more potent than organized home district constituents. It would take a courageous Senator or Representative to vote against the desires of a home district infrastructure while promising that the policy change in question will benefit everyone "in the long run." Getting to the point where "in the long run" extends beyond the next election is often difficult, to say the least.

The existence of decentralized infrastructures creates less policy insulation for Congress, but they do offer many opportunities for individual members of Congress to display their sensitivity to constituent demands. Because the infrastructure members are widely dispersed, and often politically organized, they offer an attractive, attentive constituency for the Congressmen or women to develop and maintain a reputation of protecting local interests. What may be lost in long-range policy coherence may be made up by protecting vocal constituents from what they regard as undesirable policy changes. Short-term electoral security may therefore be purchased through special exceptions, exemptions, exclusions, permissions, licenses, clearances, and so forth, from long-term policies.

Such a case of long-range management upset by localized political opposition from decentralized infrastructure members as the 1979 Amtrak service cutbacks illustrates the problems inherent in choosing to shift from an established but poorly performing technology that possesses a decentralized but attentive infrastructure. In its first nine years, Amtrak's revenues increased by 205 percent, while its expenses grew by 291 percent. In 1977, after a struggle over Amtrak subsidies, Congress ordered the Secretary of Transportation to prepare a thorough "zero-base" study of the entire system. The Secretary's Final Report recommended substantial restructuring, with a 43 percent reduction in passenger miles, and the termination of several long-haul trains. Public response to the Secretary's recommendation was explosive. Amtrak serves thousands of communities throughout the United States, hundreds of which would have been adversely affected by the proposed policy change. Passenger organizations, environmentalists, city and state governments, even businesses that serviced passengers, flooded their representatives with demands that the cutbacks be reduced. Most complained that not only were "their" trains being arbitrarily cut, but also most of the remaining service would be concentrated only in the "Northeast corridor," from Boston to Washington, DC. As one person observed:

> You see, people in places like Kansas are not going to pay just to run trains in the Northeast. They want their own trains. That's what happened on Capitol Hill.

The clamor to protect regional economic interests eventually affected the Secretary's planned reductions. The 43 percent cutback was reduced to only about 20 percent, "splitting the difference" between supporters and opponents of the plan. Moreover, several poorly performing trains that had been slated for termination but crossed the districts of important Congressmen were given reprieves. Whether or not the Secretary's plan was the best available, the final result did reflect *ad hoc* policymaking rather than long-term management.[11]

Conclusion

The typology I have drawn highlights an important irony in American technology policymaking. Because concentrated infrastructures have fewer participants whose diverse demands must be somehow integrated into a single policy, they are better policy partners for those members of Congress interested in long-term policy management. However, they are also frequently less accountable to the public specifically because they have fewer participants in the decision process. Hence, they often lack safeguards against manipulation by special interests, and sometimes produce policies benefitting only infrastructure members. Their potential for long-term management for the common good may often be subverted by their undemocratic character.

Although decentralized infrastructures are more democratically accountable because they have more participants, they often produce incoherent policies precisely because of that responsiveness. The potential for responsive long-term policies is often subverted by the clash and compromise of so many voices. The demands of technological responsibility and democratic responsiveness clash as Congress makes technological policy decisions. This conflict remains the fundamental problem of science and technology policymaking in our society.

The role of Congress in technological policymaking emphasizes the potential for internal conflicts between the need for long-term, coherent policies in technological areas and the dominant operating style of a national legislature. This operating style suffers from the defects of its virtues: technologically responsible policies can be undermined by Congress' own democratic responsiveness, when individual members of Congress seek to protect local groups. Responsible technological management requires a long view of social change, as well as the willingness to take risks and to accept the inevitable dislocations accompanying technical change. Technological opportunities beneficial to society as a whole must be balanced against the cost of upset individual lives. Constituent demands to be given a special exclusion from such disruptions, if multiplied by all of the members of Congress, would obviously make coherent policies impossible, a situation that is especially true for long-term policies whose rewards may only be visible years or decades later.

The creation of the Congressional Office of Technology Assessment, and greater use of the resources of the General Accounting Office, the Library of Congress Legislative Research Service, and the Congressional Budget Office all contribute to an increased ability to manage technology policy in Congress. Yet Congressional policymaking remains highly complex and, despite improvements in forecasting and analysis, still fraught with cross-cutting pressures. Decision-making is fragmented among a multitude of committees and subcommittees, interest group pressures, political debts and deals. The close working relationships between committee members, interest group lobbyists, and bureaucrats—the "cozy triangles"—and the highly constituent-oriented style of many members can contribute to the derailment and derangement of long-term policy coherence for the sake of protecting established policies and privileges.

This conflict, of course, is not unique to technological policymaking. However, the way that Congress resolves the conflicts inherent in the opposing agendas of technological responsibility and democratic responsiveness is fundamentally important for both sets of policy demands. Long-term policies beset by repeated interruptions and alterations to accommodate local demands stand little chance of success. By the same token, long-range policies that ignore their potential impact upon individual lives violate established ethical norms central to our political self-definition. The virtue of the typology I have described in this essay is its direct focus upon this enduring conflict in policymaking.

Designing measures of relative technological integration and of relative policy discontinuity should pose no intractable problems. The existence and strength of a technological infrastructure can be measured through an analysis of firms already engaged in the specific technical field, through investment levels and trends, sales figures and profit levels, distribution networks, even stock market activities. Where technological policies involve governmental contracts instead of market forces, the levels of Congressional appropriations, size and distribution of contracts and subcontracts, and similar measures will serve as good indicators of the existence—or prospective formation—of a technological infrastructure, along with some estimate of its strength. It remains to develop some form of scalar ranking of various technological infrastructures, thereby permitting empirical testing of the impact of different forms of Congressional policymaking.

Levels of policy discontinuity may be more difficult to rank, since the quality of discontinuity can be more elusive. In some cases, the new technology—for instance, wind-generated electricity—may appear to be discontinuous when, in fact, it is more correctly described as the reintroduction of an older technical tradition. In other instances, such as the creation of wholly new life forms through genetic engineering, it is possible to determine a high level of radical discontinuity. One possible approach would involve expert opinion sampling in the relevant field, to develop a consensus concerning the degree of policy discontinuity in a proposed policy change.

This policy typology, therefore, can also be seen as a possible first step toward predicting the outcomes of specific technological policy battles in Congress. Modification and refinement would then emerge through practical application in specific studies, accumulating thereby a set of case studies framed in essentially the same theoretical language. This Congressional policy typology may thus serve to advance the sophistication and rigor of technological policy analysis, and prepare the way for further theoretical developments.

NOTES

1. Congress is the focus of this typology because it contributes greatly to the evolution of technological policy in the United States, in a wide variety of ways. It is ironic that the scholarly literature has tended to neglect Congress' role in technological policymaking, while examining the special influences of the White House and "techno-science" agencies, such as NASA, the Department of Defense, NIH, etc. Dorothy Nelkin *et al.*, *Controversy: Politics of Technical Decisions* (Beverly Hills, CA: Sage Publishing, 1979); David Elliot, *The Control of Technology* (New York: Springer–Verlag, 1976); Ina Spiegel-Rösing, Ed., *Science, Technology, and Society: A Cross-Disciplinary Perspective* (Beverly Hills, CA: Sage Publishing, 1977); Thomas J. Kuehn and Alan L. Porter, Eds., *Science, Technology and National Policy* (Ithaca, NY: Cornell University Press, 1981); William T. Golden, Ed., "Science Advice to the President," special issues, Parts 1 and 2, *Presidential Politics and Science Policy* (New York: Praeger Publishers, 1978); Henry Lambright, *Governing Science and Technology* (New York: Oxford University Press, 1976); Joel Primack and Frank Von Hippel, *Advice and Dissent: Scientists in the Political Arena* (New York: Basic Books, 1974).
2. Ted Greenwood, *Making the MIRV: A Study of Defense Decision Making* (Cambridge, MA: Ballinger Publishing Company, 1975); Ronald Huisken, *The Origin of the Strategic Cruise Missile* (New York: Praeger Publishers, 1981).
3. Daniel S. Greenberg, *The Politics of Pure Science* (New York: The New American Library, Inc., 1967), chapters X and XI.
4. There is a considerable literature reviewing the management of civilian nuclear energy. See especially, Steven L. Del Sesto, *Science, Politics and Controversy: Civilian Nuclear Power in the United States, 1946–1974* (Boulder, CO: Westview Press, 1979); Steven L. Del Sesto, "Nuclear Reactor Safety and the Role of the Congressman: A Content Analysis of Congressional Hearings," Volume 42, Number 1 (February 1980): 227–241, and Dorothy Nelkin, *Nuclear Power and its Critics* (Ithaca, NY: Cornell University Press, 1971).
5. Patrick W. Hamlett, "Technological Policymaking in Congress: The Creation of the U.S. Synthetic Fuels Corporation," paper presented at the Annual Meeting of the American Political Science Association, Denver, CO, 1–5 September 1982.
6. Patrick W. Hamlett, "Technological Policymaking in Congress: The Amtrak Passenger Service Cutbacks of 1979," paper presented at the Annual Meeting of the Midwest Political Science Association, Cincinnati, OH, 17 April 1981.
7. Del Sesto, "Nuclear Reactor Safety and the Role of the Congressman," *op. cit.*
8. Joel Primack and Frank Von Hippel, *op. cit.*, chapters 2, 5, and 13.
9. Hamlett, "Technological Policymaking in Congress: The Creation of the U.S. Synthetic Fuels Corporation," *op. cit.*
10. John Kingdon, *Congressmen's Voting Decisions* (New York: Harper and Row, 1980); Aage Clausen, *How Congressmen Decide: A Policy Focus* (New York: St. Martin's Press, 1973); John Jackson, *Congress: The Electoral Connection* (New Haven: Yale University Press, 1971); Donald Matthews and James Stimson, *Yeas and Nays: Normal Decision Making in the U.S. House of Representatives* (New York: John Wiley and Sons, 1975).
11. Hamlett, "Technological Policymaking in Congress: The Amtrak Passenger Service Cutbacks of 1979," *op. cit.*

B. Leadership in the Executive Branch: Recent Trends and Their Implications

PERGAMON
POLICY
STUDIES
ON SCIENCE AND
TECHNOLOGY

Science Advice to the President

William T. Golden
Guest Editor

A Special Issue of Technology in Society

George Bugliarello
A. George Schillinger
Editors

Martha Miller Willett
Managing Editor

Pergamon Press
NEW YORK • OXFORD • TORONTO • SYDNEY • FRANKFURT • PARIS

Technology In Society, Vol. 2, pp. 165-174 (1980)
Printed in the USA. All rights reserved

0160-791X/80/010165-10$02.00/0
Copyright © 1980 Pergamon Press Ltd

Executive Leadership for Science and Technology

Willis H. Shapley

In its latest incarnation, advisory machinery on science and technology in the Executive Office of the President has now been in place for more than two years. It is timely to look at what has emerged, compare it to previous expectations and current needs, and give some thought to how the pattern of central leadership in science and technology in the Executive Branch might evolve most usefully over the next several years.

Advice to the President on science and technology is not an end in itself. The effectiveness of the organizational machinery to provide it depends on the context in which it operates. One crucial aspect, of course, is how the Science Adviser fits into the structure of the Executive Office and the operating patterns of the Presidency. Another is how the Science Adviser and his office relate to the rest of the Federal science and technology establishment. Thus, evaluation of the current Office of Science and Technology Policy (OSTP) and consideration of the ways in which it might evolve entail the broader questions of the mode of operations of the Executive Office as a whole and the organization of the science and technology activities of the Executive Branch.

These questions are too big to be dealt with in their entirety here. Some of the considerations which experience and observation suggest may be parts of the answers can be indicated, leaving many blanks to be filled in by others. To anticipate, the basic thesis will be that the current concept of OSTP is generally good and working well, but that there is an additional need for a strong focus of central leadership in Federal science and technology which should not and does not need to be in the Executive Office. This leads to the conclusion that the goal should be a minimal Science Adviser's office in the Executive Office supported by a heftier "Agency for Science and Technology" which, in addition, would perform central planning and leadership functions and bring together a group of federal agencies directly concerned with science and technology.

Willis H. Shapley (b. 1917) is senior consultant on research and development budgets and policies, American Association for the Advancement of Science. For many years as a senior official in the Bureau of the Budget, Executive Office of the President, he was concerned with research and development matters, and from 1965 to 1975 was Associate Deputy Administrator of NASA.

Today's OSTP

OSTP as it now exists can be briefly characterized as follows. A distinguished scientist, Dr. Frank Press, serves as Director of the office and as Science Adviser to the President. He maintains a low profile and evidently has earned and gained the confidence and respect of the President, his immediate associates, and key figures in the Office of Management and Budget (OMB) and other elements of the Executive Office. His staff has been held to about 20 professionals by Presidential edict, in order to limit growth of the Executive Office. Extra help is provided by detailees from other agencies, by drawing on a large stable of consultants and by assigning the National Science Foundation (NSF) and other agencies to do studies and reports. Some standing groups and numerous ad hoc teams on particular problems have been formed, comprised of government or non-government people or both, but there is no standing general advisory committee like the President's Science Advisory Committee (PSAC) of the 1960s.

In its first two years, Dr. Press's OSTP has been active across a wide front of problems in science and technology. Budgets for research and development, especially for basic research, have been a prime concern. Dr. Press and his staff have established working arrangements with OMB whereby OSTP is apparently accepted as a responsible partner in the decision process, rather than regarded with suspicion as a special pleader for science and technology. The range of other activities of OSTP is impressive. Those that are matters of public knowledge include the initiation of a major study of innovation in industry, strengthening basic research policies and organizations in mission agencies (notably in the Departments of Energy and Defense), leading an interagency review of national space policy, establishment of interagency programs on earthquakes and climate, development of policies in important health and environmental areas, promotion of a new program of competitive grants for agriculture research, and establishment of a new Institute for Science and Technical Cooperation with developing countries. OSTP staff participates in the Executive Office system of preparing, reviewing, and endlessly revising the Presidential Review Memoranda (PRM's) and other documents through which policy options are developed and presented to the President. On important issues, Dr. Press also gives his personal advice and the independent views of his advisory groups directly to the President and his associates.

In terms of visible results of general interest, two accomplishments stand out. One is the wholehearted acceptance by the Carter administration, after an initial period of ambivalence, of the importance of Federal support of basic research and of budgetary policies protecting basic research and recommending substantial increases each fiscal year. This was a continuation of policies instituted by President Ford at the urging of his Science Adviser, Dr. H. Guyford Stever, but there is no doubt that the new Science Adviser was largely responsible for the special emphasis given by President Carter to basic research, in many public pronouncements and in specific budget recommendations.[1]

The other outstanding accomplishment is also there for all to see but has gone almost entirely unnoticed. This is the President's special message to Congress on science and technology of March 27, 1979—a 7500-word document that summarizes the rationale, objectives, concerns, and future directions of federal activities related to science and technology.[2] It contained little that was new—no spectacular new proposals or goals. Its significance, both within and outside the government, lies in the fact that it is the first comprehensive and authoritative articulation of Federal policy on science and technology. Previous Presidential statements on science and technology or research and development have been in the nature of catalogues of programs[3] or ad hoc urgings for support of budget requests, usually for basic research, in trouble in Congress.[4] Reports of the National Science Foundation, the National Science Board, and other federal agencies, even those attempting to be comprehensive and carrying a Presidential endorsement, have generally reflected a scientific and technical outlook on national policy rather than national policy on science and technology. It is surely to the credit of the Science Adviser and his office that they were able to produce a wide-ranging and thoughtful statement of Federal policy on science and technology that the President and his administration could accept.

Expectations Fulfilled?

How well is the current OSTP fulfilling the expectations of those who believe in its importance and urged its recreation after the earlier Office of Science and Technology was abolished by President Nixon in 1973? As we have seen, science advice is back in the White House and making its presence felt in budget decisions and other important matters. OSTP is tackling some of the important programmatic and policy questions and has produced a comprehensive statement of Federal policy on science and technology. Have we now reached a viable long-term organizational pattern for advice and leadership on science and technology in the Executive Branch?

The answer to the first question, I believe, is that OSTP is doing a first-rate job in meeting the general expectations to the degree possible within the external constraints. The current OSTP does, however, in some respect fall short of the expectations of the Congressional authors of the legislation mandating its recreation.[5] It is a much slighter organization than seems to have been envisaged. Several functions, spelled out in great detail in the law, have been down-graded or eliminated entirely. The initial reluctance of the Carter administration to accept any science and technology presence in the Executive Office, because of a strong commitment to reducing its total size and suspicion of special pleading for science and technology, was overcome only at the price of restricting the size of OSTP and of removing some of its statutory functions.

By reorganization and other executive actions, responsibility for two major reports was transferred from OSTP to the National Science Foundation: an annual Presidential "Science and Technology Report" and an annually updated "Five-Year Outlook" of problems and opportunities in science and technology; parts of the latter were subsequently passed on by the NSF to the National Academy of Sciences. The new President's Science Advisory Committee (PSAC), set up in the law with an initial two-year life to study Federal organization for science and technology but with the possibility of being transformed by the President into a born-again 1960s, PSAC was abolished outright on the grounds that the President's overall reorganization project could perform the necessary functions.

It is too early to judge how well the Carter administration's stripped down OSTP and the reassignments of responsibilities will approach the legislated expectations. The NSF produced the first annual Science and Technology Report about six months late.[8] The report consisted of a bland "strategic overview" by OSTP, a presentation of budgetary information on Federal research and development that had already been rendered obsolete by Congressional action, and chapters on a variety of other topics that contributed little that was new. It was a disappointing performance, not Presidential in a meaningful sense—in contrast to the subsequent special message of the President in March 1979 discused above. A decision was made to skip the second annual report due in February 1979; NSF says that it will try to do better in 1980. As for the Five-Year Outlook, the National Academy of Sciences and NSF are still at work at this writing, and it remains to be seen what is ultimately produced.

With respect to both of these reports, one must ask whether the expectations written into law really were reasonable in the first place. It is hard to see what real need there is or significant use would be made of the Science and Technology Annual Report, even if it can be produced at the beginning rather than the end of each session of Congress. The President's major recommendations on research and development and other aspects of science and technology are spelled out in his State of the Union and Budget messages. A special analysis in the budget identifies the amounts for research and development; further details are provided in the voluminous justifications of the agencies. There is always the option of a Presidential special message, if needed. In any case, it is difficult to visualize the National Science Foundation, with its central focus on research, especially basic research in universities, as the President's anointed spokesman for all Federal science and technology.

The Five-Year Outlook raises some of the same concern. Its scope is intended to include technology and the application of science and technology to problems of national significance, not just the outlook for science as such. There is much merit in the notion of a periodically updated road map of where science and technology are going and how they might contribute to dealing with important problems, but there is also room for doubt that the annual paperwork exercise implied by the legislation is the best approach. There do not seem to have been compelling reasons for loading this function on OSTP in the first place. A stronger case can be made that the type of in-depth study and looking ahead required could best be done by an independent group able to work with experts in many fields; from this standpoint the delegation of the task to the National Academy of Sciences and its associated engineering and medical organizations seems to make sense.

Even if one accepts that OSTP should not be expected to take on the workload of these specific reporting requirements of the OSTP Act, it seems to me doubtful that OSTP as now conceived can fully meet the needs of the Executive Branch for central leadership on science and technology. The job is too big for a staff office in the White House. There is a need for dealing with more than just the most pressing attention-demanding problems. Leadership should be available when needed on small policy problems as well as great. The whole area of the management of Federal R&D, technical operations and services, cries out for more leadership attention. There should be time to think, to anticipate future problems, and to try to get ahead of them. Decisions should be based on a depth of understanding unlikely to be achieved when the pace is so hectic. A small, dedicated, but overworked staff may be able to put on one virtuoso performance after another, but it cannot be expected to do the full advisory and leadership job that Federal science and technology activities require.

The situation could be mitigated, perhaps, by a modest increase in OSTP staff. This might relieve the pressure and enable OSTP to be more effective in its present mode. But there are objections and disadvantages to consider. Holding down the size of the Executive Office is not just a matter of appearances and campaign promises. The last 20 years have seen the growth—sometimes by fits and spurts, sometimes by slow accretion—of a superimposed bureaucracy within the Executive Office that mirrors or cuts across the main elements of the Executive Branch, a trend that many responsible officials and observers believe should be resisted at every opportunity. For practical reasons, OMB has to be divided into separate cells to deal with groups of agencies; some special staff is undoubtedly needed to coordinate national security and domestic matters. But on top of this, almost every special interest whose concerns span several Federal agencies is able to persuade itself, and sometimes the President or Congress, that it needs a special office in the White House or Executive Office. Telecommunications, drug abuse, aging, environmental quality, space, and consumer affairs, not to mention science and technology, are examples. With the expansion of specialized staffs in the Executive Office, there is an inevitable tendency for decision-making and decision-blocking power to shift from the responsible Cabinet officials and agency heads to anonymous staff people in the Executive Office. Only a few agencies have the political or bureaucratic clout to resist. Problems of coordination of agencies and programs by the Executive Office are multiplied by the difficulties of coordination of so many staff elements within the Executive Office itself. Each further increase aggravates the situation. More staff might help OSTP but, from a broader standpoint, could rightly be viewed as a step in the wrong direction.

The Need for Institutionalization

The most critical shortcoming of the current OSTP concept is that it fails to provide institutional continuity for science and technology advice in the Executive Office and policy leadership in the Executive Branch. The present arrangement is inherently unstable. It depends on personal understandings among the principals in the Executive Office and on the acceptance of OSTP by the other bureaucracies there entrenched. All this could vanish with a change in administration or even a change in some key individuals. No one will deny a President the right to organize, staff, and operate his own office as he and his closest associates see fit. The same reasons that first pointed the Carter administration in the direction of abolishing OSTP, or at least removing it from the Executive Office, might well be persuasive to another President. Even if the shell of an OSTP survived, nothing would be easier than to ignore it if it is not wanted as a part of the official family. We have been down that road before.

Institutional continuity is important for science and technology policy and leadership. The impact of science and technology is a long-term affair. Lead times are long. Studies of important topics can take many months to set up properly, months or perhaps years to conduct, and as long again to evaluate and implement; the whole process often stretches from one administration to the next. Each administration should not have to start from scratch. An institutional memory can help it to learn from past experiences and avoid unnecessary replowing of old ground.

Institutional support is also essential for central policy leadership and advice in science and technology. This can be obtained in part, as OSTP is now doing, by drawing on other agencies in and outside the government and by other indirect mechanisms. But a directly controlled institutional base with a broad range of scientific and technical resources available on call would provide a much stronger capability, both for performing studies and analyses and for managing and evaluating those performed by others.

A New "Agency for Science and Technology"

With OSTP too slender to do the total job and facing an uncertain long term future in the Executive Office, what should be done to provide for effective advice and leadership in science and technology in the years to come? The answer is to establish, in addition to OSTP, a new, permanent, strong, independent agency with broad central responsibilities for advice and leadership in science and technology in the Executive Branch. The essential features of the new agency would be fourfold.

First, it would be headed by a Presidential appointee of Cabinet rank, although the agency would not need to be a Cabinet department—let us call it the "Agency for Science and Technology" (AST). The head of AST—call him or her the "Administrator"—would be the principal official in the Executive Branch responsible for policy, planning, advice, and general leadership on matters involving science and technology that are not clearly within the scope of responsibilities of other departments or agencies. He or she would assume many of the responsibilities currently performed or expected of OSTP, and be a principal supporting arm to the President's Science Adviser and OSTP for the functions they retain. The relationships of the Administrator and AST to the President, the Science Adviser, and OSTP would correspond to those that have existed for many years between the Secretaries and Departments of State and Defense on the one hand and the President, the Special Assistant for National Security and the National Security Council staff on the other. The Administrator, like the Secretaries, would be the senior official under the President in his areas of responsibility. He or she would have full management responsibility for the resources and activities of the agency and direct access to the President to give advice and receive guidance. The President's Science Adviser, like the Special Assistant for National Security, would be the President's personal adviser at the White House, a second independent voice when wanted. He or she would see that science and technology matters receive proper attention in Executive Office decision-making and that interagency policy and program problems are appropriately dealt with. The Science Adviser and the OSTP staff would draw on the resources of AST for technical and analytic support, just as the Special Assistant for National Security and the NSC staff draw on the resources of State and Defense.

Second, the Administrator of AST would have a central policy and planning staff office as his or her principal arm for carrying out the agency's responsibilities for advice and leadership in science and technology in the Executive Branch. This staff would include a permanent cadre of persons qualified to provide leadership and good judgment in the handling of the policy and planning functions of the agency. The permanent staff would be augmented on an ad hoc basis, as needed, to deal with particular matters by temporary assignment of personnel from other parts of the agency, other Federal agencies, or the university or private sectors. The central policy and planning office would carry out on a broader basis many of the policy functions OSTP is now performing on a shoestring basis. It would take on the general planning and reporting functions in science and technology for the Executive Branch, including the five-year outlook and perhaps the annual report specified in the act reestablishing OSTP. In both policy and planning matters, the role of the office itself would be primarily leadership, guidance, coordination, and evaluation. Detailed studies, reviews, and planning would be assigned by contract or otherwise to qualified outside groups to the maximum extent feasible. The service functions of maintaining and improving the data bank of statistical information and other science and technology indicators would be transferred to AST and attached to the policy and planning office.

Third, the principal science and technology agencies now independent or not integral to the missions of other departments or agencies would be transferred to and become operating elements within the new agency. These would include as a minimum NSF, the National Aeronautics and Space Administration (NASA), the National Oceanic and Atmospheric Agency (NOAA), the National Bureau of Standards (NBS), and perhaps some general research and technology activities now lodged in the Department of Energy. The aim would not be to extract and consolidate all the science and technology activities of the Federal government into a single agency. The principal objective would be to provide Cabinet level leadership and advocacy for some independent agencies now outside the mainstreams of government and for some general science and technology agencies now submerged in departments with other primary interests.

A second important objective would be to provide the Administrator of the new AST a strong operating base of scientific and technological capabilities to support his or her roles in science and technology advice, policy, and planning in the Executive Branch. By being able to draw directly on the broad capabilities of the staffs, laboratories, and research and development centers under the control of NASA, NOAA, NSF, NBS, and perhaps others, the Administrator will possess capabilities for both quick reaction and in-depth responses to policy and planning needs in science and technology—capabilities well beyond those of a small OSTP in the Executive Office. The Administrator would also be in a position to deal authoritatively with management problems of the agencies put under his control, problems that are now left to be dealt with or to fester in the individual agencies or are handled sporadically and remotely by OMB or OSTP.

The fourth essential feature of the new agency is more difficult to define—its relationship to the science and technology activities of other Federal agencies. The Administrator of AST would not control activities or programs of other agencies. Defense R&D would remain the full responsibility of the Secretary of Defense, health R&D that of the Secretary of HEW, and so on. As the Cabinet level official for science and technology, however, the Administrator of AST would have functional leadership responsibilities for science and technology throughout the Executive Branch. His positions on science and technology would be generally similar to those of the Department of Defense with respect to defense-related activities of other agencies, the Department of State with respect to international activities, the Department of Labor with respect to labor-related activities, to mention only a few parallels. As in these cases, practical accommodations would have to be worked out. The guiding principles should be that the Administrator of AST should not be circumscribed on jurisdictional grounds from effectively discharging his general advisory, policy, and planning functions, but, at the same time, should not undertake to duplicate or interfere with the activities and responsibilities of the other agencies.

The Timeliness of Action

These, then, in broad outline, are the essential features of a new Agency for Science and Technology to institutionalize in a permanent and constructive way the functions of advice, policy, and planning for science and technology in the Executive Branch. The ideas it embodies are not new, but have not received much advocacy recently, either separately or in the combination suggested above.

The suggested role of the Administrator of AST and his central policy and planning staff is very similar to some of the original conceptions of the role NSF would play within the Federal government. These conceptions faded when the early NSF turned away from broader responsibilities to focus on basic research, especially in universities, and was eclipsed by the explosive growth of other Federal science and technology programs—in Defense, the National Institutes of Health, and later NASA. The notion of NSF as the "lead agency" for science was kept alive over the years by its friends in OMB. There have been attempts to broaden NSF with greater emphasis on applied research, and staff offices and support programs for general policy planning in science and technology have been established. This trend has been reinforced by numerous policy study assignments to NSF by OSTP and OMB.

The time has long passed, however, for recasting NSF into an effective central leadership agency for Federal interests in science and technology. Its focus on long-term academic research makes NSF too remote from current problems of operational concern that require prompt judgments and action, not just further research. NSF's "bag" is science, not technology; but the most crucial Federal policy concerns run primarily to technology, not to science. It can even be argued that efforts to make NSF something more than a foundation to support basic research may serve to detract from this vitally important function, by blurring the focus and by introducing extraneous criteria for research support. The concept proposed above is designed to provide a fresh start and new home for broad-based policy in science and technology while preserving and providing new Cabinet level leadership for the NSF in its primary role of supporting basic research.

The concept of the proposed new Agency for Science and Technology is also very similar in many respects to that proposed in H.R. 4461, one of several bills introduced in 1975 when Congress was considering science and technology policy legislation to establish advisory machinery in the Executive Office. Title III of this bill, which was very carefully conceived and deserved much more attention than it ever got, would have established a "Department of Research and Technology Operations." The composition and functions envisaged for the department correspond closely to those set forth above for the proposed AST and its administrator.[7]

H.R. 4461 was put forward by its sponsors, Congressmen Teague and Mosher, the ranking leaders of the Committee on Science and Technology, as a stimulus to discussion, not as a firm legislative proposal. Title III drew little or no support in the scientific and technical community, was opposed by the administration, and was assumed to be anathema to the agencies slated to be folded into the new department—although at least some leaders of those agencies are known to have recognized the advantages of merging their common interests to achieve a stronger position in the Executive Branch. In the legislative process that led to Public Law 94-282 reestablishing OSTP, the idea of reorganization along the lines of Title III was quietly dropped. All interest and pressure were focused on getting science and technology back in the White House; answers to other fundamental questions that might have delayed this objective were deferred. As we have seen, study of the organization of Federal science and technology activities was made the mission of the newly recreated President's Science Advisory Committee (PSAC) and later was supposed to become a function of President Carter's reorganization project when he abolished PSAC in 1977. There have been no signs of official interest since that time.

For the reasons suggested above, however, the idea of a major new central agency for science and technology ought to be revived. The time is ripe to move toward a stable, long-term pattern of advice and leadership in science and technology. With an able and effective Science Adviser in place, and having now gained a good appreciation of both the values and the problems of science and technology, the Carter administration is in an unusually favorable position to advance and secure acceptance of such a proposal. The President and his Science Adviser could leave no legacy of more lasting value from their stewardship of United States interests in science and technology.

References and Notes

1. For details see the reports prepared by Willis Shapley, Don Phillips, et al., *Research and Development in the Federal Budget: FY 1978* (1977), *Research and Development: AAAS Report III* (1978), and *Research and Development: AAAS Report IV* (Washington, D.C.: American Association for the Advancement of Science, 1979).

2. "Message to Congress on Science and Technology." *Weekly Compilation of Presidential Documents*. Vol. 15, No. 13 (March 27, 1979), p. 529.
3. Federal Council on Science and Technology. *Annual Report on the Federal Research and Development Program: Fiscal Year 1976.*
4. "The President's Message to the Congress Urging Approval of His 1977 Budget Request and Creation *of an Office of Science and Technology Policy*". *Weekly Compilation of Presidential Documents*. Vol. 12, No. 13 (Monday, March 29, 1976), p. 474.
5. "National Science and Technology Policy, Organization, and Priorities Act of 1976." *Public Law 94-282*, approved May 11, 1976.
6. National Science Foundation, *Science and Technology: Annual Report to the Congress* (Washington, D.C.: National Science Foundation, August 1978).
7. H.R. 4461, 94th Congress, 1st Session. See also Committee on Science and Technology U.S. House of Representatives, *A Proposed National Science Policy and Organization Act of 1975* (Washington, D.C.: Committee Print, 1975); and Committee on Commerce, Science, and Transportation and Committee on Human Resources, U.S. Senate, *A Legislative History of the National Science and Technology Policy, Organization, and Priorities Act of 1976* (Washington, D.C.: Committee Print, April 1977), pp. 273-320.

Presidential Politics and Science Policy

foreword by
Irving Louis Horowitz

LIBRARY OF CONGRESS
6
AUG 10 1981
Congressional Research Serv.
MC 3166

CHAPTER 5

NATIONAL SCIENCE POLICY AND THE OFFICE OF MANAGEMENT AND BUDGET

The specific science bureaucracies discussed in the previous two chapters are not the only presidential agencies to exercise control over science policy. The central instrument of presidential power over any area of spending is the Office of Management and Budget (OMB), formerly the Bureau of the Budget (BOB). This organization is directly responsible to the president for the enforcement of his wishes in the executive branch, the preparation of the U.S. government budget, as well as serving as the clearinghouse for the executive branch's legislative proposals and the overseer of executive-branch-congressional relations (Gilmour 1971). Since the OMB is vital in implementing and enforcing presidential policies, it is consequently highly important as a lever of presidential power over science. Yet the OMB's role in science policy has never been systematically delineated. This chapter is an analysis of the authority of the OMB over science policy: when, why, how, and by whom is the OMB's power brought to bear on the scientific research enterprise? The OMB is not alone in this area, sharing responsibility with the National Security Council (NSC), with White House staff, and especially the Domestic Council on science-policy issues. But the OMB is more distinctly and systematically involved in the process of science-policy construction, implementation, and evaluation than the rest of the presidential offices.

THE OMB'S POWER

Perspective

Before examining the OMB's influence on science policy it is necessary to explain the major outlines of the federal budget-making process. The budget is a unidimensional measurement of the goals and programs (as well as the compromises) of the federal government. Nowhere else are the government's aims and programs (and the division of spoils) more clearly reflected than in "who gets what," that is, the budget. Each budget cycle is a twelve-month interval called a fiscal year (FY), beginning on July 1 of each preceding calendar year. For example, FY 1966 begins July 1, 1965, and ends June 31, 1966. Beginning in 1977, under the Budget Reform Act of 1974, Public Law 94-344, the date for the beginning of the FY has been changed to October 1 of the preceding calendar year. The budget preparation begins over two years in advance of its actual implementation. Thus the preparation of the FY 1978 budget was well under way by the early spring of 1976. The line departments and agencies, such as the Departments of Defense (DOD) and Health, Education, and Welfare (HEW) presented their estimates of needed budgets to OMB at a formal review process in the spring of 1976 and this review continued throughout the summer. The review process is characterized by OMB budget examiners asking the bureaucratic offices to justify and rationalize their budget estimates in view of the designated mission of that office and the president's own priorities. In line with good management practices and the presidential agenda, the budget examiners scrutinize proposed agency spending. New or expanding programs receive the closest attention, usually with an eye toward reducing (but infrequently increasing) these programs in line with priorities, and controlling spending is invariably one of these. Much interaction and negotiation takes place between the OMB examiners and agency personnel seeking to defend the planned expenditures. (For a transactional analysis of this process see Wade 1965; Davis and Ripley 1967.)

In the fall the director's review takes place with the OMB position established by November. At this juncture the assistant directors and the OMB director thrash out unresolved differences between the OMB and the agency positions. If the agency staff do not feel they got satisfaction during these reviews, they can appeal the decision to higher levels in the White House and even to the president himself. The OMB usually joins in battle at these higher-level meetings by presenting a rationale for the steps it took. The judgments at these higher levels are often, thought by no means exclusively, in the OMB's favor. Revisions in the budget continually take place right up to the moment the budget is set in final type in January. Then in January the budget is presented to Congress by the president.

The congressional committees that appropriate money are the OMB's most formidable opponents. These committees have tremendous sway over the "pursestrings" of the treasury and have many times taken the OMB to task for scuttling programs that a committee favored. When agencies become aligned with their respective appropriations committee, congressional action (or inaction) is usually enough to override the OMB. During hearings, an agency head may be asked by a congressman (often in collusion), "What would you be able to do if your budget were twice as big?" To the OMB's chagrin, the agency representative is then usually only too happy to respond to the solicitous congressman. While such intimate pairings of executive agency and congressional interest are relatively infrequent in science politics (notably so when compared to military affairs), they do occur. When the individuals in charge of the relevant committees are powerful, they can generally deflect the spending plans of the OMB and the president (Wildavsky 1974; Fenno 1973). The most pointed example of this process has been the biomedical-sciences research during the 1950s and 1960s, a case explored later in the book.

The power of the committees has been diminished by the Budget Reform Act of 1974. This law empowered a congressional budget office to supervise the composite budget of the entire federal government. This office works to get all the various appropriating committees working together to produce a budget which meets size limitations previously agreed upon. Thus the autonomy of a committee is undercut by the necessity of working within a delimited margin. While it is too early to understand the secondary consequences of the 1974 act, quite clearly it limits the power of the committees and their potent chairmen. This in turn strengthens the relative position of the OMB since its decisions have already been justified in terms of the total federal budget. This will tend to curb "end runs" around the OMB by agencies seeking the succors of its congressional oversight committee.

Conventional wisdom dictates, then, that once a budget is set by Congress, the president has little recourse but to go along with the measure, or veto it and hope that if his veto is sustained, Congress will produce a more acceptable funding proposal. This, as will be seen later in the chapter, is quite clearly not the case.

(In the forthcoming analysis, the OMB appears to be a highly rational organization, but clearly this rationality can be overstated. Like any organization, the OMB is subject to the pressures of its contextual environment and perturbations in its internal operation. The same conflicts which characterize every organization's internal operation [role conflict, information distortion, short-term goal displacement, and so on] as well as its external circumstances [special-interest pressure, perception of societal needs, institutional competition, and so on] also affect the OMB. While these factors should be borne in mind, they are not the focus of this analysis. Instead this chapter is aimed at demonstrating how the level of presidential power, the OMB, has been and is being used to serve the president in fostering his goals for American society through science policy.)

The Nature of the OMB

Only one agency in the federal bureaucracy has management-supervision responsibility for all government-sponsored research operations, operations that now total over seven billion dollars per annum. That one agency is the OMB. Besides the OMB, relevant congressional committees may evaluate scientific research or policy through special inquiries, or by the appropriations process. The National Science Foundation (NSF) performs a monitoring and advisory function, and some individual executive departments and agencies do exert significant influence over particular aspects of research policy. The office of Science and Technology Policy (OSTP) created in the Executive Office of the President (EOP) under 1976 legislation, also supports the OMB through policy analysis and program assessment. It is through the OMB, however, that the general expression of the leadership of the president is translated into specific science policy on a broad-based operational basis. Hence, the OMB plays a pivotal role in the formulation and conduct of national science policy.

Typically, studies focusing on the creation of national science policy examine a particular agency or issue, or probe the diffuse forces shaping science policy. When these discussions touch on the OMB, it is usually as either the nemesis or the blackbox. The OMB, as the president's fiscal management arm, is viewed as primarily responsible for terminating, or at best, limiting scientific programs. This article will try to penetrate this stereotypic, unidimensional image of the OMB by analyzing its role and its limits in shaping national science policy.

The OMB (known, as we have noted, as the Bureau of the Budget, or BOB, until it was reorganized in 1970) serves as permanent career staff to the presidency although the upper echelons change with each president. Henry Jackson's Subcommittee on Policymaking at the Presidential level noted the pivotal role of the OMB when it stated that, traditionally, "the President regards this Bureau as his strong right arm in executive management, serving as the President's 'lengthened shadow' across the whole front of fiscal policy and program management" (Jackson 1965:55). The OMB provides direct staff assistance to the president on the entire range of federal activity. It is within the EOP, of which the OMB is a crucial part, that all controversial presidential policy issues must be resolved (Bureau of the Budget 1965b:2). According to the Jackson subcommittee, the OMB director sits on the innermost policy councils of the presidency, and, after the president, the "Budget Director has the most comprehensive view of the policies and programs of the government" (Jackson 1965: 57).

The 1970 transmutation of the BOB into the OMB reflects the agency's steadily increasing participation in the management of government. In addition, the new title suggests the powers the agency has more recently acquired. While continuing to play the same key roles (preparing the budget, legislative supervision, and fiscal operations), the OMB has had its management capabilities strengthened in order to improve the effectiveness of ongoing programs, especially those which cross agency lines (House Committee on Government Operation 1970:70).

Philip S. Hughes, a deputy director of the BOB from 1948 to 1969, has testified that prior to the 1970 reorganization, there was a gradual shift in the BOB's role, from that of simply assuring that governmental functions were handled in an economical way to that of evaluating government priorities and ordering achievement of those priorities through management supervision (Senate Government Operations Committee 1969:327). That the OMB has sedulously pursued the presidents' interests is reflected in former President Nixon's 1970 comment transmitting the OMB reorganization plans to Congress. "Resistance to organizational change," Nixon asserted, "is one of the chief obstacles to effective government." The OMB, therefore, was to monitor and evaluate organization and management programs on a regular basis to overcome this resistance. On a later occasion, Nixon commented that the OMB would help "streamline" and make governmental practices more effective. The OMB was to "finally bring real business management at the very highest level into the executive branch of the Government" (*Weekly Compilation of Presidential Documents* 15 June 1970).

The OMB has achieved considerable power as it has sought to ensure that government operations be carried out efficiently. While it is true that much of this power is negative (that is, curtailing programs and limiting budgets), the OMB can also use positive inducements such as *giving* an agency additional manpower, money, favorable policy decisions, and moral support.

OMB CONTROL OVER SCIENCE POLICY

Science Policy and the OMB's Organization

The OMB is organized into divisions. Although some responsibility for science policy and programs resides within other divisions, the broadest responsibility for dealing with matters of science policy lies with the Division of Science, Space, and Energy Technology.

The OMB does not, nor could it possibly, review science policy and programming in their entirety. No single federal agency could keep track of the diverse projects, equipment, manpower, and facilities involved in federal research and development (R&D). The OMB does, however, single out for scrutiny policies and programs which appear to have significance under the president's priorities. In this context, "significance" means research with putative negative or positive payoffs for presidential power, influence, and prestige or support positions expressed in presidential statements and directives.

One of the areas upon which OMB attention is specifically focused is federal legislation for science. In dealings with Congress, OMB officials (and their predecessors in the BOB) have enunciated or promulgated the administration's position on such issues as the Atomic Energy Commission (AEC); establishment of the NSF (House Interstate and Foreign Commerce Committee 1949:39-44); and in the late 1950s, in the structuring of the National Aeronautics and Space Administration (NASA) (Schoettle 1966); the feasibility of a federal department of science in the early 1960s; the proposal to set up a national foundation for the social sciences in the mid-1960s (Senate Government Operations Committee 967); and, more recently, the Teague bill proposing centralization of science-policy-making powers (House Committee on Science and Astronautics 1973:104-32).

In fact, executive-branch positions on science-policy questions are usually handled by the OMB. For example, the administration response to a 1973 congressional inquiry into the sufficiency of executive planning and coordination efforts for science and technology was prepared under direction of the OMB (House Committee on Science and Astronautics 1973). Other recent questions include determining the "level of effort" advisable for solar energy research, the desirability of creating a new presidential science adviser's office and a department of energy, as well as the level of automotive R&D support.

The OMB is generally the executive spokesman whenever the Congress challenges the administration position on politically charged science-policy issues. This was exemplified when, in 1966, the BOB played a decisive role in the controversy by defending the federal government's decision to use geographic distribution as the method for allocating R&D funds (Carey 1968:149-50). Additionally, the OMB acts as a filter for information being transmitted from the executive to the legislative. Prior to giving congressional testimony all executive-branch employees, including officials with science-policy responsibilities, must submit their statements to the OMB for review and approval. Bitter conflicts often ensue over what can be and is said during congressional testimony. The testimony itself is often "massaged" after its presentation until the "proper" viewpoints are expressed.

The organization and management of scientific and technological undertakings within the federal bureaucracy are also subject to OMB scrutiny. The OMB has grappled with such questions as how to structure a new agency for astronautics and space research; which offices should be combined to form the Environmental Science Services Administration; and whether to create the National Oceanographic and Atmospheric Agency (NOAA). Problems of interdepartmental coordination have also engaged the OMB's attention. In one instance, the OMB resolved a conflict between NASA and the Air Force over manned space flight (Logsdon 1970:30). In another, the OMB also played a major role in dividing the responsibility for construction of university reactors used for research between the AEC and the NSF, and in jurisdictional questions concerning the proper assignment of obligations for setting tolerances for exposure to radiation (Carey 1968). The recent prominence of energy research has also prompted OMB to resolve disputes between the Energy Research and Development Administration (ERDA) and NSF on basic research responsibilities and between NASA and the Interior Department's Office of Coal Research on coal R&D.

Policy regarding the procedures involving science research, grant management, and budgeting guidelines are also under the OMB's authority. The OMB has a sizable influence in creating guidelines for allocating indirect costs to research grants; developing principles of cost sharing for grants to universities; and establishing common systems for auditing grants dispensed by various federal departments.

Thus far the focus has been on the structure and operations of the OMB, but individuals who comprise the OMB and their relationships with other agency personnel give the OMB its character. The OMB staff, broadly speaking is a generalist staff composed of economists, accountants, political scientists, lawyers, and individuals from a host of other disciplines. There are also many scientists and engineers scattered through the OMB, yet the OMB is known for the critical skills of its examiners rather than the cultivation of the expertise of any particular discipline. This was pointed up by one official in charge of the OMB's government organization branch who said, "There's been a continuing debate as to whether the budget bureau should develop a specialized staff. The prevalent opinion still holds that the bureau should retain general rather than technical or special skills" (Barfield 1973:465). In fact the OMB in its recruitment policies discriminates against people with prior background in a prospective examining field, fearing that familiarity breeds biases. During their tenure OMB staff with science-policy responsibility have accumulated considerable sophistication in the disciplines that impinge on their bailiwick. This sophistication permits the OMB staff to perceive the broad outlines in matters involving science policy without having to match their understanding of specific information with agency personnel. BOB examiners "acquire an uncanny 'nose' for the important issues without really being able to argue the technical merits" (Brooks 1968: 310). This is exemplified in atomic energy R&D questions, where the president has traditionally leaned heavily on the OMB for advice.

Atomic energy R&D has always been among OMB's strongest areas of interest. Immediately following World War II, BOB undertook responsibility to convert the Manhattan District project, which constructed the atomic bomb, into the AEC. "During the Truman years, every policy statement that Truman made concerning atomic energy was drafted in the Budget Bureau," stated a former BOB staff member. The BOB attained its authority in this area because atomic energy matters were of great importance to the president: among the presidential offices only the BOB had the necessary skills to develop in this field. Truman needed some close staff work from people he could trust, "so he turned invariably to the Budget Bureau for Policy," said the BOB staff member.

Several career staff members in the budget bureau have been favorably disposed toward scientific research, believing that science has both cultural and practical benefits which are not immediately apprehensible. As a result, the BOB sheltered basic research during the lean years of funding. Said William Carey, a former assistant director of BOB, "We felt that basic research could not survive in open competition for money" (*Science* 18 January 1974:182). To an extent, this protectiveness eroded the position of science supporters within the OMB since, whenever they advocated a pro-science position, their advocacy was immediately discounted since they were perceived as a "lobbyer" for basic science rather than a disinterested seeker of presidential interests. This tended to aggravate the problems of securing support for science, since valid arguments would not be distinguished.

The budget examiners' orientation toward their respective bureaucratic agencies spans the entire spectrum from advocacy through neutrality to hostility. From the OMB's perspective the most preferable orientation is neutrality. The relationship between OMB and an agency continually fluctuates. The agencies' orientation runs the gamut, from trying to provide the OMB with as much information as possible on the assumption that the OMB will act more favorably if it has a more adequate depiction of the problems facing an agency to trying to give as little information as possible, hoping that the less the OMB knows, the less damage it will be able to inflict. In an attempt to build larger programs and budgets an agency will try to give the OMB what the agency thinks the OMB wants to hear, that is, conform to the anticipated OMB priorities. The personalities of the various actors themselves play a large role, tempering not only the OMB-agency relationship but also affecting a particular agency's ability to override an OMB decision. As has been mentioned, an administrator may make an appeal around the OMB to a congressional committee. Alternately, the administrator may appeal to the president or his top staff. For example, Glenn Seaborg, scientist-chairman of the AEC had great influence with Johnson, as Lewis Strauss, an earlier AEC chairman, had had influence with Eisenhower. One BOB high-level staff member hyperbolized, "I think if Glenn had wanted to put a reactor on top of the White House LBJ would have said OK" (*Science* 18 January 1974:182).

The concern of the Division of Science, Space and Energy Technology about the government science policy's effect on the scientific community is quite real, although it represents only a small portion of the activities. This concern is evident in the OMB's efforts to (1) maintain a balance between various disciplines and styles of scientific research; (2) ensure comprehensiveness of programs activities, to guard that no lacunae develop in important areas and also prevent overlap between agencies' research activities; (3) evaluate research, both in terms of adequacy of research funding and quality of research effort in the laboratory; and (4) attempt to maintain a high level of significance in government-supported research.

Research areas an agency considers important, and hence funds, influence the content and direction of the scientific disciplines receiving funds from that agency (cf. Useem 1976). The OMB strives to minimize the disruption in an affected field as the priorities of a federal agency shift away from that particular field. The OMB tries to provide balance by using another agency, often the NSF, to take up some of the slack when funding in one research area is reduced. This is done in order to preserve the earlier federal investment in the passed-over research area. For example, the DOD has withdrawn most of its support for basic research, due to the 1968 Mansfield amendment to defense appropriations, cutting temporarily DOD-sponsored pure research as well as other contingencies, such as concern about perverting the purpose of universities. The Mansfield amendment did not attack basic research per se. Rather it was designed to limit unwarranted DOD participation in scientific research. As part of its science-policy responsibility the OMB sought to limit the immediate shocks of this policy decision. The OMB assumed a "stewardship" by increasing the NSF's support for basic research to offset this loss.

The OMB has performed a similar function in trying to keep the relative emphasis of separate disciplines in equilibrium. There has been little attempt to seek a dollar-for-dollar trade-off in various disciplines as federal funding shifts; instead the OMB aims at preventing any discipline from falling too "far behind" its sister disciplines. Though the OMB acts as a "broker" in the competition for funds between particular disciplines (for example, between chemistry and high-energy physics), there is no clear pattern to suggest whether the OMB actually initiates brokerage activities or only responds to the entreaties of special-interest groups. It is generally safe to say that there exists a somewhat cooperative relationship in which the OMB and the scientific community work together in order to make an adequate and objective assessment of the merits of each discipline's case.

Any scientific discipline can absorb more money, and any research problem can expand at any given time. Hence the simple yardstick of "contribution to man's wealth of knowledge" is not an adequate criterion by which to evaluate the division of available research money among disciplines. A more explicit and open system of justifying the "pie-slicing" operation is needed than is presently available.

In fulfilling the brokerage role, the OMB has become responsive to the persuasive efforts of the scientific community, accepting several of its assessments of the relative contribution of particular scientific disciplines. As part of the "neutral competence" in its decisions, the OMB encourages the scientific community's leaders to analyze, in a hard-nosed fashion, any particular research field's contribution to the national welfare. This type of analysis is often quite difficult, since almost by definition, the material payoffs of many types of basic research are not immediately perceptible. Further, "the national interest" is a slippery concept both in the specific and in the abstract. The problem becomes further aggravated by the fact that some national objectives themselves shift rapidly.

In order to resolve this problem the OMB encourages various assessments. These assessments often take the form of reports, although meetings with representatives of disciplines do take place. The National Academy of Sciences (NAS), through its Committee on Science and Public Policy (COSPUP), has been particularly prolific (cf. Boffey 1975). An example of such a report is *Chemistry: Opportunities and Needs*, (NAS 1964) which was explicitly created to boost funding levels for chemistry (Greenberg 1968). A 1968 study, *Economic Benefits from Oceanographic Research* (NAS 1968) measured the value of attaining objectives from a cost/benefit perspective. COSPUP's Greenstein panel on astronomy clarified outstanding issues which convinced the OMB to permit work to move forward on the Very Large Array radio telescope. Another report, had a direct impact on policy decisions in high-energy physics; as a result of this study and other assessments, the OMB challenged continued operation of the Princeton-Pennsylvania accelerator, which was subsequently shut down.

Personal contact between the scientific elite and middle- and upper-level staff of the OMB, once infrequent, has been increasing. At times, members of the Division of Science, Space, and Energy Technology meet with groups representing a scientific discipline to discuss a group's special research problems vis-a-vis utility, funding, and manpower. Inspection and field trips to universities and installations help sensitize OMB decision makers to the real-world situations facing the scientific community, and reduce the decision makers' dependence upon secondary and mediated information. The trips are too infrequent, however, to circumvent the distortion of the real "state of the art" in research across the nation. Representatives of prestigious scientific organizations are tapped for their input, both in assigning priorities within their own disciplines needs and in designing cross-disciplinary research. Robert Green of the NAS plays this advocacy role for the physical sciences with great effectiveness. Henry Riecken served in a similar capacity for the social sciences when he worked for the Social Science Research Council (SSRC) but since his move to academic life, no SSRC official has emerged to replace him. (There is a noticeable lack of a social-science advocate in the federal establishment.)

The relationship between the OMB and the scientific community cannot be characterized as "hand in glove." The OMB has always played devil's advocate, insisting that hard and critical questions be asked. Paramount among OMB concerns is that the scientist's self-service be minimized. As long as overt lobbying is absent from the scientific community's policy recommendations, the OMB will continue to utilize its input. Mutual respect and confidence have developed over time and the informed insights of the scientific elite are valued by the upper echelons of the OMB although there is continuing suspicion on OMB's part that self-serving ideas will slip or be pushed in.

A second area of OMB concern is comprehensiveness, that is, the OMB's interest that an important area or problem is not ignored simply because it is not under the authority of any particular governmental agency. An equally (and sometimes more) important aspect of comprehensiveness is the elimination of redundant research and overlapping agency-research responsibility. The OMB strives toward agency coordination to root out imbricating agency research responsibilities and projects as well as supervising agencies so that scientific inquiries which would otherwise "fall between the cracks" of the organizational structure are dealt with adequately. The OMB makes an active monitoring effort so that gaps in any significant research question are filled and no major opportunities or potential problems are overlooked simply because no agency has clear-cut responsibility for them. Long-term climatic variation and forecasting is one such area. When scientists exposed the threat of ozone-layer depletion caused by aerosol-propellant pollution in the upper atmosphere, the OMB convened the Interagency Task Force on the Inadvertent Modification of the Stratosphere to investigate the problem and advance recommendations. Included in the task force were NOAA, NASA, and the Environmental Protection Agency.

Another aspect of interagency coordination is research installation management, which deals with such specific concerns as the procedures governing the transfer of research facilities from one department to another. The OMB's role in this process was discussed in great detail in the 1972 General Accounting Office (GAO) study issued in the aftermath of the conversion of Fort Detrick in Maryland from a biological-warfare research installation to a cancer laboratory. As the United States reduced its commitment to chemical and biological warfare in the late 1960s, Fort Detrick's original mission gradually became obsolete. The DOD which has supported the facility, attempted to find another department to sustain the large physical plant and research work force. Finally, under OMB supervision, the National Institutes of Health (NIH) took over Fort Detrick as a cancer-research unit, but not before a sharp reduction in the number of professional researchers took place. The GAO criticized the protracted and ambiguous changeover negotiations, which had been supervised by the OMB, and recommended that the OMB assume a more vigorous role in future negotiations of this type. The GAO believed the OMB should strive aggressively toward becoming a "single coordination point" for the supervision of government research facilities (GAO 1973:24). In response to that report, the President's Advisory Council for Management Improvement recommended that the OMB should become more greatly involved in overseeing the operation of federal R&D facilities (General Accounting Office 1972).

OMB's third area of concern includes making sure that agencies deal adequately with all scientific research issues within their purview and ferreting out the weaknesses and gaps in agency research. The earlier reference to "comprehensiveness" describes the OMB's concern with research coverage between agencies. "Adequacy" in turn is concerned with the internal activities of agencies. The OMB uses several yardsticks in the determination of adequacy of agency-supported research. One yardstick of the national interest is what the president himself sees as important. The second yardstick occurs when the OMB itself takes an active interest in agency research it deems of direct national concern. A third, which causes OMB intervention, is when it decides that an agency is being hamstrung by the bureaucratic superstructure's interfering with agency adequacy. A fourth is when an agency has not aggressively pursued funding in a vital area. In the area of nonnuclear energy, the Department of the Interior, traditionally a very independent department, maintained a research arm largely unresponsive to both government and peer criticisms of the quality and character of its research. With the onslaught of the energy crisis, the OMB dramatically increased the funding level of the Interior's Office of Coal Research (OCR), hoping to spur research into this alternate source of energy. The OCR's budget shot from $20 million to $295 million between fiscal years 1972 and 1975. With the sudden increase in allocations, however, the OCR was swamped with proposals and the office's contracting mechanism became paralyzed. The OMB convinced NASA to loan some of their proposal-evaluation experts to the OCR in order to get the agency moving on coal-related research.

There is continual pressure, both inside and outside government, to exploit breakthroughs in basic science for "spin-offs" and practical applications for the larger society. The OMB is not immune to this pressure. It recognizes the political benefits for the president, and has encouraged the NSF to direct research monies into solutions of major national problems. With OMB support, the NSF launched the Interdisciplinary Research Relevant to Problems of Our Society programs in 1969, later reborn as Research Applied to National Needs (RANN) in 1971 with funding of $13 million. For fiscal 1976, RANN was authorized at the level of $71.5 million. The RANN program exemplifies the OMB's ability to shape agency programs to perceived national interests.

The final concern of OMB's science division is to enhance significance in scientific research. The executive branch has been under increasing pressure about this in recent years because of the development of an antiscientific backlash in Congress resulting from efforts by Senator William Proxmire (D.-Wisc.), among others. One of the agency's central goals has been to ensure that research, carried out through the use of government funds, conforms to the canons of scientific method, including use of valid data-gathering techniques. A further and perhaps more important goal, is to see to the pursuit of questions which stand to make an important contribution to the theoretical development and basic understanding of the scientific discipline involved. The OMB rarely involves itself with the internal methodologies and arguments of scientific research, but it is concerned that the research undertaken does have a redeeming value and is not simply wasting funds on "useless" research or vitiated by inadequate research techniques. (Some social-science and education questionnaires have been rejected on these grounds.)

Budgeting and the Research Process

The OMB achieves its most significant impact over science policy by setting federal budget priorities for science. The major portion of science-division manpower is devoted to the program-evaluation-and-budgeting process. And the OMB uses its review power over R&D budgets to ensure that agencies are responsive to presidential priorities. As noted previously, OMB's power of the purse is usually exerted negatively, that is, by reducing an agency's requested level of funding. Some of this reduction occurs because agencies inflate their submissions to begin with in the expectation of their requests' being pared back. Carey, a longtime participant in and observer of the OMB, has characterized the OMB involvement in budget making and evaluation as a "conscious design to give the President assurance that the government's investment is balanced and scaled both to policy objectives and to what the budget can stand." The OMB's chief role is that of rationing budgetary resources–an integral part of policy making (Carey 1968:150).

The OMB's policy-setting role for science is most prominent as it parcels out the government's finite fiscal resources, that is, producing the budget of the United States Government. The OMB's overriding aim is to harmonize the "national interest," as defined by the president and the White House, with the available budgetary resources, either through undertaking a leadership role as described previously, or by the more usual procedure of examining and "modifying" (i.e., usually cutting) the budgets and programs of ongoing scientific projects. Both approaches constitute policy setting (cf. Shapley 1976).

Specific programs and general content areas are probed by OMB examiners, who are assigned specific responsibility. Examiners report to their respective division directors and to the leadership of the Division of Science, Space and Energy Technology. The allocation and spending of research funds are scrutinized by these examiners. The examiners then inspect both the ongoing and proposed priorities as well as the research programs of individual agencies. The OMB examination is by no means an accountant's exercise in numbers and decimals. Rather, such scrutiny "becomes a penetrating examination of the content and design of scientific and technical programs, with program examiners putting the questions and the departmental-witnesses providing what they hope are the answers, sometimes successfully, sometimes not" (Carey 1968:150). The OMB does not generally scrutinize scientific research on a project-by-project basis. Instead, the emphasis is on a selective process of identifying and resolving critical questions. A "slice" of basic science such as high-energy physics or oceanography may, however, by assayed for that discipline in all the involved agencies.

Examiners themselves have tremendous responsibility and power to decide what and what not to present in defense of an agency request. The examiner plays a dual, and often conflicting, role being both an agency advocate and simultaneously an agency opponent; the examiner is constantly being called upon to attack agency plans and then defend those plans to the examiner's superiors. In the director's review, roles of advocate, critic, and impartial judge are combined. The resulting conflict puts the examiner in an untenable position which rarely produces the best, most balanced results.

A fundamental problem in the examining process is that the examiner possesses formidable responsibility without concomitant accountability. There are few incentives for excellence and disincentives for poor work motivating the examiners. This largely stems from the lack of accountability for work done inside the OMB. One factor reducing accountability is the political and bureaucratic layering isolating examiners from final decisions in their program areas. Often the individual with the most information, the examiner is usually furthest removed from the final decision which takes place in the upper reaches of the OMB and the White House. There is a strict line of command and a narrow flow of information; to go outside this hierarchy invites swift censure. This is because along with power, information is one of the most important and jealously guarded assets in Washington. Subordinates discovered cultivating outside sources of information are dressed down by their superiors at the OMB; no one wants to be outmaneuvered or embarrassed at a critical point. Frantic efforts to uncover the source of an information "leak" in the OMB (as well as other agencies) point this up. When the culprit who let the information slip is identified, at times the result of a full internal investigation, he simply disappears from the OMB. So long as the examiner avoids this faux pas (or other major slips, such as being won over or "captured" by a subject agency), the examiner has little to fear.

The lack of familiarity with an agency's substantive programs and the lack of accountability breed a rashness and cockiness on the part of the examiner that infuriates and dismays bureaucrats and science advisers of all stripes. The original or thoughtful bureaucrat often gives up trying to get innovative programs accepted after they have been beaten down year after year; sometimes, however, these programs can be disguised in ones already acceptable to the OMB. The OMB strikes back through "ratchet exercises" which begin by trimming the federal budget back by gross amounts (often in the billions). Each agency and most programs (and especially the big ones) are somewhat arbitrarily cut back to meet the lowered ceiling. A further illustration of arbitrariness occurs during the preparation of the final budget. Right up until the last minute there is extensive rewriting and modification in the federal budget. On occasion the program budgets undershoot or overshoot the desired totals. In these cases the arithmetic problem is corrected by randomly adding or subtracting funds to a program until the budget adds up to the desired total.

The lack of accountability is also reflected on the interpersonal level. One examiner who worked at the OMB for two years never experienced or even heard about another examiner experiencing reward or encouragement for a good recommendation or a reprimand for a poor recommendation; in fact there is almost no feedback at all. Responsibility is diffused to the entire OMB; blame for any missteps are "corporate," assigned to the OMB as a whole and never attributed to an individual. There is not even personal accountability to the examined agencies. When an examiner is asked by an agency why a particular program was cut, he is instructed to answer, "It was the president's decision—ask him!"

Helping insulate an examiner from both his superiors and agencies is the institutionalized skepticism and extreme suspicion about any advocacy shown for a program. The burden of proof is on whoever wants to add or change a program. These sanctions against any growth or modification are usually quite stringent. The exception to this are areas of presidential concern; if an area is explicitly earmarked "presidential priority," such as energy or cancer, the opposite occurs. Many low quality programs are immediately approved and the search goes out for programs which can be fitted to these priorities. To identify the president's priorities, the OMB staff pore over presidential papers, memos, and other documents trying to divine hints and suggestions of the president's position and take action on the basis of this information. Also phone calls and directives from the president's or vice-president's office spur the OMB to swift action on a particular issue.

Being a political institution, OMB decisions are generally skewed to the path of least political resistance, thus the importance of White House guidance. But questions about congressional sensitivities and sensibilities are also significant; concern is expressed about whose district a program is in, who on Capitol Hill is supporting and who is opposing a particular program. On important issues, a "political impact" statement is prepared. In this context, the White House Domestic Council has played the role of arbiter between Congress and the OMB.

These reviews and examinations have an impressive and dramatic impact on the budget, both government-wide and in the science-and-technology-related agencies. Tables 1 to 6 show how the OMB has influenced the budgets of particular agencies involved in scientific research. Although the figures are neither all inclusive nor highly detailed, they do highlight the significant influence of the OMB upon the budgetary process. The tables reflect the cuts (and occasionally boosts) in spending requests imposed by the OMB on various agencies with large scientific research components. Congress's appropriation to an agency is often different from the OMB's allowance; yet the change in the budgets demonstrate that the magnitude of OMB's modifications equals and at times exceeds Congress's. These figures do not reveal a monotonic trend since budgetary, developmental, and personality features vary each year. The central point, however, one that is clearly revealed in these tables, is the striking impact of the OMB on agency research budgets. These tables underscore the OMB's ability to give and, especially, to take away money; this ability rivals and frequently transcends that of Congress. (For a general discussion of OMB budget setting, see Wildavsky 1974; Barfield 1970; Havemann 1974; Wade 1975.)

Tables 1, 2, and 3 deal with the AEC (now ERDA). Table 1 shows BOB influence on certain research programs of that agency for FY 1967 and FY 1968; in that period BOB sliced AEC requests by 35 percent and more, and several were dropped altogether. Table 2, showing ERDA's basic and applied R&D funding from FY 1973 to 1977 again demonstrates the significant "modification" inflicted on agency budgets by the OMB. Table 3 reveals the changes the OMB has inflicted on ERDA's budgets throughout its history as well as the final amounts budgeted by Congress. Table 4 presents similar statistics on NIH appropriations, FY 1962 to 1977. Table 5 covers FY 1959 to 1977; during this time OMB has acted as a powerful brake on NASA plans for expansion. Interestingly, while the OMB did reduce NSF's budget requests, OMB tended to be more generous to NSF than Congress (Table 6).

A controversial but useful OMB budget-management tool is impoundment, euphemistically called "budget reserves." Impoundment takes place when the OMB bars an agency from spending certain money appropriated for it by Congress. During the Nixon administration, impoundment was used to a greater extent than under previous presidents. The active use of impoundment of funds intended for scientific research is reflected in Table 7, which lists the amount of funds impounded from the NSF.

Increasing Congressional displeasure with impoundment as well as reverses in lawsuits involving impoundment led to limitations on the OMB impoundment stricture. On June 28, 1974, OMB Director Roy Ash declared that the OMB would no longer impound funds. The Congress, already incensed with OMB impoundments, however, gutted OMB's impoundment power. The Impoundment Control Act, part of the Budget Reform Act of 1974, established a reporting structure wherein all impoundments must be reported to Congress within 45 days. There are two types of impoundment, and a careful distinction must be made between them. The first type are deferrals postponing or delaying an agency's authority to spend money. These deferrals mean that the OMB is permitting the money to be spent but is withholding the money for the period immediately at hand (usually a quarter, but funds may be deferred up to the end of the current FY). The OMB may renew the deferral each FY for as long as it wishes. The leverage of deferral is sharply undercut by the fact that a deferral, in addition to being temporary, can be voted by the passing of a resolution by either house of Congress. In contrast to deferrals are recisions. Recisions are infrequent since to use this power, the administration must gain the approval of both houses of Congress. Unquestionably the Congress has made severe inroads in the OMB's use of impoundment.

The president and his executive officers have many more discretions in fiscal power beyond setting the budget and controlling its expenditure through impoundment. Louis Fisher details the many paths of presidential power in this area. Lump-sum appropriations (rather than specification of the exact purpose of the expenditure), transfer authority between classes (taking funds applied for one class of appropriations and applying them to another), reprogramming

TABLE 1

Selected AEC Research Projects Approval, FY 1967-68
(thousands of dollars)

Project	Budget to BOB	BOB Allowance	Congressional Appropriation
FY 1967			
Accelerator improvements, Zero Gradient Synchrotron, Argonne National Laboratory	3,200	2,000	2,000
Accelerator improvements, Brookhaven National Laboratory	1,300	800	800
Bubble Chamber for Brookhaven	16,300	0	0
200-Bev Accelerator (design)	7,500	0	0
Addition to cyclotron building at Lawrence Rad. Laboratory	1,200	0	0
Biological research labs, Oak Ridge National Laboratory	7,600	0	0
FY 1968			
Accelerator improvements, Zero Gradient Synchrotron, Argonne National Laboratory	2,100	1,900	1,900
Accelerator improvements, Cambridge and Princeton	1,000	400	400
Technical services buildings, Brookhaven National Laboratory	850	0	0

Source: Scientific Research July 1967: 31; congressional-appropriation column added by Energy Research and Development Administration.

(shifting funds within an appropriation item), transfers in time (no year specification on the funds), and even making commitments unauthorized by Congress all give the president wide latitude in controlling the expenditure and purpose of funds. The president's budgetary control over science policy extends far beyond the setting the budget and winning its approval from Congress. No matter what the final form of the budget approved by Congress is, the president and his top officers can radically alter spending patterns by availing themselves of presidential spending power (Fisher 1975).

The budget examiner, the budget review, and impoundment are all tools the OMB uses to exercise control over executive agencies. While the OMB usually can justify its use of such devices, it is not ordinarily called upon to do so, since the burden of justifying a request for funds rests upon the agency

TABLE 2

R&D Appropriation History of AEC/ERDA, FY 1973-77

(millions of dollars)

Basic and Applied Research Operations

Fiscal Year	Budget to OMB		OMB Allowance		Congressional Appropriation	
	Basic	Applied	Basic	Applied	Basic	Applied
1973	286	154	283	171	275	150
1974	305	182	290	168	254	195
1975	307	214	324	230	257	260
1976	333	487	286	513	274	421
1977	389	483	337	370	370	444

R&D Operations

Fiscal Year	Budget to OMB	OMB Allowance	Congressional Appropriation
1973	1,352	1,374	1,362
1974	1,505	1,411	1,475
1975	1,704	1,704	1,668
1976	2,491	2,346	2,225
1977	3,014	3,042	3,168

Source: Energy Research and Development Administration.

TABLE 3

AEC/ERDA Appropriation History, FY 1952-77
(millions of dollars)

Fiscal Year	Budget to BOB/OMB	BOB/OMB Allowance	Congressional Appropriation
AEC			
1953	4,503	4,503	4,125
1954	2,437	1,593	1,058
1955	1,418	1,366	1,210
1956	2,050	1,416	834
1957	1,986	1,816	1,899
1958	2,971	2,492	2,324
1959	2,344	2,492	2,647
1960	2,781	2,604	2,652
1961	2,809	2,643	2,664
1962	2,885	2,598	2,547
1963	2,933	2,987	3,135
1964	3,150	2,893	2,743
1965	2,838	2,693	2,625
1966	2,925	2,481	2,366
1967	2,667	2,263	2,199
1968	2,350	2,493	2,509
1969	3,203	2,800	2,616
1970	2,912	2,366	2,223
1971	2,427	2,388	2,308
1972	2,670	2,338	2,294
1973	2,741	2,657	2,633
1974	3,195	2,482	2,389
1975	3,629	3,320	3,264
ERDA			
1975 Pay Raise Supplemental	6	6	6
1976	4,691	4,620	4,516
1977	7,699	6,227	6,334

Source: Energy Research and Development Administration.

TABLE 4

NIH Research Resources Appropriation History, FY 1962-77

(millions of dollars)

Fiscal Year	NIH Estimate to Department	Department Estimate to BOB/OMB	Budget Estimate to Congress	Congressional Appropriation
1962	122.7	119.3	95.3	127.6
1963	161.9	150.3	147.8	159.8
1964	211.9	179.4	164.7	163.9
1965	177.4	168.3	163.0	164.8
1966a	60.7	59.1	58.7	60.6
1967	80.2	73.8	67.8	68.5
1968	101.6	98.6	81.1	81.1
1969	89.9	90.4	87.5	84.8
1970b	80.1	76.5	69.7	67.9
1971	65.7	65.7	63.7	66.3
1972	67.5	65.6	68.1	75.0
1973	84.3	74.2	75.0	75.1
1974	153.1	133.0	88.6	129.4
1975	90.0	82.7	82.7	127.2
1976	88.9	90.0	81.1	130.3
1976 Transition Quarter	21.4	21.4	20.3	20.3
1977	93.0	93.0	92.3	135.0

a Excludes the "National Institute of General Medical Sciences" established as a separate appropriation in 1966.

b Excludes the "Office of International Operations," transferred out of this appropriation category in 1970.

Source: National Institute of Health.

TABLE 5

NASA Appropriation History, FY 1959-77
(millions of dollars)

Fiscal Year	Budget of BOB/OMB	BOB/OMB Allowance	Congressional Appropriation
1959	394.2	426.6*	330.9*
1960	647.8	508.3	523.6
1961	1,134.2	967.3*	966.7*
1962	2,425.1	1,940.3	1,825.3
1963	4,238.0	3,787.3	3,674.1
1964	6,209.7	5,712.0	5,100.0
1965	5,575.0	5,445.0	5,250.0
1966	5,562.8	5,260.0	5,175.0
1967	5,566.9	5,012.0	4,968.0
1968	5,478.0	5,100.0	4,588.9
1969	4,760.3	4,370.4	3,995.3
1970	4,274.8	3,771.9	3,749.2
1971	4,497.4	3,376.9	3,312.6
1972	3,689.4	3,312.7	3,310.1
1973	3,385.2	3,407.6	3,407.6
1974	3,540.4	3,053.8	3,039.7
1975	3,350.7	3,267.1	3,231.2
1976	3,636.2	3,539.0	3,535.0
Transition quarter.	965.4	958.9	925.1
1977 estimate	3,862.5	3,697.0	3,692.5

*Includes funds transferred from DOD (1959, 146.6; 1961, 2.7)

Source: National Aeronautics and Space Administration.

submitting the requests, which is precisely what it must do in the course of the OMB's formalized evaluation process. The growing use of evaluation, particularly cost/benefit analysis (C/BA), offers the OMB a major advantage: C/BA forces the agencies to rationalize their own priorities in terms of the White House's over-arching goals rather than forcing the OMB to "disprove" the utility of specific programs.

Cost/Benefit Analysis and Research

The introduction of C/BA procedure to government management and budgeting has been called the "rationalization of the decision-making process." As Wilensky (1967), among others, points out, however, the larger goals of the political process are too value-laden to be subjected to C/BA. For example, while the cost and benefits of developing and maintaining different space-station systems can be delineated, the decision as to whether man should explore space is not a question that can be resolved in these terms. At best, the debate about man's role in space takes place in the political or societal arenas rather than the explicitly economic arena. Thus goal setting remains an outcome of the political rather than the cost/benefit process. C/BA asks how to accomplish a predetermined goal most efficiently.

The issue of C/BA raises two distinct questions. First, there is the question of whether, and to what extent, C/BA techniques are being injected into science policy formulation and management. Second, if C/BA is being increasingly utilized, how applicable is it to the various aspects of basic and applied research?

Finding improved means of analyzing federal programs has been a priority of the EOP for well over two decades. In 1961, the BOB released a booklet describing progress made in improving budgeting practices and concepts as authorized by President Eisenhower, a process called "program management" (BOB 1961). Impetus for increased analytical monitoring of research was given by the Bell report (1961), commissioned by President Kennedy. C/BA has become a keystone in upgrading budgetary and management supervision and, as a former BOB director has pointed out, C/BA systems have created a centralization of power and planning (Schultze 1968:92-96). Centralization of analysis has meant greater power in the hands of the OMB, the prime wielder of this "rational choice tool," and in the EOP in general (cf. Downs 1967).

> **We [at the Bureau of the Budget] have been working at top speed to change and upgrade the Government's decision-making process and to inject better methods into the way Government works out problems of choice and makes up its mind what to do next. And I see no reason why R&D should have immunity from all this (Editorial by William D. Carey in *Science* 3 January 1969:8).**

TABLE 6

NSF Appropriation History, FY 1952-77

(thousands of dollars)

Fiscal Year	Budget/BOB/OMB	BOB/OMB Allowance/	House Allowance
1952	14,780	14,000	300
1953	15,000	15,000	3,500
1954	15,000	15,000	5,724
1955	20,600	14,000	11,000
1956	24,650	20,000	12,250
1957	58, 791	41,300	35,915
1958	72,835	77,300	48,750
1959	152,167	140,000	115,000
1960	232,340	160,300	143,273
1961	214,835	190,000	160,000
1962	325,085	277,000	248,000
1963	422,400	358,000	310,000
1964	558,000	589,000	323,200
1965	640,000	487,620	420,400
1966	554,000	530,000	480,000
1967	647,932	532,732	479,999
1968	614,000	526,000	495,000
1969	613,400	500,000	400,000
1970	641,400	500,000	420,000
1971	681,200	513,000	497,000
1972	664,500	623,150	585,000
1973	692,318	687,318	652,320
1974	724,660	592,660	574,260
1975	703,450	788,200	773,600
1976	899,500	755,400	711,100
1976 Supplemental	1,400	1,400	1,250
Transition Quarter	186,575	167,700	167,634
Suppl TQ	471	471	421
1977	792,000	802,000	750,000

*Based on congressional action.

Note: Includes supplemental appropriations and Special Foreign Currency Program and obligated funds carried forward from prior year(s).

Source: NSF Appropriation History; NSF History of Obligations; NSF FY 1977 Budget to Congress.

Senate Allowance	Congressional Appropriations	Obligated
6,300	3,500	3,466
6,000	4,750	4,424
10,000	8,000	7,954
14,000	12,250	12,487
20,000	16,000	15,989
41,300	40,000	38,630
52,300	49,750	49,974
140,000	134,000	130,940
160,300	152,773	156,600
191,600	175,800	174,996
275,000	263,250	260,282
335,000	322,500	320,754
373,200	353,200	354,585
420,400	420,400	415,967
479,999	479,999	466,428
479,999	479,999	465,119
505,000	495,000	505,228
410,000	400,000	432,646
461,000	440,000	462,618
522,500	513,000	496,405
648,150	622,000	600,720
639,160	645,740	610,271
584,260	579,260	645,650
761,400	768,150	693,127
717,100	714,000	730,166
1,250	1,250	1,250
167,634	167,634	167,634
421	421	421
801,600	773,600	773,600

The Planning-Programming-Budgeting System (PPBS) technique, judged a failure was an offshoot of C/BA. PPBS was adopted by the federal government in another attempt to rationalize and manage federal programs in accordance with the larger goals promulgated by the presidency. First developed at the RAND Corporation during the 1950s and adapted by the DOD under the aegis of Robert McNamara (DOD Secretary during the Kennedy administration and the first part of the Johnson administration), PPBS was not problem-free at the Pentagon. Administration officials decided it worked well enough, however, to merit adoption throughout the federal government. On August 25, 1965, President Johnson announced to his cabinet that PPBS would, in his words, "improve our ability to control our programs and our budgets rather than having them control us" (Hildebrand 1968:7). Despite Johnson's enthusiastic boost, PPBS was resisted by the federal bureaucracy and soon came under sharp attack (e.g., Hoos 1972). The system was charged with producing superfluous paperwork. The original PPBS purpose was lost in the many details of the bureaucratic process. Bertram Gross, a social scientist specializing in this area, summarized the problem: "The flood of PPBS paperwork, clogging the channels of government communication, has in some cases threatened the very capability for rational action that it was supposed to enhance" (Gross 1969). Another problem was that a system designed to work with hard data and specific measurements associated with defense needs was not readily transferable to the soft data and diffuse measurements used by domestic agencies. William Gorham, former assistant HEW secretary (and in charge of the early government work on social indicators) stated' "It's going to take many years to develop the kinds of output measures we need to understand what we are getting for our money."

When PPBS was introduced, its utility in R&D programs was widely questioned (cf. Scherer 1965). At the time it began, Carey stated that "nothing is in sight, so far as I can tell, that will replace level-of-effort decision-making on the funding and mix of basic science." This was widely interpreted to mean that R&D was still a matter of judgement. In 1967, however, Carey reversed himself, acknowledging that PPBS "could help . . . in balancing resources among competitive though disassociated research investments such as the 1,000-bev high energy machine and ocean research" (*Scientific Research* July 1967:31).

Considerable debate has revolved around whether or not C/BA is appropriate for evaluating scientific programs. The NAS attacked PPBS on the grounds that it has "major difficulties . . . cost/benefit analysis is difficult to apply and may be misleading when applied to R&D projects and especially to basic research" (House Committee on Government Operations 1967b:4). Other criticisms have been based largely upon the fact that the long-term benefits of basic research cannot be assessed in quantitative and especially in monetary terms; research is "intermediate" to the extent that it may provide links to progress in solving larger problems. Further, it has been argued that the true value of science is "ideational," "spiritual," "serendipitous," or linked with the "essence

TABLE 7

NSF Budget Reserves, FY 1952-77

(thousands of dollars)

Fiscal Year	Budget Reserve
1952	–
1953	–
1954	330
1955	–
1956	–
1957	4,000
1958	–
1959	2,000
1960	–
1961	–
1962	–
1963	–
1964	–
1965	–
1966	–
1967	12,000
1968	46,500
1969	20,000
1970	–
1971	9,660
1972	31,200
1973	62,400
1974	–
1975	20,000
1976	10,000
1977	–

Note: Includes Special Foreign Currency Program. All budget reserves from 1954-73 were subsequently released by BOB/OMB and obligated by NSF. In 1975 and 1976 $20 million and $10 million respectively were deferred and approved by Congress under terms of the Congressional Budget and Impoundment Control Act of 1974; these deferrals were subsequently released by OMB and obligated by NSF.

Source: National Science Foundation Appropriation History.

of civilization," hence not subject to C/BA. (For a discussion of the relative merits and problems of C/BA in scientific and technological programs, see Margolis and Barr 1965; Held 1966; Joint Economic Committee of Congress 1969; Doh 1970; Organization for Economic Cooperation and Development 1972; and Sapolsky 1973.)

Cost estimates for future untried technologies, such as the breeder reactor, are subject to great variation, often simply resting on the opinion of one or another group of experts. Discount rates and other critical variables are largely unknown. Projects must be evaluated in relative terms, such as the costs of substitute technologies which are themselves difficult to judge. Thus coal liquefaction and gasification will be uneconomical until oil reaches the price of, say, $18 per barrel, a price which itself is determined by political and economic rather than scientific processes. The crucial underlying problem is how to evaluate different programs which pursue different aims; both process-development costs and output measures are elusive in many types of R&D.

The potential, as well as the limits, of C/BA are becoming better understood as the payoffs of different types of scientific research are more adequately preceived and objectively defined. C/BA is being incrementally applied to more scientific programs with greater success. For the most part, basic science cannot be adequately subjected to cost/benefit scrutiny. Despite this limitation, C/BA itself (although specific systems have been abandoned) is becoming increasingly useful as a tool to the OMB.

The central position of the OMB in directing C/BA was explained during a Congressional Inquiry:

> Cost/benefit analysis in R&D implied a careful and detailed evaluation of programs, weighing alternative research strategies against each other in terms of broad national goals and relative costs. This kind of analysis can be and should be carried on with respect to R&D programs even when quantification is impossible.
>
> In brief, the national benefits of many R&D programs are not capable of measurement [sic], but benefits can be subjected to intensive analysis and weighed against costs. Upon such analysis, careful judgments will be informed by analysis (House Government Operations Committee 1967:5).

1969 Congressional hearings on PPBS highlighted several useful analyses within the BOB. Included in the "selected" list of examples were several projects dealing with science and technology and social-science utilization in evaluation (which varied in the quality of work and validity of assumptions). Among relevant examples of utilization were the following:

- A study showing the costs and benefits of the Liquid Metal Fast-Breeder Reactor development. This study was used to determine both the budget (a $3.5

billion program was requested) and the pacing program. (This turned out to be a poor example of C/BA's utility; both the costs and the time needed for the breeder program were drastically underestimated.)

- An analysis of disease control, exploring alternative means of controlling cancer and arthritis. This analysis was useful in redirecting programs in medical research.
- A study of the costs and benefits of the Apollo flights in order to determine the space program directions for the 1970s.
- A study of the costs and benefits of nuclear-rocket R&D in order to plan future space expenditures and the goals of the space program.
- Studies of the Peace Corps on the intracountry basis, leading to shifts of Peace Corps volunteers and resources into more productive areas.
- An analysis of the supersonic transport (SST) which identified potential monetary and nonmonetary returns and proved useful in evaluating public policy in this area.
- An analysis of the summer Head Start program which showed insignificant measurable gains, resulting in a reallocation of resources to the full-year program.
- A study of alternative research programs in law enforcement which has helped to identify areas of highest priority for research (House Government Operations Committee 1969:329-30).

PPBS's problems caused it to be abandoned around 1970. A new system, Management by Objective (MBO), which was first pioneered by the Harvard Business School, has evolved as the current major format of OMB's C/BA. MBO was initiated in the OMB by deputy director Frederick Malek and has been forcefully implemented by Malek's successor, Paul O'Neill. Under the MBO system, each agency submits a list of objectives for the upcoming year. This list is carefully scrutinized by the OMB and then discussed with the originating agency. The approved lists of objectives are then sent to the president. If the objectives are acceptable to the president, the agency is expected to achieve those goals. After approval there are frequent meetings between the OMB top staff and agency personnel to monitor progress on the achievement of the objectives. The OMB hopes the MBO process will avoid the pitfalls of the PPBS system, especially PPBS's tremendous paperwork loads. MBO is not a rigid system, but rather a "way of life, a way of thinking, how you do business," according to Malek (*Science* 25 January 1974:289; see also Havemann 1973a, 1973b).

How well has MBO worked out for scientific research thus far? Malek has claimed that the NSF has done "a very good job in this area" seeking to get "something quantifiable or at least measurable" (*Science* 25 January 1974:289). Among recent objectives (1975) for the NSF have been the assessment of ocean resources, increased international scientific cooperation, and the development of scientific knowledge about energy (Havemann 1974b).

The same problem that debilitated PPBS's approach to research—that the outcome is difficult to cast in quantifiable terms that also have intrinsic meaning—often haunts the MBO approach to research. Although basic research remains highly resistant to this type of analysis, given suitable information, priorities can be set. Malek points to the NIH's biomedical research program. HEW, of which NIH is a part, "has an objective related to the effectiveness of implementation of the cancer program and subobjectives related to the direction research on cancer is to take. Cancer [because of political reasons] has a little higher priority than other types of research" and is therefore a more important type of biomedical research for the NIH (*Science* 25 January 1974:289). Applied research is of course much more amenable to MBO-type of analyses. By definition, applied research is aimed at producing results to help resolve a particular issue or question. Whether or not a particular issue or question has been resolved can readily be seen; consequently MBO has much greater utility. Whenever possible, agencies are encouraged to formulate their programs in MBO terms (Lewis and Zarb 1974). NASA specifies applied research objectives such as using space satellites to locate, measure, and predict the natural resources; describing earth's geophysical characteristics; or assessing and monitoring water and environmental quality (Havemann 1974b). By measuring the accuracy of comprehensiveness of such studies the degree of the objective's fulfillment can be assessed both by NASA and the OMB.

Carter is introducing Zero-Base Budgeting (ZBB) into the federal government through the OMB. ZBB assumes that each program must be carefully justified, both absolutely and in relationship to the overall goals of an agency. No preconceived notions about a program's worth are accepted and the program's value must be warranted "from the ground up" (see Pyhrr 1977). ZBB is to be integrated into the MBO process in an attempt to foster C/BA-style thinking. Yet ZBB is suffering from the same limitations and having the same advantages as other types of Cost benefit analysis, is no panacea (Anthony 1977).

As these various examples demonstrate, C/BA is being used to help make decisions and manage programs at the level of the EOP. C/BA has established a payoff matrix into which specific programs can fit. The framework of the matrix is delineated at the presidential level and the programs can be scrutinized from the standpoint of presidential priorities. The use of C/BA has strengthened the presidency in its ability to deal with science policy. The executive office is free to lay down the broad judgmental framework, and the agencies must then demonstrate how their programs can be expected to fulfill the political goals of the president. The analytical justification is built into the budgeting process, and deviation from the president's goals is more easily detected and corrected. James Schlesinger put the matter in the clearest terms:

> It is an objective of this [the Nixon] administration to *institutionalize* [C/BA] within the departments, so that they automatically perform more and more of the task [of rationalization]. The Bureau

> will continue to monitor the programmatic content embodied in agency budgets in line with Presidential priorities (Senate Government Operations Committee 1969:311, original emphasis).

The advantages of PPBA and MOB are clear, yet they also have inherent difficulties which limit their effectiveness (Merewitz and Sosnick 1971; Hoos 1972). A key problem clearly has been that in scientific research, and especially basic research, it is difficult to state or even create objectives amenable to such comparison or measurement. Further, while C/BA may give the OMB a good "lock" or check on new projects, it is less effective in monitoring the current operations of agencies, and the OMB periodically gets surprised by unnoticed changes or growths in ongoing agency programs. The line agencies must also cooperate fairly closely with the OMB if C/BAs are to work. Agencies can, if they desire, undermine this process without too much difficulty. C/BA can be decoupled by formulating objectives so that a predetermined and readily attainable result can be shown. An alternative strategy to frustrate C/BA is to state nonmeaningful outcomes that are amenable to quantification and will probably be going up (or down) anyway, thus inaccurately reflecting success. Both these actions comply with the form of C/BA while frustrating its intention. Nevertheless C/BA is useful in getting agencies to focus on questions of the relative advantages various research projects have to offer or even in asking why it is worthwhile to undertake certain research projects in the first place. C/BA is a way of thinking about and analyzing what agencies are trying to achieve, a way of doing business. And to encourage this way of thinking the OMB can resort to painful reprisals on recalcitrant agencies. But reflecting the difference between perceived and real power, one OMB staff member said, "If an agency really wants to screw us, they can do it."

OMB'S POLITICAL ENVIRONMENT

Relations with the White House

While the OMB is part of the office family of the EOP, it is not formally part of the White House. OMB middle- and upper-echelon staff interact with the NSC (for foreign affairs), the Domestic Council (for domestic affairs), a range of White House staff members and White House councils, and various task forces and committees. The ultimate aim of all the OMB's activities is to serve the presidency.

The president's wishes provide the direction for OMB activity. Depending on his personality and situational demands, the president expresses his interest in various aspects of the science budget. The OMB transforms the general presidential will into science policy through three different modes. The first is in response to a presidential statement or agenda for action. A clear declaration by

President Nixon launched Project Independence, a call for attaining energy self-sufficiency by 1980. This spurred efforts to support the entire array of energy R&D, including the breeder reactor and coal, items already high on the Nixon priority list. In the second mode the president's response to the various options presented to him before a program begins reveals his thinking and desires in that particular area. This "detail end," the results of decisions and choices presented to the president, provides other indications to the OMB of the president's orientation on an issue. The president's decision on which of several courses of action should be taken on the number of earth-resource satellites or the pacing of the development of the space shuttle are useful indicators to the OMB that transcend the particular issue. The OMB is sensitized to receive policy "signals" from the president, yet these signals are often very fuzzy, diffuse, and unclear, leaving the OMB with little choice except the trial-and-error method (but consequently with more decision-making latitude) (cf. Niskanen 1973). Close consultation between the president and the OMB director facilitates OMB understanding of presidential wishes by filtering down information and perceptions through the organizational hierarchy. This "testing the waters" mode is the reverse of the "declaration" mode. The third mode is through the implementation end of program development. This amounts to "feedback" from the president after certain programs and policies have been implemented and are in operation. Oftentimes the president's response is motivated because of complaints or problems arising from a certain course of action. In any event, the president's reaction permits the OMB to fine-tune (or completely readjust) its policy in a given area. This was the case behind a decision made, after industry complaints, to move all new uranium-enrichment research efforts to the private sector. Through all three of these methods the OMB seeks to correlate its efforts with priorities of the president.

Two major factors help determine the connection between the president and the OMB: the attitude of the president and the strength and personality of the OMB director. Truman had full confidence in his BOB and relied on it heavily. Eisenhower was distrustful of the BOB, feeling that after 20 years of Democratic control the BOB would not serve his interests well enough to merit his full trust. The Eisenhower BOB directors tended to be relatively weak, and even a very able BOB director, Maurice Stans, was not able to restore Eisenhower's faith in the BOB. The Kennedy administration was a period of renewed strength for the BOB, having dynamic and influential directors David Bell and Kermit Gordon (Otten 1964). The directors have continued to be highly influential throughout the Nixon, Ford, and Carter administrations, although some directors had the full confidence of the president and others didn't. On many occasions the OMB director serves as an adviser directly to the president and this strengthens OMB's voice in policy affairs.

The White House advisers to the president themselves often have a significant role in science policy formulation and in guiding the OMB's direction in science policy. Thus during the height of pre-Watergate politicization, White House staff members made plans for the OMB to redirect federal R&D funds into politically advantageous channels while simultaneously withdrawing these funds from political "enemies" although Watergate prevented these plans from reaching fruition. On another occasion, presidential counselor George Schultz (who was also Secretary of the Treasury) intervened literally to save the National Science Foundation budget from total evisceration. Likewise presidential adviser Mel Laird acted personally to have gutting, biomedical-research budget cuts rescinded, thus rescuing the health research community.

According to the organizational charts the OMB is under the indirect tutelage of the NSC and its domestic counterpart, the Domestic Council, now called the domestic policy staff by the Carter administration. When the OMB was established in 1970, President Nixon also established a corollary organ inside the White House, the Domestic Council, composed of White House personnel and some cabinet officers. The Domestic Council's mission, stated Nixon, would include "providing rapid response to presidential needs for policy advice on pressing domestic issues" as well as assessing, defining, and finding alternative ways of achieving a national goal and objectives. The difference between the Domestic Council and the OMB was that "the Domestic Council will be primarily concerned with *what* we do; the Office of Management and Budget will be primarily concerned with *how* we do it, and *how well* we do it" (Nixon 1970: 259, original emphasis).

The immediate fear was that this move would separate policy from operations, leading to a sharp loss of power and respect for the OMB personnel and lead to disastrous consequences for sound governmental practices (Schick 1970). The OMB, however, moved to maximize the interface between it and the Domestic Council. George Schultz, OMB director at that time, said that he was able to and would continue to work effectively with Ehrlichman, Nixon's assistant for domestic affairs and director of the Domestic Council: "We will have offices in the White House on the same floor . . .we will be in constant touch with each other" (Kempster 1970:45). Many of the Domestic Council staff were recruited from the OMB, assuring at least temporarily continued representation of the OMB view at higher levels in the White House.

More than any other factor safeguarding the OMB's power was the faulty assumption underlying the belief that policy could indeed be separated from administration. To a very large extent administration functions *are* policy-making functions. Nixon's original distinction became a differentiation between policy (in this case of over-arching goals) and implementation (the expertise and ability to attain these goals) with policy per se the domain of the Domestic Council and implementation the domain of the OMB. Even these distinctions were more a matter of emphasis than substance since there can be no true separation at this level either. The Domestic Council has evolved toward a support staff for White House presidential assistants whereas the OMB has recouped many of its responsibilities initially absorbed by the Domestic Council (House Appropriations Committee 1974:636).

Important policy-setting functions for civil and military science policy do reside within the Domestic Council, the NSC, and the various special presidential assistants despite the ongoing authority and responsibility of the OMB. The formal organizational arrangement sets these councils as the policy-formulation bodies and assigns the OMB responsibility for carrying out the policies. In practice the lines are blurred with the OMB taking an active role in formulating and recommending policy and dovetailing its efforts in line with these bodies and in accordance with the plans of the president. The Domestic Council and the NSC themselves work with the federal bureaucracy to help develop priorities in line with overall White House policies and to supervise the implementation of presidential decisions (Organization for Economic Cooperation and Development 1974:155). As such, the staffs with scientific-policy responsibility maintain close relationships with the OMB and liaison with the appropriate scientific and technical experts in the executive-branch bureaucracy. The Domestic Council has at times sensitized the OMB to political considerations, especially those emanating from Congress.

The OMB also received input on science policy from the president's science adviser, a White House post, and from the Office of science and Technology (OST), both part of the EOP before their abolition in early 1973. While the OST's relationship with the powerful OMB was always unequal, the OST was often able to participate in helping the OMB to define questions, analyze government options for alternative policies, and even, in some cases, voting on an OMB decision. Several OST staff members provided specialized backup and advisory services for the OMB, forming a technical extension arm for the budget bureau. While some OMB staff members perceived the OST staff as striving to offer impartial advice, the OST advice was often lacking the political dimension needed to be useful in forging political decisions (Barfield 1973). Other OMB staff perceived the OST inputs on budgets as a special-interest group lobbying for preferential treatment. Such attitudes diluted the impact of OST arguments. The OST inputs did contribute to the OMB position on the fast-breeder reactor, the manned orbiting laboratory, and the space shuttle, among other major projects. There was considerable speculation on the part of government officials and scientific leaders on whether the OMB, supplemented with advice from the science-policy office of the NSF could adequately fill the gap left by the disbanding of the OST. The OMB has thus far been able to move ahead without being hampered by the loss of OST technical experts. A White House science staff, the OSTP, was reestablished in the summer of 1976 but this action has been based significantly on political and contingency-planning rationales and not singularly on such an office's utility to the president and the OMB in formulating and overseeing the implementation of science policy (although this is also seen as the major benefit–see Chapter 10).

Politicization

One of the hallmarks of the Nixon administration was the politicization of the government, and the OMB was no exception to this rule. The most notable example of this phenomenon in the OMB was the creation of Program Associate Directors (PADs). The PADs are the White House's appointees over each major OMB division. These PADs maintain close contact with the president and his assistants and provide a solid linkage between the White House and the OMB. Heclo (1975) and others have decried the creation of these posts. Their argument is that these politically motivated men will destroy the neutral (in terms of agency or program preference) competence of the OMB. The "neutral competence" has been the primary reason why the OMB and its predecessor, BOB, had been so successful. The treatment an agency received, for better or worse, was rationalized in the interest of serving the presidency (and therefore, perhaps circuitously, the nation). The OMB, however, lost the appearance of "neutral competence" and instead represents the short-term political interests of the president, and these short-term plans can be very destructive to the long-range operation of many agencies.

The additional layer of political appointees on top of the OMB's hierarchy, it has also been argued, will have dismal consequences for the morale of the OMB and perhaps even the OMB's effectiveness. Criticizing the BOB-OMB transformation, President John Giner of the American Federation of Government Employees, AFL-CIO, argued that the way in which the top levels of the civil service are selected inevitably affects the middle and lower levels. "Political influence, special interests, manipulations, exceptional criteria will creep into the system, whether we like it or not, and no matter how upright the administrators are." This would prevent those with "drive and initiative" from entering government service (House Government Operations Committee 1970:126).

These fears proved well grounded in some cases. In the politically charged atmosphere of the Nixon staff administration, the political appointees from the White House "seriously diminished the responsibilities of the career, professional staff of OMB and its capacity to provide the kind of objective and expert counsel to the President which characterized earlier operations" (National Academy of Public Administration 1974:43). This did in fact occur, even to the extent that PADs interfered with hiring decisions at the lower OMB staff levels. The new layer of PADs created an additional barrier between careerists who formerly had access to assistant directors. Delays, frustration, and poor communication were problems aggravated by blocking the "institutional memory," the benefits of years of government experience being brought to bear on seemingly "new" problems. As a result staff turnover became quite high (Fisher 1975:57), further aggravating these problems.

While many of these fears were well grounded, the rationale behind politicization can be understood by looking at its function. It is important to make a distinction between two types of political appointments which can be made because the particular type chosen makes considerable difference in the implications for the OMB operation. There are those political appointees who embody the perjorative aspects of the term politician. These people, often appointed to pay off a political debt, can have very destructive effects such as those enumerated above. Several of Eisenhower's appointments to the BOB have been characterized this way (although if Eisenhower had little esteem for the BOB, this would have been a convenient way for him to discharge political obligations without interfering with the areas he deemed important). Robert Mayo, Nixon's first director of the BOB, has also been described as fitting in this category.

A second type of political appointee is the competent or even highly competent political operation. These individuals, who are more typical of the recent PAD officers, are more tuned into the needs and priorities of the president and are therefore able to make the OMB more responsive to the president's political dimension, consequently serving the president better. They are more ably equipped to take the larger national and political framework into account in making OMB technical and policy decisions. OMB decisions do not take place in a vacuum and the decisions sometimes have to be adjusted to fit in at the political level. Clearly there are now more political appointees in the OMB than 20 years ago in the BOB, but this means the OMB is more linked with the political level and is consequently able to do a better job serving the president (as opposed to the presidency).

The politicization process, then, has both a negative and positive aspect. It aids the OMB in doing a better job in serving the president and striving toward his goals. Politicization has negative ramifications in that it may demoralize OMB career personnel and have spillover effects in reducing cooperation among the bureaucratic departments fearful that a new program will not succeed on its own merits but only if it is in accord with a politically expedient program of the president. In the area of science policy many argue that the clearest example of politicization process is the OMB's sharpened attack on the peer-review system. This system wherein leading scientists evaluate proposals for funding ostensibly guarantees the autonomy of science. The major complaints against it are that it is difficult for administrators to have central planning, it frees the bureaucratic agency from responsibility for carrying out programs which are tightly coupled with presidential objectives, and it breeds a "buddy system" wherein elite scientists help one another to mutual advantage without regard to public interest (*Science* 25 May 1973:843).

While the ultimate impact on the social system of science of this politicization cannot be discussed in this chapter (see Chapter 11), yet clearly the issue of the politicized OMB's impact on science and national policy will come under increasing scrutiny. Heclo (1975) has explored this issue in terms of national policy, and Congress has taken first steps in this direction via the Budget Reform Act of 1974, but the issue will be the topic of many controversies in the years ahead.

CONCLUSION

The OMB is intensely involved in all aspects of science policy. It participates directly in the formulation of policy goals; in the monitoring of the ongoing programs; evaluation of the results; and implementation of modifications, limitations, terminations, and innovations in these activities. The OMB Science, Space, and Energy Technology division is the only place in the government that has responsibility to monitor the overall federal science research enterprise, which in turn affects private and industrial policy. The OMB like any organization has not always performed without misjudgment, error, or failure, but it has been one of the most important mechanisms in determining and regulating the scope and level of national policy for scientific research.

The future direction of the OMB seems clear. The OMB will be rationalizing its budget-and-management process and call upon the executive agencies to do the same. The OMB is also becoming more politically responsive to the White House. The OMB's concern with basic research and the health of a research field shows that it was responsive to long-term societal goals; to the presidency as well as to the particular president. Now, however, it seems the direction will be toward even greater political responsiveness to the man who occupies the Oval Office, and his lieutenants in the White House. Further, the OMB is receiving encouragement from many sectors to take greater responsibility for guiding the national scientific research efforts: a congressional committee (House Science and Astronautics Committee 1970), a statesman of university-science policy (Wolfle 1972), and the GAO (1972) have all advocated centralizing responsibility for science policy in the OMB.

In sum, the OMB is the single most effective influence on national science policy. From all signs it will continue to gain power in guiding and controlling federally sponsored scientific and technological research. In order to do so, the OMB intends to learn, in a rational and analytical fashion, what the benefits are to be derived from the national investment in science, and the answers will have to be weighed in terms which are politically meaningful to the EOP.

The ultimate questions transcend issues of rational coordination and address the issue of to whom the OMB is accountable and responsible. Is it only to the "political" president, the particular needs of a particular needs of a particular man in power? Or is it ideally to the agencies the OMB examines as well (and by extension, the people and constituencies these agencies reflect)? What trade-offs are necessary to achieve a balance between these responsibilities, and where should the fulcrum of the balance be placed? A major problem in dealing with the issue of responsibility is the lack of direct personal accountability for very serious, wide-impact decisions that are made by the OMB. An opportunity exists to enhance responsibility in the OMB by providing strong motives for excellence in the analysis of the real needs of agencies and of the public needs which extend beyond the term of office of any particular president. The dilemma comes in that the provision of these motivation may serve to reduce the OMB's utility to the particular president in power and thus reduce the OMB's power over the agencies. This is because the political mission of the OMB will have shifted to another presidential staff since presidents inevitably feel they need this service. The conflict and contradictions between service and power must be perennial in the OMB.

PART VIII

LESSONS FROM OTHER COUNTRIES?

BACKGROUND AND SUMMARY

The readings in Part VII suggest that a number of distinctive institutional and historical forces have been at work in shaping the process by which issues in American science policy are defined and addressed. But the difficulities and the struggles that have characterized much of that process are not altogether unique to the United States—striking an appropriate balance in the roles of scientists, elected politicians, and administrative officials in the formulation of science policy has been a continuing problem for all the developed countries. The selections in Part VIII, which use a comparative approach to explore similarities and differences in the ways different nations handle aspects of their science policy, help put the American experience into perspective.

In the first selection, "Comparative Approaches to R&D Policy Coordination", Ronald Brinkman analyses the methods used by the United States and five Western European countries (France, Britain, Belgium, the Netherlands, and West Germany) to promote the interagency coordination of R&D policy. According to Brinkman, interagency coordination has become important to advanced industrial societies because "the expansion of governmental tasks has also been accomplished by an increased specialization of objectives and tasks within particular agencies, a tendency toward self-perpetuating relationships between public agencies and their constituencies, and the institutionalization of bureaucratic forms of behavior—all of which tend to hinder change and mutual adaption" (p. 73).

Using four variables, (1) procedures, (2) aims, (3) distribution and sources of influence among participating agencies, and (4) the frame of references which orders governmental actions in R&D for coordination purposes, Brinkman concludes that differences among the six countries stem from general features of policy formation within each nation and changes over time in the conception and goals of "science policy." But there are also some commonalities across nations. In recent years, he argues, "the comming of age of science and technology policy has put somewhat different strains on interagency coordination. Dramatic institutional changes and shifts in the allocation of resources are now less common; annual budgetary outlays continue to show at best only slight increases in constant dollars. Governments are now taking a decidedly managerial attitude to what they have created. Existing institutions are incited to make better use of their facilities and resources, and new tasks must be met in part by recycling existing resources. The thrust of contemporary science policy is indicated by such watchwords as "utilization", "reconversion", "redeployment", and mobility". Pre-established areas of competence among agencies are not always the best suited to effecting shifts in existing resources to meet new demands. Thus, the situation has renewed calls for coordination in many countries. But coordinating units . . . accustomed to coordinating through the selective allocation of new resources, have experienced difficulty adjusting to the new context of science and technology decisionmaking" (p. 89).

But in spite of these calls for greater coordination of R&D policy and expenditures, finding the right model for achieving such coordination is elusive. Even evaluating national approaches to R&D policy coordination is difficult because the inherent ambiguities of the coordination concept and the difficulties in defining the "perfectly coordinated" system are obstacles to evaluating coordination formats both within and between nations. In short, according to Brinkman, "there is 'no one best way' to coordinate which can provide a standard for the relative success or failure of any particular format." Each nation is "simply different in [its] ambitions, procedures, organizational arrangements, and frames of reference" (pp. 89–90). The difficulties encountered in evaluating and designing coordination efforts leads Brinkman to conclude that "interagency coordination is an intrinsically frustrating, unglorious enterprise whose success story will probably never be written" (p. 91).

In the second selection, Brinkman and Arie Rip analyze the evolution of high-level science advisory councils in France, the Netherlands, and the United States in light of common developments in the relationship between science and government between 1957 and 1977. In "Science Policy Advisory Councils in France, the Netherlands and the United States, 1957–77: A Comparative Analysis," Brinkman and Rip address three questions that are central to the role of scientists in R&D policy making: "(1) What are the most striking similarities in the role and evolution of science policy advisory councils in the three countries and what are their differences? (2) What are the trends and features in the science-government relationship, common to all three countries, which account for the similarities in the councils' role and development? and (3) What are the unique features of each council's political and cultural setting which best account for the observed differences among them?" (p. 169).

In general, Brickman and Rip found that the creation of the councils (the President's Science Advisory Council (PSAC) in the U.S., the Comite Consultatif de la Recherche Scientifique et Technique (CCRST) in France, and the Raad van Advies voor het Wetenschapsbeleid (RAWB) in The Netherlands) in the late 1950's and early 1960's came in response to similar needs of government in the domestic and international spheres. In their first years, the councils performed useful tasks and exercised substantial influence, but by the late 1960s and early 1970s, all three councils showed a decline in activities and impact. This decline is "attributable to the 'bureaucratization' of science policy-making, a slower rate of growth in R&D expenditure, a more goal-oriented approach in governments' sponsorship, changing public attitudes toward science and technology, and the intrinsic difficulties faced by the council members in acting both as scientists and policy advisors" (p. 167).

During the mid-1970's, however, the councils were partially revitalized because of emerging needs and institutional developments affecting all three governments. Brickman and Rip observe that "around 1975 and 1976, there were signs that the tide of governmental support of R&D was again on the rise. Also, the public, policymakers and scientists themselves may have become more accustomed to scientists proffering political judgements, and to disputes between experts" (p. 182). Furthermore, the growth of the science policy bureaucracy reached such proportions that political authorities felt the need for an independent source of advice as a counterweight to the regular bureaucracy. Finally, the desire to have some body that would transcend the parochial interest of different agencies and the general movement towards participation and democratization in social institutions in all three countries led to the recomposition of the membership of the councils based on more representative criteria.

Brickman and Rip conclude that the distinctive features of the science policy advisory councils in each nation derive from a number of factors: differences in the size of the three countries, their productive bases and the historical relationship between their public and private sectors, as well as underlying differences in the nation's political organization and culture. The conjunction of all these forces tends to be mirrored "in governmental conceptions of science policy, in the role played by scientists in public affairs, and in the organization of scientific and technological interests in political decision-making" (p. 167). But, despite these differences, the roles and functions of these bodies—to promote government support for science and technology, to evaluate proposed projects, to forecast developments, to provide a broad overview, and to set priorities while adopting to changing perceptions of governmental needs—suggest that they will survive for many years into the future, although perhaps with more modest expectations about their impact.

The third selection in this section, Michael Mulkay's "The Mediating Role of the Scientific Elite," compares and contrasts the internal organization of academic science in Britain and the United States and its effects on the development of science and science policy. In particular, Mulkay focuses on the elite of the scientific community. He argues that this elite is strongly committed to objectives regarded as central within that community and acts as a "buffer group" successfully resisting instrumental demands from outside the community by government and others in order to maintain the "considerable freedom for members of the academic research community to pursue their own scientifically defined interests" (p. 446).

After providing "copious evidence of the existence of a small elite stratum in science as well as some indication of how this elite is created and sustained" (p. 453), Mulkay turns his attention to the relationship between government and the academic elite of science. Since the late 40's and 50's, beginning with the Manhattan Project, members of the network of elite scientists in both Britain and the United States have been engaged in advising their governments on a variety of issues having a large technical component, in helping to implement major technological programs, and in regulating government support for academic research. Mulkay asks: "How and to what extent does this elite influence the formulation and implementation of government policy toward science?" (p. 456).

Concentrating on academic science in Britain, Mulkay asserts that "[t]here can be no doubt that governmental support of academic research is primarily instrumental. The massive sums of money necessary to maintain modern, high technology science are furnished because 'basic' academic research is seen as being the fundamental source of military and industrial technology" (p. 457). He notes that in Britain in recent years there has been increasing concern in official circles to regulate basic research so as to ensure that its practical benefits are forthcoming as directly and as quickly as possible. Much of this control, it is said, should come from outside the research community to maximize practical pay-offs. Not surprisingly, the scientific elite has been extremely unreceptive to attempts by laymen to define closely their professional objectives. Their resistance to external pressures has been particularly effective whenever the system of informal social control by competent colleagues extends into governmental decisionmaking. As a result, in Britain, "attempts to guide the academic scientific community in a utilitarian direction have been on the whole relatively ineffective" (p. 461).

Mulkay concludes by noting that in Britain, "at the highest levels of science policy, it is clear that politicians and civil servants play a decisive role; for example, in determining what proportion of total public expenditure is to be devoted to scientific research. . . . However, when science policy comes to be implemented in detail, involving the allocation of resources between particular areas of research, the technically competent scientific elite assumes the dominant role. At this more specific level, the deliberations of elite members are usually shielded from direct pressure and, consequently, the decisions which ensure tend to be based almost exclusively on considerations of scientific significance" (p. 463). Mulkay speculates that relationships are likely to be somewhat different in the United States. Because "the university system and the sources of financial support are much more diversified and uncoordinated than in Britain, [it] has been argued that . . . the American scientific community is more internally divided, more competitive, and hence, more innovative" (p. 464). However, the question still remains as to how these organizational differences between Britain and the United States determine whether scientific elites in the United States are more or less effective in pursuing their own interests than in Britain.

The division of influence between scientists and laymen in making public policy choices about science is mirrored in the criteria used for making these choices. One useful way to address the criteria issue is to recognize the importance of the "social merit" of scientific programs. Where the criterion of social merit is added to the funding equation, assessments can still be made on the basis of the achievements of workers in the field in question and of reflective opinions by qualified scientists about the ripeness of a field for further important developments and its implications for progress in adjacent fields. Other "external criteria," however, may be added on the basis of the urgency of the practical problems to which the prospective knowledge is to be applied. This is essentially the issue addressed by I. Th. M. Snellen in final selection of this reader, "Social Merit as a Criterion of Scientific Choice: Its Application in Dutch Science Policy."

Snellen begins his discussion by acknowledging that the "urgency of practical problems is difficult to assess, yet that is what must be done if the criterion of 'social merit' is to be applied" (p. 16). He observes that cost-benefit and cost-effectiveness analysis using nonmonetary measures have helped in making these decisions, but the determination of the relative merits of different, alternative objectives is still a difficult procedure.

A new way for deciding which objectives are more urgent has been applied to R&D policy choices in the Netherlands. This procedure uses a methodology that compares pairs of alternative programs reducing the arbitrariness of the estimates of costs, benefits and other effects anticipated from the competing objectives and from the proposed research itself. These methods are used mainly to provide a basis for comparing the results of qualitative arguments with those of quantitative judgements. The application of the criterion of social merit to the choice of research programs has become an established practice in the Netherlands, implemented through sectoral councils for science policy which develop long-term plans for governmentally-supported, applied research. The main task of the sectoral councils is to advise the minister concerned as to preferred directions for scientific research to serve practical objectives in their sector. The jurisdiction of the sectoral councils is limited to applied research; research which is primarily of scientific or intellectual interest falls outside their purview. Perhaps most importantly, "[t]he sectoral councils are composed of representatives of scientists working in the field, of governmental departments and of users—both 'productive' and 'consumers'—of the desired scientific results. Thus, interest, competence and political representation are combined in the composition of the sectoral councils and other advisory committees" (p. 20).

The job of the sectoral councils in defining "social needs" is by no means any easy task, since demographic changes can surprise forecasters and opinions vary as to the significance to assign to a potential problem. Nevertheless, through the use of policy analysis and broad, general ideals, the councils have been able to develop "policy-trees" that can be used to generate topics for research and to assess proposals. Thus, the Dutch sectoral councils, working within the framework of a comprehensive national policy for scientific research, have been able to develop and apply a method (albeit tentative and inexact) for channeling governmental research support that addresses the pluralistic structure of the interest groups that are concerned with applied social research.

Snellen admits that many very important issues have not been addressed by this procedure, including the proportion of the total funds which should be devolved to basic verses applied research, the allocation of funds for the training of scientists, and the distribution of the best scientific talents between research decided on the basis of social merit and research projects chosen on other grounds. "All this notwithstanding," he concludes, "the work of the Dutch sectoral councils is a genuine contribution to the international analysis of the criteria of scientific choice" (p. 36).

A. Science Advisory Systems and R&D in Europe: Comparative Perspectives

Policy Sciences 11 (1979), pp. 73–91 V. 11 aug. 1979 p 73-91
© Elsevier Scientific Publishing Company, Amsterdam—Printed in Scotland

S-79 14498

Comparative Approaches to R & D Policy Coordination*

RONALD BRICKMAN
Program on Science, Technology and Society, Cornell University, Ithaca, N.Y., U.S.A.

ABSTRACT

Approaches to the interagency coordination of R & D policy adopted by the governments of five West European countries and the United States are described and analyzed comparatively in terms of four variables: (1) procedures; (2) aims; (3) distribution and sources of influence among participating agencies; and (4) the frame of reference which orders governmental actions in R & D for coordination purposes. Differences stem from general features of policy formation within each system and changes over time in the conception and goals of "science policy". The inherent ambiguities of the coordination concept and the difficulties in defining the "perfectly coordinated" system are the primary obstacles in evaluating coordination formats both within and across systems.

The harmonization of policy initiatives taken by different political actors at different times and towards different ends is one of the most recurrent and vexing problems of contemporary government. The growing complexity of modern society and the extension of the role of government to ever larger areas of social and economic activity have multiplied the pressures for policy coordination.[1] Yet the expansion of governmental tasks has also been accompanied by an increased specialization of objectives and tasks within particular agencies, a tendency toward self-perpetuating relationships between public agencies and their constituencies, and the institutionalization of bureaucratic patterns of behavior—all of which tend to hinder change and mutual adaptation. The interagency coordination of policy has become, under such conditions, both more desirable and more difficult. How governments respond to this dilemma constitutes a major test of their ability to deal effectively with the interwoven problems of advanced industrial societies.

On the whole, scholars have contributed little to the policymaker's understanding of what policy coordination entails and can accomplish. Despite the familiarity and importance of the notion in policymaking and administration, we lack a clear formulation of the range of possible formats of coordination, the development of criteria of evaluation in choosing one format over another, and an identification of the factors which condition the objectives, procedures, and results of policy coordination in different settings. Indeed, "coordination" has the dubious distinction of being one of those catch-all, over-abused and vaguely defined terms in our political vocabulary which, unlike such similarly elusive notions as "freedom" and "democracy," has not been clarified through a long tradition of scholarly analysis.[2]

---

*An earlier version of this paper was presented at the annual meeting of the American Political Science Association in Washington, D.C., September 1977.

[1] Hugh Heclo, "Frontiers of social policy in Europe and America," *Policy Sciences*, 6, 4 (December 1975), 403–421.

The absence of conceptual analysis is accompanied by a general paucity of empirical materials which describe how policy coordination has been attempted in specific situations. Even rarer still are cross-national studies of the phenomenon. Yet it is through the comparison of national experiences that we can perhaps best discover the nature and causes of variation in coordination approaches. Rather than a universally uniform, technical problem of public administration, coordination is revealed through cross-national comparison to be a complex mode of intragovernmental relations which reflects both the general processes of policy-making of a political system and the substantive framework of the policy being coordinated.

We examine here how the governments of six countries—the United States, France, Britain, Belgium, the Netherlands and West Germany—have dealt with the problem of policy coordination in one area of public policy: research and development.[3] The long-standing attention given by virtually all Western governments to coordination in this sector results from a fundamental dichotomy in the way R & D policy is conceived and organized. On the one hand, the major part of governmental interest in science and technology stems from the use of R & D to achieve some broader political, economic or social goal; thus, governments are reluctant to separate R & D programs from the agencies and departments responsible for national defense, agriculture, health, industrial development, and so on. On the other hand, there is widespread sentiment that a nation's capability in science and technology deserves to be treated as a whole and on its own terms; thus mechanisms of overview are required to achieve a balanced, efficient use and development of national intellectual and technical resources. Governments have attempted to reconcile these opposing conceptions by complementing the pattern of interagency distribution of authority over R & D with structures and procedures of coordination.

There are other factors which have given special prominence to policy coordination in the R & D sector. R & D is often regarded as an integral, continuous process where an agency's needs can extend from basic research through more applied stages to the development of a particular process or product. Nuclear energy and space research agencies commonly sponsor research of all types. Other institutions, however, such as the National Science Foundation in the United States or the National Research Development Corporation in the United Kingdom, specialize in basic, applied or developmental research, each of which has many of its own requirements and modes of administration.[4] Complicating further the division of responsibilities in R & D is a lack of correspondence between agency spheres of action and the organizational and intellectual divisions within the scientific and technological communities; a single scientific discipline, area of applied research, or research institution will often be affected by the actions of several agencies. Finally, the intrinsically dynamic character of science and technology and their perceived utility in solving a variety of governmental problems make for constantly changing public demands upon the R & D enterprise. New and politically salient programs of research arise more frequently and rapidly than do public agencies to handle them. Interagency coordination has been a common if sometimes only temporary solution in developing such emerging fields as oceanography, atmospheric sciences, demography, computer applications, and environmental research.

---

[2] The one notable exception, whose discussion of coordination is used to far-reaching purpose, is Charles Lindblom, *The Intelligence of Democracy*, New York, Praeger, 1964. See also Gérard Timsit, "Le concepte de coordination administrative," *Bulletin de l'Institut International d'Administration Publique*, 36 (October–December 1975), 125–148.

[3] This study is based on research extending over a three-year period (1974–1977) and includes some sixty interviews with public officials in the six countries under study.

While these qualities of R & D may limit somewhat the applicability of the analysis to other areas of public policy, they have led public authorities to accumulate a rich store of experience in dealing with the harmonization of policy formulation and program implementation across departmental lines. A systematic, comparative analysis of these experiences can provide important clues to the nature, limits and possible achievements of this increasingly critical dimension of contemporary government.

The Dimensions of Variation

How do approaches to the coordination of policy vary? In the public administration literature, two types of coordination are discussed with some frequency. "Central", "top-down", or "vertical" coordination all refer to a centralized authority bringing together disparate actors and actions and fashioning them into some kind of coherent whole. "Horizontal" or "decentralized" coordination refers to exchanges among peers, with no single authority having ascendancy over the others.[5] While sufficient for some purposes, the distinction fails to do justice to the inherent complexity of the coordination concept or to the considerable variety in coordination processes and ambitions encountered in practice. Our cross-national review of interdepartmental relations in the R & D sector suggests four analytically distinct components of the coordination concept:

(1) the procedures by which the actions and intentions of the participating actors are made known and brought into a single frame of reference; (2) the type of coherence sought as a goal of the coordination effort; (3) the relations of authority and

---

[4] In the United States, however, the National Science Foundation accounted for only 16% of Federal expenditure on basic research in 1974 (National Science Board, *Science Indicators 1974*, Washington, D.C., GPO, 1975, p. 55); in France, the Centre National de la Recherche Scientifique accounts for 15% of total governmental expenditure on basic science, or 42% once general university subsidies are excluded ("Les moyens consacrés par la France à la recherche et au développement en 1969," *Le Progrès Scientifique*, Special Issue, 1972, p. 61).

[5] These types are discussed by Lindblom, op. cit., and W. Henry Lambright in U.S. Congress, House of Representatives, Committee on Science and Technology, Subcommittee on Domestic and International Scientific Planning and Analysis, *Interagency Coordination of Federal Scientific Research and Development*, Special Oversight Hearings, 94th Congress, 2nd Session, Washington, D.C., Government Printing Office, 1976.

influence among the participants; and (4) the cognitive frame of reference which determines which actions are selected and how they are analytically juxtaposed for coordination purposes. Each of these components comprises in turn several variants or dimensions of variation. Together, they provide an analytical grid with which national experiences can be described and compared.

Procedures of Coordination

Procedures of coordination refer to the various ways agencies communicate their policy intentions and programs to each other or to a "coordinator", and to how this information is processed with a view to uncovering mutual incompatibilities. These procedures range from various formats of information exchange (informal or formal communication of projects and programs, interagency representation on agency decision-making boards, periodic meetings of agency officials) to such techniques as budgetary review and mutual planning. Coordinating procedures vary along such dimensions as: *time frame* (whether the process involves post facto review programs, examination prior to final authorization or long-term planning); the *content* of the information (financial, substantive, evaluative); *comprehensiveness* (whether all R & D programs of a particular agency, or only some according to such criteria as level of resources committed, enter the coordination process); and *scope* (the range of national policy falling within the coordination process).

Procedures used in one country can contrast markedly with those employed elsewhere. The French, for example, have emphasized annual budgetary formulation and medium-term planning in their R & D policy coordination. This approach attempts to integrate disparate agency programs and allocative decisions in a comprehensive package before resources are committed. Both procedures involve the interdepartmental communication of R & D information of both a financial and substantive nature. Yet certain important segments of the public-sponsored R & D effort, notably military research and civilian aeronautical projects, are excluded.

The Belgians and Dutch also have nominal "research budgets", but they are more compilations of allocative measures already taken than a framework of decision involving trade-offs and mutual compromise. Neither goes into a detailed examination of the substantive content of agency programs, but both are virtually complete in the range of governmental activity in R & D incorporated in the interagency review. The Belgians and Dutch use planning only in a few narrowly defined areas; these take the form of "national programs" or plans which entail a cabinet-level commitment of resources to meet goals in such fields as nuclear energy, the environment, or the social sciences. The Belgians also have relied heavily on an ambitious attempt of information exchange by means of a periodic inventory of all research underway in the country's scientific institutions, both public and private. This approach is founded on the principle that rational decisionmaking in R & D depends on information which is as "global, complete and analytical as possible".[6]

---

[6] Services du Premier Ministre, Programmation de la Politique Scientifique, *Apercu Statistique sur le Potentiel Scientifique et Technique de la Belgique, 1963–1971*, Brussels, SPPS, 1975, p. 17.

The West Germans have eschewed both annual budgeting and joint planning in their coordination efforts. Since 1974, their approach is based on an elaborate system of interagency communication of departmental projects and plans. Unlike the Belgian format which focuses on on-going research at the grass-roots level, the Germans stress the communication of proposals for sponsored research at the agency level. For projects involving financial resources over a certain threshold, detailed summaries of the project's nature, funding recipients, and financial implications are distributed before final action is taken. These, together with the description of less costly initiatives, are stored in a data bank for future reference. Each ministry having an important R & D program is also required to draw up a statement of its research plans during a five-year interval. These are discussed in meetings of an interdepartmental committee of civil servants.[7]

The British and American procedures of interagency coordination have been much less systematic. Both countries have relied on informal, *ad hoc* arrangements, either sporadic consultations taken on the voluntary initiative of the officials concerned, or committee meetings organized to deal with special topics that overlap agency jurisdictions. In the United States, a limited role of coordination has been performed through the budgetary process by the Office of Management and Budget.[8]

Aims of Coordination

Procedures of coordination describe how agencies interact in order to coordinate, not what they hope to achieve. Yet coordination entails an aim as well as a process. Aims are the standards of coherence in the set of actions by which the appropriateness of any single action or proposal is assessed. In practice, these standards are rarely spelled out with clarity. With them, however, policymakers establish, if only implicitly, whether "coordination" is needed, advanced, or achieved.

In the coordination of governmental R & D activity in West European countries and the United States, one can distinguish three principal alternative aims which govern coordination efforts. These three aims, here labelled atomistic, low, and high coordination, vary essentially in the nature of the framework which is used to establish the "order" or "coherence" among agency actions.

Atomistic coordination

In atomistic coordination, there is no explicit, overarching framework in which the suitability of agency actions can be unambiguously judged. Instead, each agency navigates as a free agent, maneuvering to achieve its ends in light of its perceptions of the actions of other agencies. In Lindblom's terms, a level of coherence is achieved through "partisan mutual adjustment" where individual agencies and actors voluntarily change their policies or behaviors in order to avoid adverse consequences of their decisions on each other.[9] There is no effort to arrive jointly at comprehensive, integrated policies which guide or bind future agency decisions.

---

[7] Bundesminister für Forschung und Technologie, *Koordinierung der Forschungs- und Entwicklungsaktivitäten der Bundesregierung, Grundsätze und Verfaheren*, Bonn, BMFT, August 1976.

[8] For a discussion of the role of OMB in U.S. science policy, see James Everett Katz, *Presidential Politics and Science Policy*, New York, Praeger, 1978, 62–98.

Atomistic coordination tends to prevail in the more unstructured forms of interagency exchange where civil servants communicate information about their respective programs and plans without any obligation to arrive at mutually acceptable adjustments. It has been the dominant mode of operations in such interdepartmental forums as the German Interministerial Committee for Science and Research before 1974 and the Federal Council of Science and Technology in the United States, and has characterized interdepartmental relations in Britain throughout a succession of organizational reforms in science policy. The primary concern in atomistic coordination is to insure that mutual awareness of agency intentions and activities is sufficient so that adverse consequences are recognized and taken into account. Thus, elaborate systems of information exchange, such as those in Belgium and West Germany, are set up with atomistic coordination as the goal whenever more ambitious standards are not or cannot be applied.

Low coordination

Low coordination differs from the atomistic form by endeavoring to bring agency policies and activities into line within an overall framework inspired by rational-technical criteria. The objectives to which the coordination process aspires are defined in terms of such managerial standards as the avoidance of duplication, the rational division of tasks and responsibilities, the resolution of mutually defeating policies, the joint management of mutually supportive efforts—in short, the efficient allocation and utilization of available resources. Incompatible efforts across agencies are identified and adjusted in light of these relatively straightforward (but hardly unambiguous, as applied to something as intrinsically uncertain as R & D) criteria. Criteria of efficiency also guide the self-motivated actions of an agency in atomistic coordination. But in low coordination, they are explicitly evoked and uniformly applied in reconciling agency differences; there are fewer opportunities for idiosyncratic interpretation. Often, they are the operating principles guiding the actions of an explicitly designated "coordinator".

Low coordination tends to prevail where the structures and processes of interagency interaction have gone beyond the atomistic level but fall short of establishing a single, comprehensive policy framework. It has characterized the coordination efforts of the Belgian Science Policy Programming Service (SPPS) working through the Interministerial Committee of Science Policy, as well as those of the Dutch Ministry for Science Policy. Since 1974, standards of low coordination have guided the West German Ministry of Research and Technology in its role as interministerial coordinator; without aspiring to either an integrated research budget or comprehensive joint planning, the ministry seeks out inconsistencies and duplication in the six to seven thousand R & D projects initiated each year by the federal government.

---

[9] Lindblom, op. cit., see also Albert O. Hirschman and Charles Lindblom, "Economic development, research and development, policy making: some converging views," *Behavioral Science*, I, 1962, 211-222.

Low coordination is also the usual form of interagency coordination effected by or in close association with a government's budgetary authority, such as the U.S. Office of Management and Budget (OMB). In a typical statement of low coordination aims, OMB has defined its coordinating role through the budgetary review process in the following terms:

> . . . (A)gency R & D requests are examined and evaluated in light of their relevance and appropriateness to agency missions and in terms of their relationship to R & D programs of other agencies . . . (A)s part of this review, budget examiners check with each other to determine whether or not overlap or duplication or gaps would result from agency R & D proposals. In addition, in those cases where R & D is intended to develop information needed for agency program decisions, checks are made to see whether or not other agencies have had similar or related research underway or completed to answer these information needs.[10]

High coordination

For many officials and proponents of science policy, it is insufficient to apply managerial criteria of efficiency to the spectrum of agency R & D activities. One must insure that public investments not only are used efficiently, but also are accomplishing what they should. High coordination involves setting system-wide goals in science and technology, weighing opportunities across sectors and programs, and adjusting the distribution of governmental R & D to a fixed level of resources and resource development. At the limit, it provides an overarching framework of policy directives within which the suitability of any particular program or proposal can be unequivocally established.

High coordination encompassing the entire range of public-sponsored R & D is found in none of the countries examined, but the French have gone furthest in this direction. The French Sixth Plan (1971–1976) was the most ambitious attempt yet to integrate the major part of a government's civilian R & D into an integrated policy framework. The plan gave high and low estimates of available resources over the five-year period, with breakdowns for specific sectors and a ranking of priority projects and policy goals within each sector.[11] Yet the effort had its limitations both in its formulation and execution: it focused on the allocation of newly available resources while rarely considering on-going programs; it gave priority ranking only to a limited portion of the financial package; and it met the almost inevitable fate of plans by failing to receive the necessary governmental funds in the annual budgetary process and by falling victim to unexpected policy developments (such as the oil crisis).[12] The Seventh Plan /1976–1980) has had similar high coordination aims, but gives financial guidelines only for the total research package; otherwise it selects out an unordered list of "priority action programs" which are to receive preferential treatment in resource allocation. The programs covered by these priority areas represented about one-fourth of French public expenditure on R & D in 1975.[13]

---

[10] Letter from Paul H. O'Neill, Deputy Director of OMB, to the Hon. Ray Thornton, U.S. House of Representatives, printed in U.S. Congress, House, Subcommittee on Domestic and International Scientific Planning and Analysis, *op. cit.*, 129–135.

[11] Commissariat Général du Plan, *Préparation du VIe Plan: Rapport de la Commission Recherche*, 2 vols., Paris, La Documentation Française, 1971.

[12] For a critical analysis of the role of the Research Plan through the early years of the Sixth Plan, see P. Papon, "Research planning in French science policy: an assessment," *Research Policy*, 2, 3 (October 1973), 226–245.

The French budgetary device of the "research envelope" also aspires to high coordination. It requires the research agencies and the R & D divisions of ministeries to send their annual budgetary requests, broken down into current operating expenses for approved programs and projected expenses for capital investments, operating costs and personnel costs of new programs, to the coordination secretariat for science and technology, the General Delegation for Scientific and Technical Research (DGRST). This unit combines the requests into a single budget draft which is then negotiated with the Ministry of Finances. Since the latter invariably fails to grant the full amount requested, negotiations ensue among the Ministry of Finances, the DGRST, the political figure responsible for research (since 1977, a State Secretary for Research), and on occasion the Prime Minister. Once the overall contours of the research envelope are known, the distribution of resources among the agencies occurs, a process in which the DGRST plays a pivotal role. The procedure terminates with the incorporation of the R & D allocations in the respective ministries' budgets and the formal approval of the appropriate ministers meeting in joint session.

The distinctive feature of the French approach to R & D coordination through both the planning and budgetary processes is that an effort is made to weave the diverse strands of R & D running through several ministries into a comprehensive policy package where substantive objectives are linked to available or projected resources. While both procedures fall short for a variety of administrative and political reasons, they do provide an integrated framework of decision across agencies.

Similar aspirations towards high coordination are found in other countries on a more limited basis. The clearest examples are the handful of "national programs" which have been developed in Belgium, the Netherlands, and West Germany. These programs commonly put forward a plan of development in a specific sector of research which meets the agreement of the concerned ministeries and receives an explicit commitment (often with financial implications) at the political level. They then serve as the basis for agency activities and interagency cooperation throughout the duration of the program.[14] In Britain and the U.S., the elaboration of common policy frameworks on even limited topics has been rare. One of the more successful efforts in the U.S. was the work of the Marine Sciences Council, which from 1966 to 1971 made considerable headway in bringing together dispersed agency activities into a single frame of reference for mutual development and joint policy recommendations.[15] For the most part, interagency coordination which has produced general policy guidelines for agency behavior in either of these countries has been limited to a few issues of a procedural or managerial cast, such as patents policy for public-supported R & D, public procurement procedures, the dissemination of results, and the administration of international scientific exchanges.[16]

---

[13] Commissariat Général du Plan, *Préparation du VIIe Plan, Rapport de la Commission Recherche*, Paris, La Documentation Francaise, 1976.

[14] D. J. van de Kaa, "National research programmes," *Planning and Development in the Netherlands*, 8, 2, (1976), 114–133; Advisory Council for Science Policy (RAWB), "Advies inzake het nationaal programma voor energieonderzoek," The Hague, December 1976.

[15] Norman J. Padelford and Jerry E. Cook, *New Dimensions of U.S. Marine Policy*, Cambridge, Massachusetts Institute of Technology, National Sea Grant Program, 1971.

The Distribution of Influence and Authority

A third dimension by which coordination schemes vary is the distribution of influence and authority among those participating in the coordination effort. This "power" dimension of the coordination concept is analytically distinct from both the procedures used to effect coordination and the levels of coherence sought—a distinction which is frequently blurred in the standard typology of "top-down" and "horizontal" coordination. It is possible, for example, to achieve high coordination through a process of information exchange and mutual agreement among peers. This happens when agency representatives agree on a plan of joint development. Conversely, some of the ostensibly "centralized" arrangements where a cabinet-level authority is invested with exclusive coordination powers adopt only a weak form of coordination of the "low" variety. An example of the former would be some of the procedural policies formulated through the interagency discussions in the Federal Council for Science and Technology in the United States; of the latter, much of the work of the SPPS in Belgium. Nevertheless, the more one moves from the atomistic to the low, and from the low to the high forms of coordination aims, the greater is the likelihood of differentiating in terms of power and influence the "coordinator" from the "coordinated". This is because the elaboration and application of a common set of criteria to bring coherence into agency actions will usually require the exercise of subjective (i.e. political) judgment. Even in low coordination, the application of rationalistic principles cannot always be effected without some degree of arbitrary discretion: what may be "duplication" to one can be "necessary program acceleration" to another.

The distribution of authority comprises two dimensions: whether it is possible to distinguish a "coordinator" from the "coordinated"; and the extent and nature of the influence which the former exercises over the latter. All of the systems under study have institutions which have been explicitly assigned the task of R & D coordination. In Belgium, the Netherlands, and France, the task is lodged in a staff secretariat headed by a politically appointed minister or state secretary who has no or limited direct authority over R & D programs. In the United States, the role has been given to the Federal Council of Science and Technology (since 1976, the Federal Coordinating Council for Science, Engineering and Technology). The Council has been loosely associated with the Office of Science and Technology Policy which, like the Office of Management and Budget, is part of the Executive

---

[16] For a report and analysis of the coordination efforts of the Federal Council for Science and Technology, see U.S. Congress, House, Subcommittee on Domestic and International Scientific Planning and Analysis of the Committee of Science and Technology, *Interagency Coordination of Federal Scientific Research and Development: The Federal Council for Science and Technology*, Report by the Science Policy Research Division, Library of Congress, 94th Congress, 2nd Session, Washington, D.C., GPO, July 1976.

Office of the President. In Britain, there was no formalized interdepartmental committee after the Central Advisory Council on Science and Technology ceased functioning in 1970; the creation of a committee of chief scientists and permanent secretaries from the departments was announced in 1976.[17] A modest effort of coordination has been undertaken by the small staff of the government's Chief Scientist, attached to the Cabinet Office. In Germany, responsibility for interministerial coordination is with the largest R & D agency in the cabinet, the Federal Ministry of Research and Technology.

Whatever the structural arrangements, the day-to-day business of coordinating R & D among agencies necessarily brings into play the relations of power and influence among these agencies. Even in atomistic coordination, where each agency is left to pursue its own ends as best it can, the "pecking order" of powerful and not-so-powerful agencies will strongly influence how mutual incompatibilities will be resolved.[18] When coordination aims go beyond this level and become the primary function of specialized bureaus, as in France, Belgium, and the Netherlands, or an explicitly mandated task of a line department as in West Germany, the distribution of influence is critical to their success. The history of these institutions is in most respects the history of a struggle for ascendancy over the regular bureaucracies. Since the latter tend to resist the changes which their effective incorporation into a general coordination scheme would entail, coordinating units must rely on the creation and manipulation of sources of influence.

What are these sources of influence? The simple mandate "to coordinate" usually proves insufficient; the history of interagency coordination in R & D is littered with good intentions not backed up by the political resources required to do the job. Thus, one common tactic is to enlist the support of powerful allies. In Belgium, the Netherlands, France and to a lesser extent, the United States, interagency coordination relies heavily on the backing of the ministry of finance or its equivalent. These authorities are natural advocates of R & D coordination on the grounds of fiscal discipline and constraint. The role of science policy coordinating units often amounts to little more than providing the scientific expertise to apply cost/efficiency principles to R & D which the financial authority lacks. Conflict arises, however, as coordinating units often take on an expansionary, promotional orientation for R & D which exceeds allowable budgetary guidelines.

Active support for R & D coordination also comes from legislatures. A clear example occurred in 1974 when the West German Bundestag withheld 20% of the departments' budgetary authorizations for R & D to force their compliance in a more stringent scheme of interdepartmental coordination. The waste, duplication, and dispersion of effort in the executive branch is of course a favorite complaint of legislators everywhere; R & D seems to provide a particularly visible target.

Another means by which coordinating units have gained ascendancy over line departments is to acquire direct control over resources, particularly those which can be used selectively to affect a wide spectrum of agency interests. In the early 1960s, the French DGRST received authority to administer a research fund which gave developmental assistance to industry and financed contract programs in areas "insufficiently taken into account by existing structures and, in particular, by public research agencies". Since then, the fund has grown to a multi-million dollar program which now represents about 8% of the annual research envelope.[19] More recently, the DGRST has received authority to administer the national graduate fellowship program. The Belgian SPPS also administers a fund to develop "centers of excellence" and to encourage research in priority areas; in 1976, resources under its direct control amounted to about 3.5% of the science budget (defined more inclusively than the French).[20] Such resources not only help coordinating units attain their own self-defined aims but also give them bargaining power in dealing with other agencies and departments.

---

[17] Parliament, Commons, The Select Committee on Science and Technology (Science Sub-Committee), *Industry and Scientific Research*; Memoranda, Session 1975–76, 136-i-v, London, HMSO, 1976, 122–127.

[18] See W. Henry Lambright, *op. cit.*, 50–54.

In addition to powerful allies and financial resources—standard fare of bureaucratic politics everywhere—coordinating units resort to procedures which give them better leverage in the coordination process. One common technique is focusing on the coordination of uncommitted resources rather than on on-going programs. One of the more attractive features of joint planning and national programs is the projection of decisionmaking into the future with the expectation of expansion. National programs in Belgium, the Netherlands, and West Germany have been effective vehicles of interagency coordination largely because they in effect create new needs and elicit additional resources to meet them. Both the French annual budgetary and five-year planning exercises concentrate on the allocation of new resources, leaving on-going programs to run their own course.

A less common technique is to change the processes of R & D decisionmaking itself. The cornerstone of the Dutch reforms in the coordination of R & D—still only partially implemented—is the creation of sectoral councils which would incorporate scientists and "users" as well as public officials in a series of advisory bodies dealing with specific R & D sectors. The science policy ministry would provide the secretariat to these councils and assure their coordination, thus giving it a role in a restructured science policy context which it now largely lacks. While the councils are only advisory, the ministry expects that the regular agencies will find the opinions of such democratically constituted, deliberative bodies politically difficult to ignore.[21]

Frame of Reference

A fourth dimension of variation in national approaches to coordination is the frame of reference that defines which governmental actions are included in the coordination effort and how they are organized into analytical categories. This dimension necessarily includes the scope of coordination as discussed in the section on procedures, i.e. the choice of governmental policies and programs subject to inclusion in the coordination net. But the dimension also refers to how the array of activity to be coordinated is conceptualized: on what basis will incompatibilities be uncovered?; through what analytical categories will the coherence of governmental policy be established? Many coordinating units have attempted to put order and method into their activities by developing categories of analysis into which R & D efforts are pigeonholed. Coherence is thus sought within each category as well as among categories. These classification schemes potentially play an important role in determining how disparate programs will be evaluated in relation to each other and where compromises will be effected. The extent to which they actually do define decisional issues is largely a function of coordination aims. In low and high coordination, they fix the parameters of the coordinative net; in atomistic coordination, they serve more as descriptive categories of actions already taken.

---

[19] *Projet de Loi de Finances pour 1976, Document Annexe: La Recherche Scientifique et Technique en France en 1975, Etat Récapitulatif de l'Effort Financier Prévu au Titre de la Recherche en 1976*, Paris, Imprimerie Nationale, 1975, p. 156.

[20] Services du Premier Ministre, Programmation de la Politique Scientifique, *Programme Budgétaire de la Politique Scientifique*, Brussels, SPPS, October 1975.

[21] The reforms are described in Minister for Science Policy, *Science Policy in the Netherlands*, Summary of the memorandum of 16 December 1974, The Hague, 1974; and C. H. Stefels, "The memorandum on science policy," *Planning and Development in the Netherlands*, 8, 2, (1976), 94–113; also interviews.

The French, for example, use a classification system of fourteen "sectors" which correspond roughly to broad disciplinary groups in science and areas of applied research, plus a four-fold scheme organized by "finality" (pure research, industrial research, socio-economic research, and big science). The Belgians have adopted a classification of programs and policies by "objective" (national defense, health, improvement of living conditions, etc.) as well as by "mass"—broad categories defined by the destination of funds (general university funds, industrial and agricultural research, and international programs). The Dutch conceptualize their R & D expenditures in terms of operating costs and wages, big science, capital investments, and international programs. Whenever the British or the Americans have launched interagency coordination efforts, the criterion of selection has usually been a substantive area of scientific endeavor where agency responsibilities overlap: atmospheric sciences, marine technology, etc. The OMB also prepares its "Special Analysis P" as an appendix to its annual budget. This statement summarizes federal R & D expenditures in such cross-departmental rubrics as facilities and conduct of R & D and basic research, as well as by broad sector (defense, space, and civilian) and agency.[22]

All of these formats share the common perspective of considering policy coordination only in relatively narrow terms of governmental R & D objectives and the allocation of resources. Occasionally, the purview of the coordination effort casts a wider net, as the Belgians have tried to do by including both publicly and privately financed research in their national inventory. But coordination efforts which go beyond agency policies that initiate research programs through governmental funding are rare. One could assess, for example, the coherence of a broader range of governmental actions with reference to their cumulative impact on the well-being of particular research institutions, such as universities; this would lead to adjustments not only in the multiple demands for sponsored research emanating from several public agencies, but also in other governmental actions, such as tax and manpower policies, which impinge upon universities. Similarly, interagency policy coordination could have as its point of reference the capacity for technological innovation in the private sector, and incorporate in its purview not only governmental programs of technological incentives and enhancement but also environmental regulation, general tax policies, public procurement practices, and tariff measures—all of which are suspected of influencing the R & D strategies of firms. In many countries, background studies which broaden the parameters of coordination in this manner have been undertaken by advisory panels and others, but these rarely seem to have led to policy changes.[23]

---

[22] Office of Management and Budget, "Special Analysis P: Federal Research and Development Programs" in *Special Analyses, Budget of the United States Government, 1978*, Washington, D.C., OMB, January 1977, 290–311.

Factors Influencing the Choice of Approach

One conclusion which emerges from this analysis of R & D policy coordination in six Western governments is that national approaches *do* vary. There is no single, universally applicable conception of what R & D policy coordination should achieve or how it should be undertaken. It is also apparent that interagency coordination is hardly a simple technical problem of public administration, but is rather a complex matter comprising a range of possible goals, procedures, structural and influence relationships and parameters of decision.

Given the diversity inherent in the coordination concept and the heterogeneity of national approaches, what considerations enter into the choice of one approach over another? Our review of national experiences in the R & D sector over the years suggests two main factors. First, the possibilities of any particular government in the matter are constrained by underlying general features in policy formation in the political system. Second, interagency coordination is affected by the vicissitudes of "science policy"—the way its political saliency and its perceived relationship in meeting desired policy goals have shifted over the past twenty to thirty years. The former factor has been essentially constant over time within a political system and relates to some of the more enduring differences in political organization across systems. The latter factor takes into account the roughly parallel evolution in conception and nature that science policy has followed in all six countries.

Coordination and General Policy Process

Notwithstanding some experimentation over the years, the features of a particular country's approach to interagency coordination have shown remarkable constancy and these are linked to some of that system's more immutable characteristics of politics and policy. Great Britain provides a case where the debate over the nature and functions of "science policy" has been long and intense, where there have been frequent ministerial reorganizations that have touched the sector—and where the format and fortunes of interdepartmental coordination have shown little change. Central overview of the range of departmental efforts in research and technology has always been weak; high coordination, virtually non-existent. From the review of British science policy, there are clearly only two options available for the governmental organization of science and technology: redistributing line authority over R & D among the departments, and atomistic coordination.[24] This situation ultimately derives from the sacrosanct principle of ministerial responsibility, according to which the political appointee heading a department is responsible for the actions of his department. This principle reinforces the resistance of the regular departments to any increase in the policy-making and overview functions of the Prime Minister's staff, kept to relatively modest dimensions in every respect.[25]

---

[23] Such background studies are generally the task of science policy advisory units, which exists in one form or another in all six countries studied. These advisory councils have had a limited impact on policy in recent years. See Ronald Brickman and Arie Rip, "Science policy advisory councils in France, the Netherlands and the United States, 1957–77: a comparative analysis," *Social Studies of Science*, forthcoming.

Indeed, the absence of high coordination ambitions and accomplishments is a hallmark of all those parliamentary regimes—Belgium, the Netherlands, West Germany (and Canada)[26]—where decisionmaking in the executive branch rests on the collective shoulders of the cabinet. The assignment of ministerial portfolios to political figures in large part on the basis of their political strengths and party affiliations gives political reinforcement to the natural tendencies of departmental bureaucracies to defend their self-defined policy objectives and programs. Political realities in these systems dictate a form of low coordination with carefully delimited measures of overarching policy (high coordination) occurring only through the voluntary cooperation of ministeries having mutual interests or of a procedural nature. Even here, however, the achievements of interagency coordination will be affected by prevailing political conditions; coordinating officials often encounter greater difficulty in achieving their ends when the current governmental majority is thin or built upon a coalition of parties.[27]

Both France and the United States diverge from the parliamentary-cabinet formation model of government, and both have idiosyncratic approaches to interministerial coordination. It is probably no accident that an integrated approach to science policy in France had to await the advent of the Fifth Republic. The major reforms in structure and procedure in French science policy were in fact one of the first initiatives taken by De Gaulle following his return to power.[28] The "high road" taken in France in R & D policy coordination has no doubt been facilitated by such Fifth Republic transformations as the decline in legislative control over the executive, the increased representation of managerially oriented administrators in the cabinet, refinements in the Plan as a framework of general policy development, and the reinforced roles of policy integration and formulation assumed by the president and the prime minister.

---

[24] J. B. Poole and Kay Andrews, eds., *The Government of Science in Britain*, London, Weidenfeld and Nicolson, 1972; Norman J. Vig, *Science and Technology in British Politics*, Oxford, Pergamon, 1968.

[25] For an analysis of the difficulties of the Central Policy Review Staff in the Cabinet Office, see A. Mencher, "Recent Developments in British Science Policy," in U.S. Congress, House, Committee on Science and Astronautics, *Federal Policy, Plans and Organization for Science and Technology*, Part II, Hearings, 93rd Congress, 2nd Session, 1974, 815–817.

[26] For an early analysis of the problems of coordination encountered by the Canadian Ministry of Science and Technology, see Peter Aucoin and Richard French, "Knowledge, Power and Public Policy," Science Council of Canada Background Study No. 31, Ottawa, Information Canada, November 1974.

[27] The point was made in interviews with science policy authorities in West Germany, Britain, Belgium, and the Netherlands.

[28] The best introduction to French science policy in the early years of the Fifth Republic is Robert Gilpin, *France in the Age of the Scientific State*, Princeton, Princeton University Press, 1968.

The United States is something of an exception to the general pattern: the most "presidential" of the six systems, it also has one of the weakest forms of interagency coordination. Students of American policymaking and administration readily account for this anomaly by evoking the decentralized nature of policymaking and administration of the American executive branch. Agencies and departments enjoy considerable autonomy.[29] The role of the President is less oriented to policy harmonization or the elaboration of national policies cutting across departments, than to an item-by-item consideration of proposals and problems as they appear on the political agenda. New or suddenly salient issues that cannot be handled by an existing agency lead to reorganization, often entailing the creation of a new, independent agency. NASA, the Environmental Protection Agency, the creation of the National Oceanic and Atmospheric Administration within the Department of Commerce are examples of institutional solutions which have forgone interdepartmental coordination. This decentralized pattern is reinforced by the committee system of oversight and authorization in Congress, whose jurisdictional divisions often parallel those of the executive branch.[30]

The Emergence and Evolution of "Science Policy"

A second factor in the way governments have approached coordination in the R & D sector derives from changes in the governments' views of the nature and importance of "science policy". As science and technology emerged and evolved as a focus of public decision, different requirements were made of interagency coordination. In contrast to the first factor, where underlying features of political organization have imposed essentially constant limits on the extent and nature of interagency relations in a given country, this factor shows both variation over time and a parallel evolution in several countries. Nonetheless, its impact on coordination has worked itself out differently in each country partly because of the nation-specific features discussed previously.

The call for coordination itself appears almost as old as public involvement in science. As early as the 1840s and 1850s in the United States, according to Dupree, "the need for some kind of coordinated effort became more apparent with every accretion to the government's scientific establishment."[31] Similar concerns were voiced in France and Britain in the interwar period.[32] Following World War II, the United States and France assigned overall coordinating responsibility to newly created science research councils, while Britain established an advisory council to the cabinet comprising both non-governmental scientists and civil servants. None of these institutions was very active or successful in its coordination tasks.[33] Governments were more concerned with establishing and developing specialized institutes and programs in such fields as nuclear energy, space, and pure research. In this period of rapidly increasing expenditures and institution-building, coordination was clearly subordinated to expansion.

---

[29] Sanford A. Lakoff, "The Scientific Establishment and American Pluralism," in Sanford A. Lakoff, ed., *Knowledge and Power*, New York, The Free Press, 1966, 377–392.

[30] Sanford A. Lakoff, "Congress and national science policy," *Political Science Quarterly*, 89, 3 (Fall 1974), 589–611.

[31] A. Hunter Dupree, *Science in the Federal Government*, Cambridge, Belknap-Harvard University Press, 1957, p. 115.

[32] Gilpin, *op. cit.*, p. 158; Poole and Andrews, *op. cit.*, 195–200.

Toward the end of the 1950s, however, the overall financial and political implications of governmental sponsorship of R & D could no longer be ignored. The development of expensive programs in high technology and the increased responsibilities of governments for funding basic and applied research brought the sector into greater political focus. The trend was abetted by the demands for further support by an increasingly sophisticated constituency, as well as by such galvanizing events as the Sputnik crisis in the United States and the technology gap debate in Europe. Governments were concerned that their R & D efforts proceed in a somewhat less haphazard fashion than in the preceding decade. Such newly created coordinating units as the DGRST in France, the SPPS in Belgium, the Ministry of Science in Britain, and the Office of Science and Technology in the United States were concrete signs of the increased importance attached to the coordination function. Their efforts included taking stock of the array of on-going programs, ensuring sustained and balanced growth, avoiding unwarranted duplication and guiding the allocation of new resources to perceived gaps in the national capability.

In the late 1960s, the era of coordinating in times of expansion came to an end in most of the Western countries.[34] A leveling off and even, in some cases, actual decline in the rate of increase in public expenditures on science and technology, were accompanied by an enhanced concern for the social and economic payoffs of public R & D investments.[35] At the same time, greater political control over the course of R & D was exerted; resources were allocated—and their results evaluated—according to "objectives" and "missions"; open-ended grant schemes gave way to more directed forms of funding such as contractual research. Governments were less inclined to meet new scientific and technological demands by the creation of new institutes and agencies. As R & D programs became more closely associated with specific objectives as defined by political authorities, the structures and processes of interagency coordination reflected the shift in emphasis. In those countries which went furthest in giving science and technology policy status in themselves, such as France, the policy guidelines for R & D were the basis of high coordination; they became the means by which coordinating units attempted to bring the programs of agencies into line. Elsewhere, especially in Britain and the United States, the effort to gain firmer political control and to tie R & D to other policy missions became the justification for a decentralized mode of decisionmaking. The natural tendencies toward atomistic coordination in these countries found support in the new fashion of conceiving science policy in terms of instrumentalities.[36]

---

[33] For Britain, see Vig, *op. cit.*, 23–24; for the United States, U.S. Congress, House, Subcommittee on Domestic and International Scientific Planning and Analysis, *Interagency Coordination . . . The Federal Council for Science and Technology*, *op. cit.*, 46–55; for France, Gilpin, *op. cit.*, 162–164.

[34] Organization for Economic Cooperation and Development, *Patterns of Resources Devoted to Research and Experimental Development in the OECD Area, 1963–1971*, Paris, OECD, 1975.

[35] Bruce L. R. Smith, "A new science policy in the United States," *Minerva*, 11, 2 (April 1973), 162–174; Mencher, *op. cit.*; Ronald Brickman, "French science policy and the changing role of the university," *Research Policy*, 6, 2 (April 1977), 128–151. The famous "Brooks Report" was an influential statement of emerging trends in science policy: Organization for Economic Cooperation and Development, *Science, Growth and Society*, Paris, OECD, 1971.

More recently, the coming of age of science and technology policy has put somewhat different strains on interagency coordination. Dramatic institutional changes and shifts in the allocation of resources are now less common; annual budgetary outlays continue to show at best only slight increases in constant dollars. Governments are now taking a decidedly managerial attitude to what they have created. Existing institutions are incited to make better use of their facilities and resources, and new tasks must be met in part by recycling existing resources. The thrust of contemporary science policy is indicated by such watchwords as "utilization", "reconversion", "redeployment", and "mobility".[37] Pre-established areas of competence among agencies are not always the best suited to effecting shifts in existing resources to meet new demands. Thus, the situation has renewed the calls for coordination in many countries. But coordinating units like the DGRST, accustomed to coordinating through the selective allocation of new resources, have experienced difficulty adjusting to the new context of science and technology decisionmaking.[38] Others practicing low coordination, such as the Dutch Ministry for Science Policy and the West German Ministry of Research and Technology, have profited from the impetus provided by the R & D management orientation. Indeed, one of the more notable accomplishments of the former was the implementation of the government's policy of budgetary restriction in 1976. The necessity to trim programs across the board gave the Dutch ministry an arena of action which otherwise had been largely absent.[39]

Evaluating Coordination

It follows from the preceding analysis that national approaches to R & D policy coordination cannot be ranked along a single evaluative scale. Since these approaches comprise different conceptions of what coordination is or should be, there is no "one best way" to coordinate which can provide a standard for the relative success or failure of any particular format. It is thus pointless to ask whether the French approach is qualitatively superior or inferior to the British or German approach; they are simply different in their ambitions, procedures, organizational arrangements, and frames of reference.

---

[36] President's Message and Text of Reorganization Plan No. 1, reprinted in U.S. Congress, House, Subcommittee of the Committee on Government Operations, *Reorganization Plan No. 1 of 1973*, Hearings, 93rd Congress, 1st Session, February 26, 1973, 98–100, and testimony of F. Malek, Deputy Director, Office of Management and Budget, *ibid.*, 3–5; Parliament, Commons, The Select Committee on Science and Technology, *op. cit.*

[37] Some recent contributions to the literature exploring these themes include Albert H. Teich and W. Henry Lambright, *Redeploying Big Science: A Study of Diversification at Oak Ridge National Laboratory*, Albany, The Institute for Public Policy Alternatives, 1975; Michael Gibbons and Philip Gummett, *Redeployment in British Government Research Establishments*, Manchester, Department of Liberal Studies in Science, Manchester University, 1975; Pierre Vautier, "La recherche dans le VIIe Plan," *La Recherche*, 69 (July–August 1976), 684–686; Ronald Brickman, "The promotion of mobility of scientists: a problem of French science policy," *Minerva*, 15, 1 (Spring 1977), 68–82.

[38] Brickman, *ibid.*

[39] Interview comments, Ministry for Science Policy, March 1976.

Nor can any single approach be readily evaluated on its own terms. As previously noted, the governing conceptions and aims of national systems of coordination in R & D are rarely spelled out and practiced with clarity and consistency. Through empirical observation, we can discover what occurs, but not in many cases discern the expectations which would provide an unequivocal point of reference for evaluative purposes. In any particular system, "high", "low" and "atomistic" conceptions co-exist and constantly shift as the operative modes of coordinative behavior; alternative frames of reference struggle for recognition in determining how the coherence and consistency of governmental action will be defined. There is chronic uncertainty among public officials about the effectiveness of various mechanisms of coordination and about the adequacy of existing interagency relations of authority and influence.

These ambiguities prevent ready-made answers to the questions which are constantly raised by public officials: are existing forms of interagency coordination working and how can they be improved? The answers presuppose that an "ideal" state of the perfectly coordinated system can be known and thus worked towards. Yet such an ideal cannot be defined substantively in the atomistic form of coordination; The "perfectability" of atomistic coordination can at best only be assumed once agency actions and intentions are fully communicated and taken into account. But at some point, decisionmakers must ask whether elaborate systems of information exchange are worth the effort.[40] Low and high coordination, in contrast, provide their own guidelines of the perfectly coordinated system: rationalistic, managerial criteria of efficiency in the first case, the substantive policy goals in the second. In principle, these criteria are standards which can be used to evaluate the effectiveness of coordinating efforts; in practice, their utility is limited, either because they cannot be unequivocally applied to the range of R & D activity, because they are incomplete or too general to indicate where the shortcomings are and how they can be corrected, or because policy success or failure depends on other factors than the effectiveness of coordination.

The uncertainties involved in defining the perfectly coordinated system have led many simply to equate the effectiveness of coordination with the ability of the "coordinator" to get his way. This reduces interagency coordination to a power game, where effectiveness depends above all on political direction and support accorded to the coordinating authority. This notion is clearly inapplicable to atomistic coordination, where there is no coordinator (except possibly for purposes of interagency communication). There, as already noted, inequalities in political resources (e.g. small agencies versus big agencies) may lead to adjustments dictated by the balance of power, not by the mutually defined best interests of the R & D effort. In low and high coordination, an increase in the ability of the coordinator to induce change in others does not necessarily make for better management or better policy for at least three reasons, all familiar arguments against central control: (1) the idiosyncratic needs and considerations which motivate R & D decisionmaking at the agency level would be taken less into account by a central coordinator [41]; (2) the capacity for making costly errors increases with centralized control; and (3) there is, in LaPorte's terms, a widening gap between the complexity of a situation and the conceptual capacity to understand it.[42] A further limitation of the induced-change approach to evaluating coordination efforts is its methodological ambiguity: if the ability of the coordinator to provoke change is the measure of effectiveness, then the absence of change should signal ineffectiveness. But the absence of change can also mean that "perfect" coordination is already achieved.

---

[40] Thus, the elaborate system of communication of agency projects and proposals in West Germany creates a data bank with over 6,000 new entries yearly; yet only 100–200 inquiries a year were received and, among the larger proposals, overt agency conflict occurred over about twenty only. (Interview with officials of the Ministry of Research and Technology, March 1976).

These difficulties encountered in evaluating coordination efforts, together with the inherent ambiguities and potential for variation which the notion masks, lead to two conclusions of apparently universal validity. First, interagency coordination is an intrinsically frustrating, unglorious enterprise whose success story will probably never be written. Second, the contours of any coordination effort—what it attempts to achieve and how it is undertaken—will reflect the topographical features of its political terrain: the fundamental features of policymaking and interbureaucratic politics of each political system, and the political vicissitudes of the policy area whose coordination is attempted.

---

[41] The classic arguments for and against centralized control of science policy in the U.S. context are presented by Harvey Brooks in *The Government of Science*, Cambridge, M.I.T. Press, 1968, 1–18.

[42] Todd R. LaPorte, *Organized Social Complexity*, Princeton, Princeton University Press, 1975, p. 339.

● *ABSTRACT*

The evolution of high-level science advisory councils in France, the Netherlands and the United States is reviewed in the light of common trends in the relationship of science and government over the past twenty years. The creation of the councils came as a response to similar needs of governments in the domestic and international spheres. The subsequent decline of the advisory councils in all three countries is attributable to the 'bureaucratization' of science policy-making, a slower rate of growth in R&D expenditure, a more goal-oriented approach in governments' sponsorship, changing public attitudes towards science and technology, and the intrinsic difficulties faced by the council members in acting both as scientists and as political advisors. The revitalization of the councils in the mid-1970s was similarly inspired by emerging needs and institutional developments affecting all three governments. In turn, the distinctive features of each council derive from underlying differences in political organization and culture. These differences are revealed in governmental conceptions of science policy; in the role played by scientists in public affairs; and in the organization of scientific and technological interests in political decision-making.

Science Policy Advisory Councils in France, The Netherlands and the United States, 1957-77:

A Comparative Analysis

Ronald Brickman and Arie Rip

The years 1957-77 saw a remarkable period of institution-building in science policy. As governmental expenditures on science and technology rose to unprecedented levels, new institutions and mechanisms of decision-making, interministerial coordination and science administration multiplied in the advanced industrialized nations. In many cases, these organizational innovations included the creation of high-level councils of scientists to advise govern-

Social Studies of Science (SAGE, London and Beverly Hills), Vol. 9 (1979), 167-98

ments on their rapidly expanding responsibilities in science and technology. In the late 1950s and early 1960s, science policy advisory councils, composed entirely or partly of non-governmental practising scientists, were established in the United States, France, Belgium, West Germany, Sweden, and somewhat later in the Netherlands and Canada, among other countries. Some of these grew out of advisory structures which had earlier played a more limited or provisional role; others, notably the Advisory Council on Scientific Policy in Britain, had been in existence since the immediate post-war era.[1] With the increased importance attached to science policy and scientific advice in the Western countries, these councils came to enjoy a privileged voice in the higher councils of government.

These developments did not pass unnoticed by scholars. Political scientists, sociologists and social historians analyzed the nature and extent of scientists' increasing involvement in governmental affairs; science policy advisory councils became familiar examples in works on the post-industrial state, which stressed the emergence of a scientific estate or establishment in both public and private decision-making.[2] The distinction between objective knowledge and subjective opinion, the translation of scientific knowledge into practical policy guidelines, the problems of scientific autonomy versus public accountability, the need for expertise versus the fears of technocracy — these issues received considerable attention at the time and indeed continue to dominate scholarly discussions of the role of scientific advisors.[3]

Since the early 1960s, however, the relationship of science and government has undergone a number of transformations. Subsequent years have consolidated the position of science and technology as a major area of governmental policy. At the same time, there have been far-reaching changes in governmental funding patterns and control mechanisms — in particular, a levelling off in total governmental expenditures on R&D, a re-ordering of priorities, and a more goal-oriented, politically controlled approach in the support of programmes. Political controversies over scientific and technological programmes have increased; the public and political authorities no longer have unqualified faith in science and technology as forces of progress. On the whole, the science policy literature has kept abreast of these developments and has documented in detail the nature of the changes in many of the advanced industrialized countries.[4] But their impact on the role and evolution of science advisory councils has been less well documented. Here we examine how changes in the context and nature of policies for science and technology have altered the activities, composition and institutional status of the councils in the period 1957-77. As prominent points of intersection between the political and the scientific spheres, these councils have mirrored many broad trends in the science-government relationship.

We undertake our analysis within an explicitly comparative framework. A point-by-point comparison of three national cases is designed to answer three questions:

1) What are the most striking similarities in the role and evolution of science policy advisory councils in the three countries, and what are their differences?
2) What are the trends and features in the science-government relationship, common to all three countries, which account for the similarities in the councils' role and development? and
3) What are the unique features of each council's political and cultural setting which best account for the observed differences among them?

Systematic exercises in cross-national analysis of this kind have been rare in science policy studies.[5] Much of the descriptive and analytical literature treats only one country;[6] studies of several countries (like those conducted by OECD and UNESCO) highlight different national institutions and procedures, but only allude to broader social and political features which might explain such variation. The literature on scientific advice, in contrast, examines issues in such general terms that the nature and dilemmas of the governmental role of scientists are assumed to be the same everywhere.[7] Theorists of the 'convergence' school go even further and postulate that the interpenetration of the scientific and political spheres is an increasingly common thread in capitalist and socialist systems alike.[8] This line of thought, together with the tendency of sociologists of science to use universalistic categories of analysis, has obscured the effects of different political and cultural settings on the recent evolution of government and science.

The present analysis is confined to three countries of which the authors have first-hand knowledge: France, the Netherlands and the United States. We have based our analysis on interviews with science policy officials, and with present and past members of the three councils; published reports and opinions of the councils, published commentaries by outside observers, and some archival material completes the documentation.[9]

The Evolution of the Three Councils

In November 1957, President Eisenhower created the President's Science Advisory Committee (PSAC), appointing seventeen scientists as members. Twelve of these had earlier constituted the Science Advisory Committee in the Office of Defense Mobilization. The President also created the office of Special Assistant for Science and Technology, a position whose incumbent would traditionally be a member of PSAC and chair its meetings.[10] The following year, the French government established the *Comité Consultatif de la Recherche Scientifique et Technique* (CCRST) as part of its institutional innovations in the area of science and technology. In addition to the CCRST, the new government of General de Gaulle created the *Délégation Générale à la Recherche Scientifique et Technique* (DGRST), which became a secretariat for the coordination of governmental R&D, and the *Comité Interministériel de la Recherche Scientifique et Technique*, a cabinet-level committee whose task was to prepare for final governmental action the nation's major options in science policy.[11] The Dutch *Raad van Advies voor het Wetenschapsbeleid* (RAWB) came into existence in October 1966 following three years of discussions.[12]

In their formally assigned tasks, composition and procedures, the councils presented a number of similarities. Each was composed of eminent scientists from a range of disciplines and backgrounds (see Table 1). The members advised their governments on a part-time basis (bi-weekly meetings were the norm in all three cases) with the assistance of a small secretariat. PSAC was enjoined by the President to assist the Special Assistant in his duties (primarily to keep him informed of scientific activities in the various federal agencies, to advise on scientific and technological matters in policy deliberations, to anticipate future trends and developments in the area) and in general to be concerned 'with problems of national policy involving science and technology'.[13] The major tasks of the CCRST were to advise the government on all measures affecting the welfare of French science, and specifically to prepare the discussions of the interministerial council with

TABLE I
Disciplinary Background and Professional Affiliation of Members of Science Policy Advisory Councils

disciplinary background	USA PSAC	France CCRST	Netherlands RAWB
physical sciences (incl. mathematics and engineering)	59	40	6
life sciences (incl. medicine)	15	16	4
social sciences (incl. economics)	3	16	5
humanities (incl. legal science)	—	1	5
not categorized	1	2	—
total number of members,[a] 1957-1977	78	75	21
professional affiliation at the time of membership	**PSAC**	**CCRST**	**RAWB**
industrial	16	13	3
academic (incl. research in public institutions)	59	46	18
public official[b]	3	16	—

[a] PSAC consisted (most of the time) of 18 members, of whom one fourth was appointed each year for four-year terms; however, many members sat for shorter periods. CCRST had 12 members up till 1976, when it was enlarged to 16 members; bi-annually, about half the membership is renewed, appointments being made for four-year terms. The 9 members of RAWB are appointed at the same time for a five-year term together.

[b] The category of 'public official' applies to directors of public and semi-public enterprises like Electricité de France, to directors of agencies like Commissariat à l'Energie Atomique and Centre National de Recherche Scientifique, and positions in the USA like Atomic Energy Commissioner and President's Special Assistant for Urban Affairs. The distinction with academic or industrial affiliation (as well as between those two) is not always sharp.

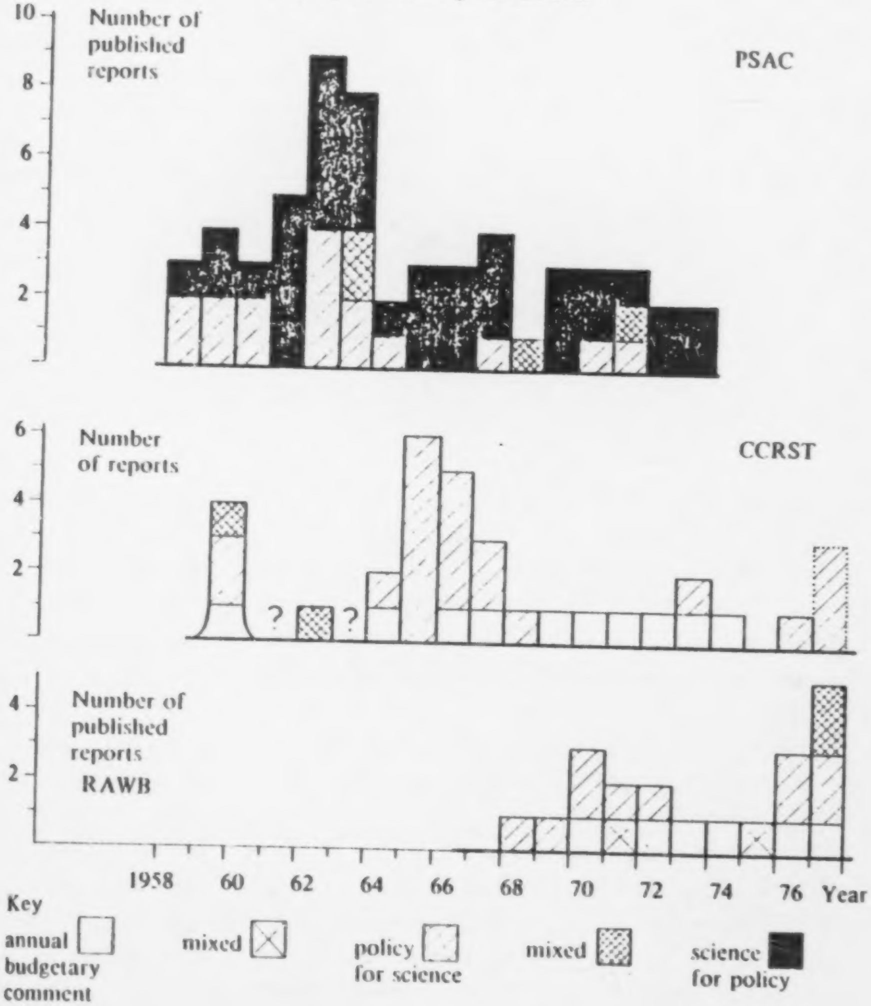
Sources: For PSAC, the list of members up till 1967 in: *The Office of Science and Technology* (a report prepared by the Science Policy Research Division of the Legislative Reference Service, Library of Congress, for the Military Operations Subcommittee of the Committee on Government Operations) (Washington, DC: US GPO, 1967), 257-59, and announcements of appointments in *Science* and the *New York Times*; our data agree with those of Charles Schwartz, 'The Corporate Connection', *Bulletin of the Atomic Scientists*, Vol. XXXI (October 1975), 15-19. Lists of CCRST members are provided by the DGRST; additional information has been taken from *World Who's Who in Science, A biographical dictionary of notable scientists from antiquity to the present* (Chicago: Marquis-Who's Who Inc., 1968) and from *Who's Who in Science in Europe* (London: Francis Hodgson Ltd., 1972). The RAWB secretariat provided names of members and background information.

respect to the allocation of public resources. The terms of reference of the RAWB called for advice on the harmonious development of Dutch science and technology to serve the national interest.[14]

The early years of these three councils were characterized by a strong esprit de corps. All three helped their governments achieve greater coherence and purposefulness in their approach to R&D activities, although the relative importance they attached to different tasks and topics varied from one to the other. In the United States, for example, PSAC was heavily involved in national security issues, even though the committee and its ad hoc panels did address such diverse topics as scientific and technical information networks, the 'strengthening of American science', scientific education, food additives, manpower needs and nuclear physics. From the beginning, the CCRST focussed on the annual science budget (which excluded military R&D). It also issued major reports and policy recommendations on such themes as the reorganization of government-sponsored research centres, terms of employment of public scientific personnel, and applied research in both the public and private sectors. The CCRST played a leading role in the formulation of the government's consecutive five-year research plans.[15] The RAWB began with an ambitious attempt at comprehensive planning in the public R&D sector, and concentrated its efforts on gaining an overview of the range of governmental programmes with a high R&D component.[16] The differences among the three councils in emphasis on 'science for policy' and 'policy for science' are illustrated by a comparative survey of their reports (Figure 1).

There is general agreement that in their first years, the councils performed useful tasks and enjoyed considerable influence in policy-making circles. In an historical appraisal, the former Executive Secretary of PSAC lists a number of occasions on which that committee, often in liaison with the Special Assistant and his staff, played an important role: the creation of several agencies and administrative offices in the R&D sector, including the National Aeronautics and Space Administration and a high-level R&D directorate in the Department of Defense (DOD); the development and, in some cases, curtailment of major technological programmes in the civil and military sectors; the control of the use of pesticides; the effective use of the sea; and the 1969 ban on biological warfare.[17] The CCRST is widely credited with a significant voice in R&D policy at this time: 'From these natural and social scientists (on the CCRST) came most of the proposals for the reform of

FIGURE 1
Annual Production and Nature of Reports of the Science Policy Advisory Councils



Sources: For PSAC, the list of Beckler, op. cit. note 10, has been used as a starting point; the Gaither report (1957), which was prepared by PSAC's predecessor SAC, has been deleted, and five reports that were published with some years' delay have been added: 1960 Report of the PSAC-GAC panel on High Energy Accelerator Physics (2nd Piore Report), published in 1965 by the Joint Committee on Atomic Energy in *High Energy Physics Programs: Report on National Policy and Background Information* (89th Congress, 1st Session); 1962 Report on Land and Water Development in the Indus Plain, released January 1964; 1962 Report of an Ad Hoc Panel on Drug Abuse, published as Appendix 1 to *Proceedings, White House Conference on Narcotic and Drug Abuse* (Washington, DC: US GPO, 1962)

302-08; 1963 Report of the PSAC Space Panels on objectives and means in lunar surface exploration, published in the PSAC report *The Space Program in the Post Apollo Period*, released 9 February 1967; 1966 Report of the Space Technology Panel on the Apollo Program, published in the PSAC report *The Space Program in the Post Apollo Period*, released 9 February 1967.

Nearly all CCRST reports are confidential. A list of titles from 1964 onwards is available from DGRST; about reports in 1959-60, some indications are given in an editorial by Pierre Lelong, the then chairman, in *Le Progrès Scientifique* (December 1962).

A list of published reports of RAWB has been made available through its secretariat.

The nature of all these reports is described with the help of the categories 'annual budgetary comment', 'policy for science' and 'science for policy'; sometimes mixed categories were necessary. The reports have been attributed to the different categories on the basis of the title (CCRST reports, some PSAC reports), of short summaries (most PSAC reports) and of the reports themselves (some PSAC reports, RAWB reports).

French scientific institutions that were implemented in the last months of the Fourth Republic and the early years of the Fifth.'[18] The RAWB was instrumental in instituting and developing the Dutch government's 'science budget' which, while failing to provide a framework for policy decision, nevertheless has served to rationalize policy discussions. The committee also successfully pressed for closer relations among the departments through the creation of a permanent interdepartmental committee of civil servants, and for the establishment of an administrative directorate-general for science policy.[19]

Notwithstanding these early successes, all three councils showed a decline in activities and impact toward the late 1960s and early 1970s. The case of PSAC is the most dramatic. Even though its attention shifted to some of the salient social and political issues of the day, the committee, together with the Science Advisor and the Office of Science and Technology, moved into a position of peripheral importance in Presidential decision making; finally, and not unexpectedly, the committee was disbanded in 1973.[20] The CCRST, meanwhile, became increasingly bogged down in the administrative details of the French science budget. The number of its innovatory reports declined sharply after 1968 (Figure 1); in that year there was some discussion in governmental circles of either making considerable changes in the committee or abolishing it completely.[21] In 1970, both the CCRST and the DGRST lost their positions under the authority of a State Secretary attached to the Prime Minister's office and were transferred to a new Ministry of Industrial and Scientific Development. In the Netherlands, finally, criticism and dissatisfaction with RAWB grew both within and outside the council; in 1971, vacant positions in the membership were not filled and the body became almost dormant.

This parallel decline in the role and vitality of the science policy advisory councils is all the more noteworthy in view of their subsequent revival. In late 1975, a governmental decree was issued which reformed the CCRST and gave the body new life: its membership, more diversified by geographical and institutional affiliations and even in ideological inclinations, was raised to sixteen; in addressing the committee, the Minister of Industry and Research assured the committee of the importance of its activities and issued specific requests for reports on some of the key problems then receiving attention.[22] Since 1973, the RAWB has undergone a similar rejuvenation, with a new chairman and membership, a dedicated staff, and a busy agenda which has led to several reports on substantive R&D programmes and organizational issues (Figure 1).

In the United States, the first signs of renewed interest in high-level scientific advice came with the appointment by President Ford of two advisory groups, one to assess the contributions of technology to economic strength, the other to anticipate advances in science and technology. The next year, Congress passed the National Science and Technology Policy, Organization and Priorities Act which reconstituted the office of Science Advisor in the White House, with staff support provided by an Office of Science and Technology Policy (OSTP). The Act also called for the establishment of a Presidential Committee on Science and Technology, composed of eight to fourteen scientists, engineers and representatives of social groups.[23] While retaining the first two offices, the Carter Administration in 1977 chose to ignore the provision to reinstitute a permanent committee of independent scientific experts in the White House. According to available reports, the Science Advisor and OSTP rely on ad hoc panels of experts drawn from both within and outside government to undertake specific tasks.[24]

Common Factors in the Role and Evolution of the Councils

This brief review of the history of the science policy advisory councils in the three countries reveals a number of similarities and differences. The most striking similarity is that councils sharing so many structural and functional attributes were created in the three countries and went through a similar historical development. This observation suggests two interrelated questions which we attempt to clarify in the following sections: a) to what extent can the evolution of these councils be explained by observable trends and events which are common to all three countries?; and b) to what extent must particular factors, unique to the political and cultural situations of each country, be invoked to account for the differences in the nature, role and areas of influence of each national committee?

Common Factors in the Origins of the Councils

Until the late 1950s, the structural accompaniment to the growth of the science and technology policy sector had been the creation or expansion of mission-oriented agencies for basic science, nuclear energy, space, and so on. Towards the late 1950s, governments perceived the need for central units which would link science and technology to economic growth and competitiveness, diplomatic influence, and national independence. The committees were created to promote overview of the range of governmental effort in the area and to advise on R&D investment opportunities.

International factors were also important. The White House apparatus for science and technology, including PSAC, was a direct response to the launching of Sputnik by the USSR in 1957; from the beginning, this advisory machinery was used in policy deliberations over military technology and in disarmament negotiations with the Soviets.[25] In France as well, the Sputnik crisis was a stimulating factor in the creation of science policy institutions; more generally, French science policy throughout the presidency of General de Gaulle was strongly motivated by the objectives of enhancing French military, diplomatic and economic influence in world affairs.[26] With fewer pretensions of playing a major role in international diplomacy, the Netherlands made no institutional moves in the post-Sputnik period other than the creation of a few ad hoc study commissions on the state of Dutch science; in subsequent years, the creation of RAWB was influenced by the trend towards science policy institution-building in other countries and by the encouragement afforded through the forum of the OECD.[27]

Common Factors in their Decline or Demise

If the creation of the PSAC, CCRST and RAWB responded to more or less comparable circumstances and needs among the three governments at the time, what accounts for their respective decline? Disagreements among members and between the councils and public authorities on particular issues are often advanced as important factors. In France, it was also noted that the lesser prestige of later CCRST members adversely affected the council's influence. In the United States, reference is often made to President Nixon's uneasy relationship with the scientific-intellectual community and his pique in response to the opposition of PSAC (or of individual members) to technological projects favoured by his Administration.[28]

While such circumstances and incidents cannot be ignored, they are unsatisfactory as explanations. Their relevance can be established, in fact, only against the backdrop of more profound changes, discernible in all three countries, in the science-government relationship. At least five common factors appear to have had a significant bearing on the decline of PSAC, RAWB and CCRST:

1) The most strikingly consistent factor across the three national cases is the incorporation of science and technology policy-making into the regular processes of government. During the 1960s, the R&D bureaucracies within the ministries and specialized agencies grew in both size and experience. At the same time, science policy machinery for overview, coordination and policy purposes gained in sophistication and influence. In France, for example, the DGRST developed into a substantial bureaucracy having direct control over major R&D programmes. The French science budget and research plans evolved to the point where their preparation and implementation became the dominant concerns of the agency's activity. In the Netherlands, decision-making was strengthened in the ministries, particularly those of agriculture and of defence. The appointment in 1971 of a Minister for Higher Education and Science Policy, and two years later the creation of a science policy ministry with a more substantial staff, represented further steps in the institutionalization of Dutch science policy. In the United States, the previously noted growth in science-oriented administrations within the national government, as well as in specialized advisory staff within the White House, indicated similar trends.

This 'domestication' of science and technology policy, both within individual agencies and in central policy and coordination units, made outside scientific advice less indispensable and even, to some, rendered the councils superfluous. Several commentators have noted the declining role of PSAC and OST in military technology as the DOD built up its own R&D machinery and as the White House developed a permanent staff in national security affairs.[29] The increased availability of scientific expertise in the departments and agencies was explicitly cited by President Nixon as a factor in his decision to discontinue PSAC and remove the science advisory apparatus from the Executive Office.[30] In France, the growing role of the DGRST and the inclusion of scientists on its regular staff eroded the privileged position which CCRST had once enjoyed in preparing the policy options of the government. Meanwhile, the conception of RAWB originally favoured by at least some of its founding fathers — that is, as a means of gaining an overview and of proffering well-pondered advice to political authority — became somewhat obsolete with the creation of a science policy ministry having essentially the same role.

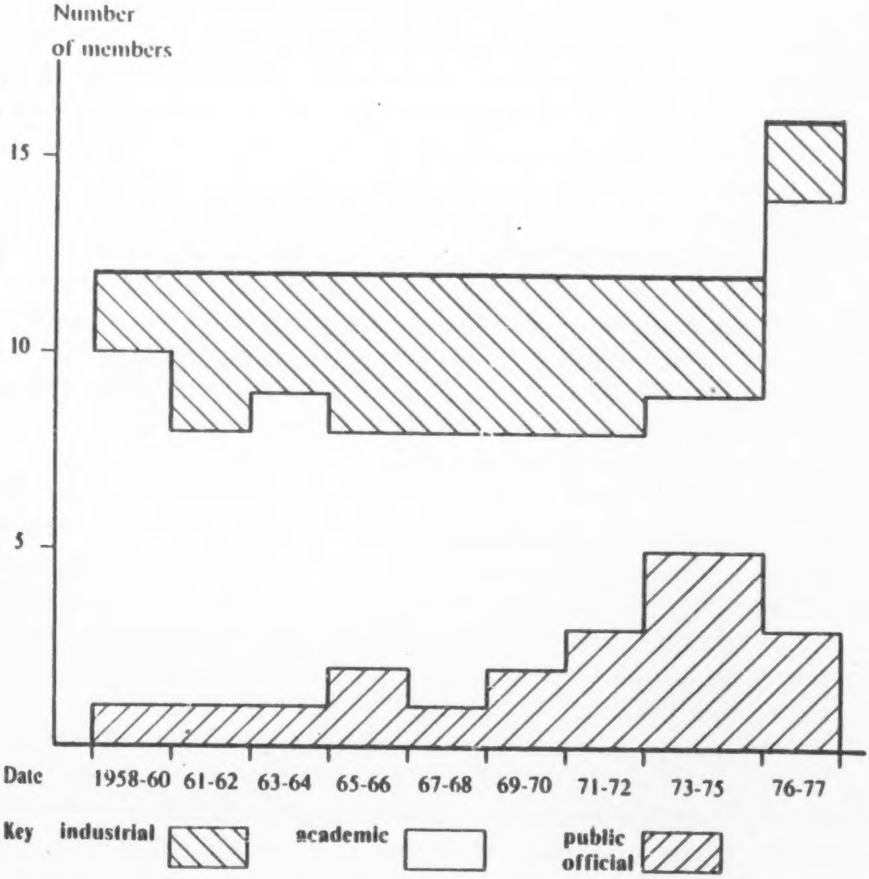
Ironically, all three councils were in part victims of their own success. Each had lobbied for a more coherent, reasoned and institutionalized approach of government in its involvement with science and technology. The achievements of their respective governments in this respect — abetted perhaps by the general law of bureaucratic expansion — eventually undermined the possibility that they might perform a unique and desired function for their political sponsors.

2) By 1970, the period of rapid growth in public financial support to R&D had clearly come to an end.[31] National R&D budgets failed to rise at the accustomed rate and in addition became increasingly devoted to meeting fixed costs and prior commitments. This reduced the margin of discretionary governmental action for which outside advice was usually sought. The allocation of resources tended to take on the form of a zero-sum power game among agencies and their clientele groups whose inflated interests were threatened with stagnation or contraction. In this climate, politics, interdepartmental and otherwise, came to the fore. The advisory councils, however, were ill-equipped to enter the fray. RAWB became more an observer than a participant, while CCRST became more of a sounding board for institutional interests (compare Figure 2, showing the affiliations of CCRST members). This trend

had less effect on PSAC, which from the beginning was not so much involved in budgetary overview.

FIGURE 2

Professional Affiliation of CCRST Members, 1957-77



Sources: See Table 1

3) At about the same time, governments came to realize that blanket support of R&D produces insufficient pay-offs, and they became more and more goal-oriented in their approach to science policy. The trend was clear in the United States with the launching of President Johnson's Great Society programmes and with President Nixon's emphasis on applying science and technology to national needs. The passing from the Fifth to the Sixth Plan (1971-76) in France marked a further shift of governmental attention from the development of a national R&D capability in itself to the uses of science and technology in furthering specific economic and social goals.[32] In both countries, and in the Netherlands, the turn of the decade marked a period of reconsideration in science policy; this led to more modest, explicit assessments of the purposes which public R&D programmes should serve.

This reorientation in aims contributed to a shift towards a less centralized, more sectoral perspective. High-level committees were too embracing in their purview and either too limited in expertise or too heterogeneous in their membership to render the kind of advice needed by the government. 'Science for policy' advice became more important, but such advice was given — or at any rate followed — only within the well-defined frameworks of departmental objectives. This new emphasis was less congenial to those councils (RAWB and the CCRST) which had focussed on broad policy-for-science issues. PSAC, which had concentrated on 'science for policy' issues from the beginning and which was the only council to use ad hoc specialized panels composed partly of non-member consultants, was better equipped to adjust to the times. But eventually, its reliance on panels of outside specialists, in which the representation of titular PSAC members progressively declined, raised questions about the necessity of the umbrella committee.[33]

4) The combination of scientific and political judgment that was required of the councils had encouraged each government to draw a large part of the membership from among a relatively small group of 'scientists-policy-makers' — that is, scientists who have an affinity for policy affairs.[34] In attempting to reconcile the scientific and the political, however, each council adopted a distinctive strategy, and each strategy had its inherent pitfalls. In the United States, PSAC members tended to introduce subjective criteria in their advice — for example, in their collective pronouncements on arms control measures and in the importance which they attached to such future problems as the quality of the environment.[35] This

eventually led them into situations where their advice failed to win sympathy from political authority or bureaucratic interests. When the committee shifted its attention away from weapons systems to less technical issues, moreover, its appreciations were not always seen as qualitatively superior to those of others.

In RAWB, from the beginning the scientific-political dichotomy was partly bridged both by the inclusion of scholars in the social sciences and the humanities (disciplines where value considerations play a more explicit role) and by the council's terms of reference, which required RAWB to take the 'national interest' into account. In undertaking its task, RAWB endeavoured to fuse the scientific and the political by taking a synthetic approach to policy-making. This approach was not unique to the council, however, and in the atmosphere of systems planning and policy analysis prevailing at the end of the 1960s, other agencies and advisory councils were set up for the same purpose, thus preempting a possible role of the RAWB.[36]

In France, the CCRST increasingly avoided political questions and prepared its opinions within guidelines drawn up by political authorities. In later years, its function was reduced to recommending marginal adjustments in the research budget and to discussing R&D management issues.

5) The shift in the late 1960s in public attitudes towards science and scientists indirectly contributed to the decline of the councils. A series of highly visible conflicts and controversies where scientists were ranged on opposing sides — Vietnam, the environment, nuclear energy, abortion — subverted the notion of scientists as sources of value-free advice.[37] The unrest in the universities in the same period further lowered the prestige of academic science in the public eye. In addition, a more critical appraisal of science and technology made tenuous the traditional belief that science and technology were automatically forces of progress.

These trends, symptomatic of a broader cultural movement in the three countries towards 'disestablishing the establishment', perhaps affected the public stature of the councils more than their everyday work. In France and the Netherlands, the sensitive issues on which scientists might fall into opposing political camps were avoided unless their treatment was explicitly requested. In all three countries, moreover, a convergence of opinion was made more likely by a certain homogeneity in the outlook and values of each council's membership.[38] Finally, members placed a high value on reaching a consensus view in their reports and on presenting an image of unity to the outside.[39] But given the less exalted public conception of science and the growing uncertainty surrounding the value of science as an input to decision-making, policy-makers could more easily disregard the advice of the councils and even dissolve them without provoking vigorous public outcry.

Common Factors in their Partial Revival

Around 1975 and 1976, there were signs that the tide of governmental support of R&D was again on the rise. Also, the public, policy-makers and scientists themselves may have become more accustomed to scientists proffering political judgments, and to disputes between experts. These developments may have contributed to the revival of the councils.[40]

In addition, and paradoxically, some of the factors which led to the decline of the councils were subsequently factors in their reinvigoration. In particular, the growth in the science policy bureaucracy reached such proportions that political authorities felt the need for an independent source of advice. As a result, the new role of the councils has been fashioned as a counterweight to the regular bureaucracy. In France, the Minister of Industry and Research stressed in 1975 that henceforth the CCRST would be 'his' group of advisors and requested reports on issues other than those brought up and structured by the DGRST. The recent reports of the RAWB have been increasingly devoted to comments on R&D programme proposals and on the ambitious plans of science policy reorganization proposed by the science policy ministry. In the United States, the 'counterweight' role of PSAC was recognized from the start (for President Eisenhower, the Committee was seen as one of the few official groups not acting in its own interest);[41] in 1975 and 1976, this justification for a scientific presence in the White House acquired new prominence.[42]

The sectoralization of governmental R&D also increased the need for some perspective which would transcend the parochial interests of the different agencies. Much of the work of the revived councils (including the provisional councils set up by President Ford) has thus been devoted to background analysis of general science policy issues and trends, including the health of university research, industrial innovation, and international scientific relations.

The cultural trend towards 'disestablishment' appears to have stimulated changes in the councils as well. In line with the general movement towards participation and democratization in social institutions in all three countries, discussions underlined the desirability of expanding the base of science policy advice beyond a small group of prestigious individuals having ties primarily with the dominant scientific and technological institutions of the country. Thus, the 1976 Act of Congress provided for a new council with a balanced composition drawn from the scientific and engineering fields, from academia, industry and government, and representing business, labour, consumer and public interest points of view.[43] The reform of the CCRST brought in women for the first time, and additional representatives from the provinces, from the social sciences and the humanities, from varying ideological and partisan perspectives and from delegates of the scientific community elected to national review boards. In the Netherlands, the impetus toward greater participation and democratization has so far had little direct impact on the science advisory council, other than bringing about wider informal consultations in the preparation of reports. Yet, in recent years, democratization has become a key issue in Dutch science policy. The reorganizations initiated by the Minister of Science Policy in 1974 provided for sectoral advisory and planning councils composed not only of scientists and government officials but also users and representatives of interested social groups. The future task of the RAWB is to provide opinions on the reports and plans of these sectoral councils.[44]

The Sources of Cross National Variation

On the whole, the advisory councils have had remarkably comparable experiences. In many respects, common trends in science policy and in general public attitudes toward science offer a reasonably good explanation of this similarity.

At several points, however, there are important differences. For example, PSAC emphasized 'science for policy' issues, while such an emphasis was less important to the RAWB and almost unknown to the CCRST (Figure 1). The CCRST was almost exclusively involved, particularly in the later years, with interagency coordination and central overview, while neither PSAC nor RAWB made much headway in this direction. The absorption of PSAC, especially in the beginning, with military and international affairs, contrasts with the virtual absence of such concerns from the RAWB and CCRST agendas. One notices as well the extensive support and attention which PSAC (and the White House science apparatus in general) enjoyed from the scientific-technological community; in contrast, neither the CCRST nor RAWB had much visibility. Finally, there are some surprising differences in the composition of the councils throughout their history: the RAWB showed a continuing balance of disciplinary and institutional affiliations, while in both PSAC and CCRST, there was an early predominance of academic natural scientists (particularly physicists) which later gave way to a more heterogeneous composition, including more industrialists in the United States and more officials of public research agencies in France (Table 1, Figure 2).

Such contrasts derive in many cases from underlying differences in national culture and political tradition. Ultimately, much cross-national variation may be traced to such historically rooted and dynamic processes as the nature and conditions of industrialization, evolution of social stratification, or even differences in 'national character'. Here, we limit our observations to three factors which appear most directly relevant to the councils' role and development: a) differing governmental views of science policy; b) differing roles of the scientist in public affairs; and c) differences in the organization of scientific and technological interests in the country and their incorporation in political decision making.

Governmental Views of Science Policy

Each advisory council has been affected by the way its government has conceived and organized its science policy. Conceptions of science policy vary according to the degree of coherence and coordination sought in governmental R&D.[46] Three conceptions can be distinguished. In 'atomistic coordination', there is no over-arching set of standards against which the appropriateness of a single R&D action or proposal can be assessed. Instead, programmes are launched and policies are developed and modified pell-mell, with coordination at best an incidental by-product of marginal readjustments among the separate institutional actors. In 'low coordination', coherence is sought in terms of managerial, 'performance' criteria of efficiency and rationalization (for example, the avoidance of duplication). In 'high coordination', science policymakers attempt to incorporate the diverse activities of separate governmental actors within an overall policy framework. Here, the appropriateness of a proposal can be established with reference to the goals and means indicated by the general policy.[47]

In 1958, the French government embarked upon the 'high road' of science policy coordination. Its objective was to bring coherence into disparate agency programmes and to ensure a balanced growth and use of resources. For this purpose, both the DGRST and CCRST were designed to stand apart from the agencies and to reconceptualize the frame of reference of R&D decision-making. The new organs availed themselves of the time-tested mechanism of the budget and the emerging overview device of the French state, the five-year plan. The destiny of the CCRST was thus tied to the agency which had the staff resources to delve into the intricacies of both French administration and French science. As the DGRST gained control over the administrative reins of coordination, the CCRST became more and more its captive.

In the United States, initiatives in science policy have remained at the agency level, with only the President (or Congress) in a position to block an agency proposal or, on occasion, make policy which cuts across agency lines. The only niche in the executive power structure for a scientific advisory council of general scope was under the wing of the President; thus its fortunes varied according to the attention it received from the President.[48] Its mode of operation was heavily influenced by the style of presidential decision-making — that is, it assessed the merits of R&D proposals and technologically relevant issues on an item-by-item basis as they rose for Presidential attention. Also, it was not excluded a priori from any area of policy within the executive branch, and its 'counterweight' role was prominent from the outset. As science and technology rarely emerged as an item of policy decision other than upon departmental initiative, the overview function of PSAC was relegated to secondary importance.

The history of science policy in the Netherlands has been characterized by the 'atomistic' mode of coordination and by a general reluctance to develop an integrated framework of decision-making. The creation of RAWB was manifestly designed to give signs of following the international trend toward a 'higher' level of coordination without changing the way decisions in science and technology were made. Since Dutch policy ultimately rests on the collective agreement of the cabinet ministers, RAWB did not enjoy, as PSAC did, access to a top political authority; nor did it have, as the CCRST did, an administrative staff to implement its opinions. The creation of a Ministry for Science Policy in 1973 did not fill this void, as the ministry itself has had very little executive authority. Throughout much of its history, RAWB aspired to a higher form of coordination than political realities permitted.

The Role of the Scientist in Public Affairs

A second factor which has given each council a distinctive character is the prevailing view of the scientist's role in public affairs. In the United States, the approach of public authorities to science and technology has always placed emphasis on the utility of R&D in meeting the practical needs of government, society and the economy.[49] The role of the scientist is in turn conceived as one of an expert who can be summoned to give advice on practical questions. This emphasis on expertise allowed PSAC to treat a wide range of 'science for policy' issues. Indeed, the idea that eminent scientists can place their superior technical competence in the service of political judgment gave PSAC its raison d'être and justified its separation from the regular civil service. The predominantly 'science for policy' orientation which fell naturally to PSAC led to some of its peculiarities vis-à-vis its Dutch and French counterparts — namely, its use of specialized panels, its greater vulnerability to 'counter-experts' within or outside government, the increasing heterogeneity of its membership, and its increasingly tenuous position as the scientific and technological component of issues decreased when social problems took precedence over weapons systems.

In both France and the Netherlands, the conception of the scientist-as-expert who is competent in a wide range of policy issues has not taken hold to the same extent. In France, expertise has always been assumed to lie within the civil service, particularly within the higher echelons issuing from the élite training schools. As technical expertise and rationalistic methods became more important in making policy, the response was not to seek outside advice but to alter the training of élite administrators.[50] The functions of 'policy for science' and 'science for policy' have been kept separate both at the highest level of decision-making and within the ministries. The role of the CCRST was thus confined to giving advice on the development of French science, usually as an end in itself but also as a means to other goals which the CCRST was not in a position to determine. Since the CCRST had little voice over policy decisions serving other ends, the committee (and often the DGRST as well) found itself excluded from many of the decisions which came to have a major impact, including, as nearly as can be determined, the Concorde project, the fast breeder nuclear reactor, and the government's policy in computer hardware.

The Dutch case is in contrast with both the American and the French. The prevailing view of the scientist in the Netherlands is one of the professional who in return for society's tacit but material support performs his services in the setting of his choice: industry, university or public research institute. It is the professional who has traditionally been invited into the inner councils of government; in devising the RAWB, the 'scientist' was on an equal footing with other professionals in the medical, legal, economic and engineering fields (Table 1). In keeping with this tradition, RAWB had to earn its influence on the strength of its advice, and a firm institutional base for the council was deemed unnecessary.

The Organization and Political Incorporation of Scientific Interests

A final source of difference among the councils lies in the organization of scientific and technological interests and the modes of their incorporation in the political system of each country. The American case, for example, is characterized by high cohesiveness and mobility within the scientific community, particularly within that group which circulates in policy-making circles.[51] It is a system where governmental funding for research is not assured by an enduring, blanket political commitment but is instead highly susceptible to volatile and short-term considerations; where universities are not direct appendages of the State apparatus and many technology-intensive and consulting firms are privately owned but highly dependent on government contracts; and where an underlying, anti-intellectual strain in American politics may surface at any moment. In this context, the industrial, university and private consulting branches of the community have made common cause to assure for themselves a place in the political sun. Whatever their actual impact on decisions, the PSAC, as well as the office of science advisor, have been elevated to symbolic importance as an indicator of prevailing political winds in the executive branch for and against science and technology, much in the same way as has White House representation of other interests, such as the environment and communications.[52] PSAC's role as a political bellwether helped give the council a higher visibility to the public and to the scientific community than either of its European counterparts.

In France, the scientific community is much more fragmented; universities, industry and government laboratories maintain little communication among each other and articulate their demands on the political system through separate channels. A further source of fragmentation is the active presence of scientists' and university faculty unions which reflect the divisions of the scientific community along essentially ideological and partisan lines.[53] The efforts of the French government to formulate a coherent, integrated science policy have been motivated in large measure by the desire to reduce inter-institutional barriers and have been severely constrained by the failure to do so.[54] The history of the CCRST can be better understood in the same light: by incorporating the diverse institutional interests of French science, the committee, instead of overcoming diversity, has in part succumbed to it. Particularly in recent years, these diverse groups have used the committee to see that their parochial interests are protected.[55]

In another respect, the activities of the CCRST have been influenced by the special character of the French political system. One of the outgrowths of France's highly centralized administration has been a proliferation of consultative procedures and institutions by which the public's demands and complaints are made known to an otherwise uninformed bureaucracy.[56] The CCRST fulfilled a comparable role: by its awareness of the problems and needs of the scientific constituency it suggested ideas for policy and informed administrators of the possible effects of its proposed measures. As already noted however, this function — much less prominent in the work of the other two councils — became less relevant as the DGRST built up its own network of scientific contacts.[57]

The Dutch scientific community is relatively homogeneous, with extensive contact among industry, government and the universities.[58] Yet, the demands scientists make upon the government tend to reflect the interests of their respective institutional affiliations. The distinct socio-cultural cleavages ('pillars') which extend to the far reaches of social and political behaviour in the Netherlands are also present, if somewhat veiled, in the politics of Dutch science.[59] In keeping with the national tendency to incorporate all pertinent interests in deliberative and advisory bodies,[60] the membership of RAWB has been artfully fashioned from among these scientific, institutional and socio-cultural groups. But RAWB did not become a mere forum for such interests, as was the tendency with the CCRST. This is primarily due to the paramount importance of the 'pillars' as channels of interest group activity in all sectors of Dutch politics; the existence of alternative points of entry meant that RAWB could reflect those pervasive networks without becoming a link in the chain of demands directed to the government.

Some Concluding Remarks

Our purpose has been to identify the common trends which have affected three science policy advisory councils over the period from 1957 to 1977, and to isolate those features of each national setting which have given each council unique qualities in role, structure, and impact. There are, of course, other factors which may have directly or indirectly influenced their development. The obvious discrepancy in the size of the three countries, differences in their productive bases and the historical relationship between the public and private spheres may have conditioned their experiences in various complex ways. At an entirely different level, the councils were not immune to the clashes of interests, to the play of personal and partisan ideologies, and to fortuitous events which often determine political decisions. It is likely that a more detailed delineation of our 'dependent variable', particularly in terms of the content of the advice given by the three councils, and their actual impact on decision-making in specific cases, would have given additional weight to socio-economic, structural and general political considerations.

The three case histories do, however, demonstrate the variability and adaptability of the councils' roles within a basically common structural framework. The general rubric 'science policy advisory councils' incorporates in practice a variety of roles and functions. One can identify at least six distinct tasks for such bodies: the promotion of science and technology as areas of governmental support; the organization of scientific and science-policy institutions; the evaluation of proposals for scientific and technological projects; the monitoring of the scientific and technological input into general policy deliberations; forecasting and early warning of impending problems; and the provision of a broad overview of departmental R&D to arbitrate between interdepartmental conflicts, to set general priorities and to identify lacunae.[61] The specific tasks and approaches adopted by one council are not necessarily common to all such councils; PSAC's focus on weapons systems and on 'science for policy' issues in general, for example, was the exception rather than the rule.

Our analysis suggests that the ability of a council to shift emphasis from one task to another according to changing perceptions of governmental needs is a key factor in its survival. Among the three cases examined, the RAWB provides the best illustration of this adaptability: having no obvious outlet either for its 'policy for science' advice (like the DGRST) or for its 'science for policy' advice (like the US Presidency), RAWB has nevertheless managed in the end to carve out a useful role for itself.[62] As for the future, the least that can be said is that there will be more modest expectations for science policy advisory councils. After the experience of the past twenty years, there can be no return to the heroic past.

It remains an open question whether the factors influencing the role and evolution of the Dutch, French and American councils have had similar effects on high-level science policy advisory bodies elsewhere. Comparable institutions were created in other countries (the Science Council in Canada, the *Wissenschaftsrat* in West Germany and the *Counseil National de la Politique Scientifique* in Belgium, for example) and have seemed, according to available accounts,[63] to follow a similar pattern of evolution. It is also possible that any differences in their composition, functions and impact can be profitably analyzed with the help of the factors of cross-national variation put forward here.

Our three examples suggest further that these nation-specific factors have become increasingly important in understanding the science-government relationship. Since the 1960s, science policy has lost much of its distinctiveness, and its processes have come to resemble those of policy-making in general. The common trends which have affected the councils throughout their history can be summarily taken as illustrations of the ascendancy of the political over the scientific. The factors which have distinctively coloured the history of each council become more important as government's approach to science and technology increasingly takes on the cast of 'politics-as-usual'. Since these factors are linked with broad differences among political and cultural systems, science in government may be less a factor of convergence than a key indicator of the underlying and enduring sources of divergence among political systems.

• *NOTES*

Research for this article was supported by Grant No. SOC 75-02414 of the National Science Foundation and by a travel grant from the Netherlands Organization for the Advancement of Pure Research (ZWO).

1. Philip J. Gummett and Geoffrey L. Price, 'An Approach to the Central Planning of British Science: The Formation of the Advisory Council on Scientific Policy', *Minerva*, Vol. 15 (Summer 1977), 119-43.

2. Don K. Price, *The Scientific Estate* (Cambridge, Mass: Belknap Press of Harvard University Press, 1965); Robert Gilpin and Christopher Wright (eds), *Scientists and National Policy-Making* (New York: Columbia University Press, 1964), especially the articles by Price, Wood, Sayre and Brooks; Daniel Bell, *The Coming of Post-Industrial Society* (New York: Basic Books, 1973); Robert E. Lane, 'The Decline of Politics and Ideology in a Knowledgeable Society', *American Sociological Review*, Vol. 31 (1966), 649-62; Alain Touraine, *The Post-Industrial Society* (New York: Random House, 1971).

3. Recent discussions on the role of the scientific advisor in government include Sanford A. Lakoff, 'Scientists, Technologists and Political Power', in I. Spiegel-Rösing and D. de Solla Price (eds), *Science, Technology and Society: A Cross-Disciplinary Perspective* (Beverly Hills, Calif.: Sage, 1976), 355-91; Ian D. Clark, 'Expert Advice in the Controversy about Supersonic Transport in the United States', *Minerva*, Vol. 12 (October 1974), 416-38; Duncan MacRae, Jr, 'Technical Communities and Political Choice,' *Minerva*, Vol. 14 (Summer 1976), 169-90; Don K. Price, 'Money and Influence: The Links of Science to Public Policy', *Daedalus*, Vol. 103, No. 3 (Summer 1974), 97-114.

4. Recent contributions to this literature include Organization for Economic Cooperation and Development, *The Research System*, Vols. I, II and III (Paris: OECD, 1972, 1973, and 1974); OECD, *Changing Priorities for Government R&D* (Paris: OECD, 1975); Bruce L. Smith, 'A New Science Policy in the United States', *Minerva*, Vol. 11 (Summer 1973), 162-74; W. Henry Lambright, *Governing Science and Technology* (New York: Oxford University Press, 1976).

5. Two recent appeals for comparative work in science policy analysis are to be found in Spiegel-Rösing and Price, op. cit. note 3, 29; and Joseph Haberer, 'Introduction by the Symposium Editor', Science and Technology Policy, *Policy Studies*, Vol. 5 (Winter 1976), 157.

6. National case studies include Daniel Greenberg, *The Politics of Pure Science* (New York: New American Library, 1967); Norman J. Vig, *Science and Technology in British Politics* (Oxford: Pergamon, 1968); and the series in T. Dixon Long and Christopher Wright (eds), *Science Policies of Industrial Nations* (New York: Praeger, 1975). Robert Gilpin, in *France in the Age of the Scientific State* (Princeton, NJ: Princeton University Press, 1968), compares French and American science policy but gives little attention to the policy role of the scientist.

7. General discussions of the relationship between science and government include J.-J. Salomon, *Science and Politics* (Cambridge, Mass.: MIT Press, 1973); Harvey Brooks, *The Government of Science* (Cambridge, Mass.: MIT Press, 1968); and M. Blisset, *Politics in Science* (Boston, Mass.: Little, Brown and Co., 1972). See also the references cited in note 2.

8. Bell, op. cit. note 2; Zbigniew Brzezinski and Samuel P. Huntington, *Political Power: USA/USSR* (New York: Viking Press, 1965).

9. Our analysis of the President's Science Advisory Committee in the United States has benefited from a rich literature of primary sources, Congressional hearings, testimonials and diaries, and secondary analyses (see note 10 below for principal references). Published material on the Dutch and French cases is much scarcer. The problem of collecting research materials on the French council is aggravated by the confidentiality of nearly all of its reports and opinions. Its 1968 report, 'Prospective de la Recherche Scientifique en France', has been published in *Le Progrès Scientifique*, Nos. 126, 127, 128, 129 (January, February, March, April 1969), and a few reports were privately made available to the authors. But the fact remains that the analysis of the French case relies more heavily on direct interviews of council members and civil servants than is the case for either the Dutch or, particularly, the American council.

10. Principal sources for the history of PSAC have been George B. Kistiakowsky (with an introduction by Charles S. Maier), *A Scientist at the White House* (Cambridge, Mass.: Harvard University Press, 1976); James R. Killian, Jr, *Sputnik, Scientists, and Eisenhower* (Cambridge, Mass.: MIT Press, 1977); David Z. Beckler, 'The Precarious Life of Science in the White House', *Daedalus*, Vol. 103, No. 3 (Summer 1974), 115-34; Robert N. Kreidler, 'The President's Science Advisers and National Science Policy', in Gilpin and Wright, op. cit. note 2, 113-43; Carl William Fischer, 'Scientists and Statesmen: A Profile of the President's Science Advisory Committee', in Sanford A. Lakoff (ed.), *Knowledge and Power* (New York: The Free Press, 1966), 315-58; US Congress, House of Representatives, Committee on Government Operations, Military Operations Subcommittee, *The Office of Science and Technology*, Report of the Science Policy Research Division, Library of Congress, 90th Congress, 1st Session (Washington, DC: US Government Printing Office, 1967). For an historical analysis of the relationship of the presidency and science and technology policy, see James Everett Katz, *Presidential Politics and Science Policy* (New York: Praeger, 1978).

11. For these institutional innovations see Gilpin, op. cit. note 6, 188-217; and G. Druesne, 'L'Organisation administrative de la recherche scientifique en France',

Notes et Etudes Documentaires, No. 3863 (Paris: La Documentation Française, 1972). For a summary of the early years of the CCRST, see Pierre Lelong, 'Le rôle du Comité Consultatif de la recherche scientifique et technique auprès du Gouvernement', *Le Progrès Scientifique*, No. 41 (December 1962), 1-6.

12. The first authoritative mention of science policy came in August 1963 when the new cabinet, in its inaugural policy statement, announced its intention to work actively toward a national science policy. This declaration of intent met with wide approval in Parliament, and soon afterwards a panel of eminent scientists and scholars, assisted by the director-general for science and humanities (Ministry of Education, Arts and Sciences) was formed to prepare an advisory report (of 17 September 1964, published as an appendix to the Bill to establish a Science Policy Advisory Council, 3 November 1965; 2nd Chamber, 1965-66, *8355*). The report proposed coordination of science policy by the Minister for Education, Arts and Sciences, a Science Policy Advisory Council with a secretariat from that Ministry, and an annual science budget. The bill was delayed because of a change of Cabinet and departmental resistance to the proposed increased role of the Ministry for Education, Arts and Sciences. The bill, finally enacted on 20 May 1966, followed the report closely on the establishment of the Science Policy Advisory Council but required that its secretariat be independent. The Council became active from 1 September 1966; three of the nine members had also been on the panel (and a fourth panel member joined after a year as a replacement).

13. The original terms of reference of the Special Assistant and PSAC are given in Killian, op. cit. note 10, 275-76; the text of the White House statement instituting PSAC is found in US Congress, House, *The Office of Science and Technology*, op. cit. note 10, 262-63.

14. The Act to establish a Science Policy Advisory Council (20 May 1966, Staatsblad 227) defined science policy to include national as well as international aspects and pure and applied research as well as development. The RAWB advises when asked or on its own initiative, and in its advice it is to consider science (including the humanities) as a whole. The Council includes in its considerations opinions put forward by existing bodies with a statutory right to advise the government. Members as well as chairmen and vice-chairmen are appointed by the Minister for Education and Science, barring opposition in the Council of Ministers, for a term of five years (once renewable). Only the chairman and vice-chairman are salaried.

15. For an account of activities of CCRST in the 1960s, see 'Regard sur les travaux du Comité Consultatif de la Recherche Scientifique et Technique (1966-1967)', *Le Progrès Scientifique*, No. 112 (October 1967), 3-16, and ibid, No. 113 (November 1967), 3-8.

16. Interim report (March 1968), published in March 1969 (2nd Chamber 1968-69, *9800*, No. 34, 40-51) and sequel to the interim report (24 April 1969), published 11 July 1969 (2nd Chamber 1968-69; *9800*, No. 50). See also the General Considerations at the end of the first term (16 September 1971), published 7 December 1971 (2nd Chamber, 1971-72, *11639*. Nos. 1, 2).

17. Beckler, op. cit. note 10, 118-27.

18. Gilpin, op. cit. note 6, 196.

19. 'Advice on the Organization of Science Policy Administration' (8 October 1970), 2nd Chamber 1970-71, *10957*, No. 2; A. J. Pickaar, *Universiteit en Hogeschooe*, Vol. 23 (February 1977), 196-216.

20. *Science*, Vol. 179 (12 January 1973), 160, and ibid. (2 February 1973), 455-59; *New Scientist* (18 January 1973), 143, and ibid. (1 February 1973), 257.

21. Our evidence comes from interviews with former CCRST members and the secretary of the committee during part of this period. For a polemical critique of the CCRST, see Roger Godement, 'Les sages, le secret et la politique scientifique française', *La Recherche*, No. 59 (September 1975), 713-18.

22. Ministère de l'Industrie et de la Recherche, Decree of 24 November 1975, *Journal Officiel* (28 November 1975); and interview data.

23. US Congress, Senate, Committee on Commerce, Science and Transportation and Committee on Human Resources, *A Legislative History of the National Science and Technology Policy, Organization and Priorities Act of 1976* (Washington, DC: US GPO, 1977).

24. *Science*, Vol. 197 (29 July 1977), 442-44, and ibid. (5 August 1977), 538; and interview notes with staff members of the Office of Science and Technology Policy, June 1977.

25. See Charles S. Maier in Kistiakowsky, op. cit. note 10, xxxvii-liii; and the testimony of Kistiakowsky before a Congressional committee in July 1974; US Congress, House, Committee on Science and Astronautics, *Federal Policy, Plans and Organization for Science and Technology*, Part II, Hearings, 93rd Congress, 2nd Session (Washington, DC, US GPO, 1974), 178-82 (also published in *Science*, Vol. 124 [5 August 1974], 38-42); see also Killian, op. cit. note 10.

26. See Gilpin, op. cit. note 6, 195 and 3-16.

27. Alexander King, *Science and Policy, the International Stimulus* (London: Oxford University Press, 1974); Piekaar, op. cit. note 19, 196-97.

28. These interpretations were prominent in the interviews conducted with council members and other informed observers in the three countries. See also Beckler, op. cit. note 10, 122-25; and 'Science Advice for the White House' (transcript of a debate among former White House science advisors), *Technology Review*, Vol. 76, No. 3 (January 1974), 9-19.

29. See Kistiakowsky in US Congress, House, *Federal Plans . . .*, op. cit. note 25; and 'Science Advice for the White House', op. cit. note 28.

30. The White House, Reorganization Plan No. 1, Letter to the Congress of the United States, reprinted in US Congress, House, Committee on Science and Astronautics, *Federal Policy, Plans and Organization for Science and Technology*, Interim Staff Report, 93rd Congress, 2nd Session (Washington, DC: US GPO, 1974), 193-94.

31. Organization for Economic Cooperation and Development, *Paterns of Resources Devoted to Research and Experimental Development in the OECD Area, 1963-71* (Paris: OECD, 1975).

32. Gilpin, op. cit. note 6, 235-44; Délégation Générale à la Recherche Scientifique et Technique, Commissariat Général au Plan, *Rapport de la Commission Recherche*, Vol. I (Paris: La Documentation Française, 1971).

33. A staff member of the Office of Science and Technology Policy cited this as one reason why the re-establishment of a permanent, high-level committee of outside scientists was not envisaged (interview data).

34. Harvey Brooks, 'The Scientific Adviser', in his *The Government of Science*, op. cit. note 7, 81-109.

35. In one panel report, the necessity to include social and political judgements in giving advice is explicitly recognised. See Space Science and Technology Panel,

PSAC, *The Next Decade in Space* (Washington, DC: Executive Office of the President, March 1970), 3.

36. There appeared, for instance, a Departmental Committee for Development of a Policy Analysis, and the WWR (Scientific Council for Government Policy); indicative of the atmosphere is the report of a commission on research into the future structure of society (Commission De Wolff), 2nd Chamber, 1970-71, *10914*, No. 2.

37. Todd LaPorte and Daniel Metlay argue that controversies in Congress and among scientific élites have considerable impact on public attitudes toward science and technology: see their 'Public Attitudes Toward Present and Future Technologies: Satisfactions and Apprehensions', *Social Studies of Science*, Vol. 5 (1975), 373-98.

38. The processes of appointment of council members in the three countries no doubt contributed to a degree of homogeneity along these lines.

39. In the RAWB, for instance, reports were delayed when opinions diverged and compromise versions were published only when external pressure became high. Minority reports in the work of the councils are almost unknown. One example is a differing opinion by Wigner in the second Piore panel on high-energy physics (1960); see Greenberg, op. cit. note 6, 234-35. It would appear that scientists endeavour to transfer the norms of rational consensus, characteristic of the scientific ethos (at least as presented to the exterior) to their activities as governmental advisors.

40. Discussions about revising PSAC or a comparable science advisory apparatus were not simply the result of President Nixon's replacement by a President ostensibly more hospitable to scientists. This is clear from the independent efforts underway in the Congress to revive the science advisory institutions. See US Congress, op. cit. note 23.

41. Maier, op. cit. note 10, liii.

42. US Congress, Senate, Special Subcommittee on the National Science Foundation, Committee on Labor and Public Welfare, and the Subcommittee on Science, Technology, and Commerce, Committee on Commerce and the Committee on Aeronautical and Space Sciences, *National Policy and Priorities for Science and Technology Act, 1975*, Joint Hearings, 94th Congress, 1st Session (Washington, DC: US GPO, 1975); US Congress, Senate, *A Legislative History* . . ., op. cit. note 23; Eugene B. Skolnikoff and Harvey Brooks, 'Science Advice in the White House? Continuation of a Debate', *Science*, Vol. 187 (10 January 1975), 35-41.

43. US Congress, Senate, *A Legislative History* . . ., op. cit. note 23, 11.

44. For a discussion of the proposals for the reform of Dutch science policy institutions, see *Planning and Development in the Netherlands*, Vol. 8, No. 2 (1976) (Whole Issue). In 1969, the RAWB was asked to consider possibilities for its own democratization. The Council advised against it, noting that the law would have to be changed. No further action was taken, although criticisms of élitism and proposals for reform were still expressed occasionally. The reorganization proposed by the science policy minister, while not affecting RAWB itself, seems to have quietened these voices.

45. Including an ad hoc Committee on Science and Technology of the National Academy of Sciences. Its report is reprinted in US Congress, House, *Federal Policy, Plans* . . ., Part II, op. cit. note 25, 441-502.

46. For a fuller treatment of these distinctions, see Ronald Brickman, 'Interagency Coordination in Science Policy: A Comparative Study of Theory and

Practice', paper presented at the Annual Meeting of the American Political Science Association, Washington, DC, 1-4 September 1977.

47. Note that this categorization of approaches to science policy differs from the usual one (such as that found in the Lamontagne Report), based on the distribution of power among science policy actors. Canadian Senate Special Committee on Science Policy, *A Science Policy for Canada*, Vol. 3, 'A Government Organization for the Seventies' (Ottawa: Information Canada, 1973).

48. An alternative institutional framework would be an interdepartmental scientific committee. The Federal Council for Science and Technology was created to perform essentially a coordination task among the agencies. It was composed of agency staff members, not outside consultants, however, and rarely took policy initiatives. See US Congress, House, Committee on Science and Technology, Subcommittee on Domestic and International Scientific Planning and Analysis, *Interagency Coordination of Federal Scientific Research and Development: The Federal Council for Science and Technology*, Report prepared by Science Policy Research Division, Library of Congress (Washington, DC: US GPO, July 1976).

49. Price traces these beliefs in part to the personal example and institutional reforms of Jefferson, who expressed faith in the value of science in developing American agrarian society; later, the pattern of grants-in-aid set by the land-grant college innovation of the 19th century became the basis of a 'new federalism' which allowed scientists to work out policy programmes largely unconstrained by party doctrine. See Don K. Price, *Government and Science* (New York: New York University Press, 1954), 1-31. See also George H. Daniels, *Science in American Society* (New York: Knopf, 1971), 288-315. A general historical treatment of the relationship of government and science in the United States is A. Hunter Dupree, *Science in the Federal Government* (Cambridge, Mass.: Belknap Press of Harvard University Press, 1957); for a more recent analysis of scientists in US policy-making, see Dean Schooler, Jr, *Science, Scientists and Public Policy* (New York: The Free Press, 1971).

50. F. F. Ridley, 'French Technocracy and Comparative Government', *Political Studies*, Vol. XIV (February 1966), 34-52; Ezra N. Suleiman, *Politics, Power and Bureaucracy in France: The Administrative Elite* (Princeton, NJ: Princeton University Press, 1974), 382.

51. Martin L. Perl, 'The Scientific Advisory System: Some Observations', *Science*, Vol. 173 (24 September 1971), 1211-15.

52. One can speculate that a contributing factor in the decision not to re-establish PSAC in recent years was the sharing of this symbolic function with the Science Advisor and his staff. The American scientific community through various spokesmen has pressed for *some* kind of White House scientific presence, but not necessarily a PSAC-like council of outside consultants. See references cited in note 42.

53. Some scientists' unions, such as the *Syndicat national des chercheurs scientifiques* (professional public researchers) and *Syndicat national de l'enseignement supérieur* (university teachers), are closely identified with the political Left; others such as the *Fédération des syndicats autonomes* are commonly associated with the political Right.

54. See Ronald Brickman, 'The Promotion of Mobility of Scientists: A Problem of French Science Policy,' *Minerva*, Vol. 15 (Spring 1977), 62-82, for a discussion of the efforts of the French science policy authorities to overcome inter-institutional barriers.

55. By avoiding the hard questions which would endanger one group in favour of another, the CCRST is often limited to simply asking the government for more for everyone, as is illustrated by one of its most dramatic moves. In 1975, it threatened to resign collectively, not because of disagreement over the government's decision to spend vast sums in yet another attempt to save the country's computer industry, but because of the government's attempt to finance the venture through the science budget without increasing overall expendable resources (*Le Monde*, 6 June 1975, and interview data).

56. Michael Crozier et al., *Où va l'administration francaise?* (Paris: Les èditions d'organisations, 1974); Jean-Claude Thoenig, 'La relation entre le centre et la périphérie en France,' *Bulletin de l'Institut international d'administration publique*, No. 36 (October-December 1975), 1037-83; Y. Weber, *L'Administration consultative* (Paris: Librairie Gènérale de Droit Juridique, 1968); G. Langrod, *La consultation dans l'administration contemporaine* (Paris: Cujas, 1972).

57. Pierre Vautier, 'La recherche dans le VIIe Plan,' *La Recherche*, No. 69 (July-August 1976), 684-86; and interview data.

58. OECD, *Reviews of National Science Policy, The Netherlands* (Paris: OECD, 1973), 251-52; OECD, *The Research System*, Vol. 2 (Paris: OECD, 1973), 144-45.

59. A. Lijphart, *The Politics of Accommodation, Pluralism and Democracy in the Netherlands* (Berkeley, Calif.: University of California Press, 1968); William Z. Shetter, *The Pillars of Society: Six Centuries of Civilization in the Netherlands* (The Hague: Martinus Nijoff, 1971).

60. Hans Daalder and Galen A. Irwin, 'Interests and Institutions in the Netherlands: An Assessment by the People and by Parliament', *The Annals of the American Academy*, Vol. 413 (May 1974), 58-71.

61. This list of roles or functions, to which synthetic planning, background analyses and information-gathering ('state of the art') can be added, is based in part on that provided by Brooks in Gilpin and Wright, op. cit. note 2, 74-77; and by Alan G. Mencher, *Lessons for American Policy Making from the British Labour Government's 1964-70 Experience in Applying Technology to Economic Objectives* (London: London Graduate School of Business Studies, and Washington, DC: National Science Foundation, mimeo, 1975), 132.

62. The same canot be said of the CCRST. By the Spring of 1977, the committee was still heavily dependent on the DGRST, whose directorate has not looked with favour upon the committee's efforts to provide independent analysis and recommendations to higher political authority. Only one report among those requested in 1975 had been completed by this time (on university research). The appointment in April 1977 of a State Secretary for Research under the authority of the Prime Minister has provided the CCRST with an opportunity to redefine its role on the French science policy scene.

63. OECD, *The Research System*, op. cit. note 4, Vol. 2, 36-37; interview data in the Federal Republic of Germany and Belgium. The Advisory Council on Scientific Policy in Britain, created in 1947 and disbanded in 1965, provides an interesting contrast, both in the early date of its establishment and in its mixed composition of departmental and outside scientists. For an account of the debate leading to its creation, see Gummett and Price, op. cit. note 1; for an assessment of the Council's work, see Vig, op. cit. note 6, 23-24 and 65-67.

Ronald Brickman is an Assistant Professor in the Program on Science, Technology and Society, Cornell University. His primary research interest is the comparison of science and technology policies of advanced industrialized countries. His published articles have appeared in *Minerva, Research Policy, International Organization,* and other journals..

Arie Rip directs the Chemistry and Society Programme of the University of Leiden. He is the author of an introductory book on the role of science in contemporary society (in Dutch), entitled *Science as a Human Endeavour.* His research interests are the social responsibility of the scientist, and the dynamics of scientific development and their interaction with science policy impulses. *Authors' addresses* (respectively): Ronald Brickman, Program on Science, Technology and Society, Cornell University, 632 Clark Hall, Ithaca, NY 14853, USA; Arie Rip, Chemistry and Society Programme, Gorlaeus laboratoria, Wassenaarseweg 76, Leiden, Netherlands.

Social Studies of Science, 6 (1976), 445-70

The Mediating Role of the Scientific Elite

Michael Mulkay

At its inception, the sociology of science was centrally concerned with relations between the scientific community and scientific knowledge on the one hand, and the wider society on the other hand. This was true, for instance of Merton's early work on the emergence of modern science[1] and on the norms governing scientific research.[2] It was also true of Barber's attempt to provide a first systematic overview of the field.[3] Recently, however, there has been a growing concentration upon the internal characteristics of the research community, and particularly on the academic research community, with the wider social setting taken as given and relatively unproblematic.[4] Those responsible for these more narrowly focused studies have not been unaware that the research community is subject to external influences. But they have assumed that, at least in some modern societies, it has sufficient autonomy to make a separate analysis of its internal processes acceptable as a first approximation.

This research strategy has proved to be fruitful. It has enabled researchers to isolate a series of manageable problems and has produced results which have significantly improved our understanding of scientific communication,[5] the reward system of science,[6] social control in science,[7] the development of research areas,[8] and so on. However, this approach has had the unfortunate consequence of drawing attention away from the possibility of extending the analysis of the research community in such a way that connections with the

---

Author's address: Department of Sociology, University of York, Heslington, York, YO1 5DD, UK.

wider society are brought within its scope. One the whole, therefore, sociologists have failed to investigate these connections in recent years, either theoretically or empirically.

I offer this paper as a small step toward redressing this inadequacy in the current approach to the sociology of science. More specifically, I shall attempt to formulate a preliminary account of some of the connections between the academic research community and the wider society, by exploring certain implications of the recent literature on the internal organization of academic science. My central concern will be with the ways in which external influences impinge on the development of science and scientific knowledge. I will show, first of all, that the academic scientific research community generates its own well-defined élite. Secondly, I will argue that much of the pressure exerted by governments, and perhaps by other lay groups, on the development of scientific knowledge is mediated through the élite. Thirdly, I will argue that this élite, having been created primarily by social processes occurring inside the research community, remains strongly committed to objectives regarded as central within that community. Finally, I will illustrate how, at least in Britain, this scientific élite operates as a 'buffer group', successfully resisting instrumental demands from outside and maintaining considerable freedom for members of the academic research community to pursue their own 'scientifically defined' interests. My main objective in the discussion below is, in a rather crude sense, theoretical; namely, to show that the sociological analysis of the internal and external relationships of science can be fruitfully combined by concentrating, in the first instance, upon the scientific élite. I shall try to reach this end, however, by means of a close examination of empirical evidence. In this discussion I shall use evidence only from America and Britain.

THE SCIENTIFIC ELITE

The assertion that a society or a community has an élite contains at least four separate elements.[9] In the first place, it means that the central rewards and facilities of the community are distributed among members in a markedly unequal fashion. Secondly, it is implied that the social ties among the few privileged members are stronger than their ties with other members. Investigators usually identify the members of a particular élite by these two criteria – that is, by their special control over scarce resources and by the network of social relations which link them together. Thirdly, the assertion normally implies that the ability to control or direct the activities of others is closely associated with membership of the élite; and, fourthly, that those who have already acquired privileged positions are able to exert considerable influence over further recruitment to the élite. Let me now summarize the evidence which shows clearly that academic science does have a well defined élite.[10]

The members of the scientific research community are centrally concerned with creating and certifying new knowledge about natural phenomena.[11] In pursuit of these objectives, participants engage in research and disseminate their findings. In many cases, these findings are treated initially by other researchers not as scientific knowledge but as claims to knowledge.[12] Exactly how these knowledge-claims are received by interested scientists varies with the level of scientific maturity of the research area in question; results usually being treated more sceptically in phases of exploration than when a cognitive framework has been generally agreed and when research findings have become relatively predictable.[13] Many knowledge-claims, perhaps most of them, appear to receive no active response from the research community;[14] whilst those to which there is a definite reaction are often either rejected or appreciably modified.[15] Nevertheless, researchers in such disciplines as physics, chemistry, astronomy and their sub-disciplines, do seem to be specially able, in comparison with those working in other fields of intellectual endeavour, to establish relatively well-defined albeit limited areas of intellectual consensus.[16] This consensus seems to consist primarily in agreement about that relatively small number of contributions which are treated as specially significant. In part, the recurrent creation of new realms of cognitive consensus in science is probably due to certain distinctive features of intellectual activity in these fields. For instance, experimental findings can often be replicated;[17] research techniques are frequently based on well-tried theoretical schemes, which make possible a fairly unambiguous interpretation of results; and findings can often be formulated with great precision in mathematical terms, so that inconsistent results can be clearly identified and investigated.[18] One other contributory reason for the attainment of consensus in science is that the system of rewards is directly linked to its creation and maintenance.[19]

The scientific research community rewards its members, in the first instance, with professional recognition of their intellectual achievement.[20] Recognition is given in return for results which are judged to have contributed to scientific knowledge and is bestowed in accordance with the perceived significance of the contribution.[21] In other words research scientists gain in professional repute to the extent to which they are seen to have contributed intellectually to areas of scientific consensus. Recognition will be forthcoming for a particular contribution only insofar as there is professional agreement about its significance. The ability of physical scientists to create areas of precise intellectual agreement and, subsequently, to assess relevant knowledge-claims unequivocally is probably responsible for the fact that the judgement of results and the award of recognition in science is relatively unaffected by certain kinds of 'particularistic' factors. Thus Gaston found that social class and secondary education were not related to receipt of recognition among British high energy physicists.[22] Similarly, the Coles show that there is no perceptible discrimination *within* the American research community against female or Jewish members.[23] Nevertheless, despite the fact that recognition is awarded to scientists independently of any variations in their social origins and in accordance with fairly clear intellectual criteria, its distribution throughout the research community is far from uniform. There are several reasons for this.

In the first place it is clear that a rather small number of scientists actually produce a large proportion of published results.[24] Price estimates that for every 100 scientists who produce one paper, there are only 25 who produce two papers, 11 who produce three, and so on.[25] To put this another way, about 50 per cent of all scientific papers are written by roughly 10 per cent of scientists. This would produce a markedly unequal distribution of professional recognition, even if each contribution (crudely measured above in terms of research papers) were judged to be of equal value or quality. In practice, of course, the worth attributed to particular research results or knowledge-claims varies enormously and, as several studies have shown, the recognition awarded to a specific paper or to the work of a particular scientist appears to depend to a considerable extent on the judged quality or significance of the work.[26] In their study of American physicists, the Coles found that the quality of a researcher's work, as perceived by his colleagues, was the single most important influence on his professional reputation.[27] Similarly, Blume and Sinclair found that, for British chemists, both quantity and quality of research were highly correlated with recognition received, quality being somewhat the more important.[28] In addition, it appears that the most productive scientists in terms of quantity are also especially likely to produce results which are judged to be of high quality. This means, therefore, that a minority of scientists will be extensively rewarded both for their large output and for the signficance of their results, while a much larger number of scientist will receive comparatively few rewards.[29]

As a consequence of this marked separation between the mass of scientists and a small number of highly productive and highly rewarded men, the scientific community contains a narrow stratum of eminent researchers. This becomes abundantly clear when we examine the actual distribution of professional rewards. There are many ways in which scientific recognition can be expressed, varying from eponymy and the Nobel Prize, through a multiplicity of other prizes and medals, to membership of national academies, fellowships, honorary degrees and editorships, down to the citation of other scientists' work. These forms of recognition are graded in accordance with different levels of contribution to science and they appear to be intended to reward degrees of scientific accomplishment.[30] Yet, despite their diversity and the fine gradations between them, scientific awards are not widely distributed. In a national sample of American physicists teaching in departments granting PhD's, itself a fairly prestigious grouping within the overall scientific community, 72 per cent had received no honorific awards at all. Even in the top twenty physics departments, only one-third of members appeared to have received any kind of award. In contrast, those few scientists who had received a Nobel Prize or were members of the National Academy of Sciences virtually monopolized all the other awards of any significance.[31] A similar situation exists in relation to citation, the most basic form of scientific reward. Whereas most articles published in scientific journals are cited no more than once, a very small proportion of papers are cited with great frequency.[32] At all levels of scientific recognition, from the Nobel Prize[33] down to the routine citing of research findings, the same marked separation of a small élite from the great majority of scientists is apparent.

Individual scientists, then, appear to be ranked and rewarded in accordance with their perceived contribution to scientific knowledge. Consistent with this proposition is the fact that greater credit is given to theoreticians than to experimentalists; probably because the contributions of the former tend to have wider implications and therefore to affect the work of a larger number of scientists more significantly.[34] At the same time there is selective recruitment into the role of 'theorist', which tends to attract the better qualified students.[35]

Similarly, in the same way that researchers are granted recognition by their colleagues for providing valuable information, specialties, disciplines, and the like, appear to be accorded prestige throughout the research community to the extent to which they are seen to have contributed to the intellectual advance of other fields. The ability to contribute in this way is often associated with the formulation of advanced theoretical schemes and with the study of phenomena which are deemed to be fundamental to numerous fields of inquiry. Thus elementary particle physics has long been regarded as a high prestige specialty, whilst the prestige of solid state physics has been relatively low. Furthermore, in the same way that there is selective recruitment into theoretical roles, high prestige fields tend to attract more students and so are able to choose those with the highest qualifications.[36]

In America and in Britain there is considerable agreement among academics in general about which universities are most prestigious.[37] In both countries, a large proportion of the scientific élite have been trained at these select universities and the majority of major awards are granted to their members. For example, in a recent study it was found that about half of all Nobel Laureates who studied in America received their degrees from four universities, namely, Harvard, Columbia, Berkeley and Princeton, which during the relevant period produced merely 14 per cent of science doctorates.[38] The membership of the National Academy of Sciences is similarly drawn disproportionately from scientists at a few universities, including those mentioned above.[39] In Britain the situation is much the same. In a study of Fellows of the Royal Society in 1971 who had received their first degree from a British university, it was found that three to four times more Fellows than could be expected on the basis of undergraduate numbers alone came from Oxford or Cambridge. Cambridge was particularly over-represented, bettering the national average by a factor of 6.5.[40] These same universities also dominate the Royal Society in terms of current university affiliation. For instance, 65 per cent of Fellows in 1960 were at Oxford or London; and this proportion has remained stable for at least the previous half-century, despite the marked decline during this period in the proportion of scientists employed in these three universities.[41] In short, in both Britain and the USA, there is strong relationship between early location in the university system and eventual success, and a marked concentration of the élite at a few major centres.

It is no doubt true that the eminent scientists located at these élite universities have, in general, produced work which has been widely recognized as significant.[42] In the social sciences as well, there is an indication that 'major advances' are often produced by academics at these few universities.[43] However, there is also evidence that membership of a leading university tends to bring researchers additional professional rewards, ***independently of their contribution to knowledge***. Thus in America the prestige of a scientist's first university tends to be positively correlated with the prestige of his current university, regardless of his productivity.[44] In another American study it was found that scientists are more likely to gain recognition from affiliation with a major university than from high productivity.[45] In this latter study, no attempt was made to control for quality of research output. Blume and Sinclair, in their investigation of British university chemists, found that even with quality of work (measured by peer group assessment) held constant scientists gained from membership of an élite university.

> Among those whose work is of relatively low quality . . 54 per cent of the Oxford and Cambridge chemists have received substantial recognition, compared to 17 per cent of London University chemists, and only 12 per cent of others. Among those of medium quality a similar differential is apparent, although it is less strong . . . At the highest quality level it is not certain that affiliation significantly affects recognition, although it must be borne in mind that the numbers here are rather small.[46]

To some extent, therefore, perhaps with systematic differences between disciplines,[47] scientific recognition is distributed in a way which favours those who are or have been associated with prestigious universities.

The concentration of the scientific élite at a few highly regarded centres is a product of various social processes. There are, for example, processes of self-selection and selective recruitment which tend to channel both the better qualified young men and their eminent elders toward the élite universities.[48] These universities have, on the whole, the best facilities for undergraduate teaching and, consequently, their students are likely to be better prepared for participating in the research community.[49] Moreover, it seems that the most prestigious research centres receive a disproportionate share of research funds both in America and in Britain.[50] For instance, in 1967-68, Oxford and Cambridge received one seventh of all university research grants and contracts, even though they employed only one twelfth of full-time academic staff.[51] It seems likely, therefore, that scientists at Oxbridge and other prestigious centres will have distinctly better facilities and resources for research, compared with their peers elsewhere.

At the same time, younger and less eminent scientists at prestigious universities will benefit in several ways from closer and more frequent contact with members of the élite. For instance, eminent scientists tend to be the focus of numerous informal communication networks.[52] They are, accordingly, well-informed about recent developments and are able to guide their students and junior colleagues into fruitful or fashionable research areas.[53] The opportunity or lack of opportunity to enter a growing field of research during its initial stage of development, when the chances of making a significant contribution are relatively high, may well be one of the major determinants of a successful career in science.[54] Sponsorship by an informed member of the élite is likely to improve one's chances in this respect.[55] Lack of social contact with eminent men will not only increase the likelihood that a young scientist will choose a research problem generally regarded as unimportant, but also that any contribution he does make will go unrecognized.[56] The task of deciding who has produced work justifying entry to the élite is, to some extent, delegated to existing members.[57] This delegation of authority finds expression in the formal allocation of awards. For it is the members of the élite who are most knowledgeable about scientific awards[58] and who play a major part in deciding upon their bequest. Thus the fact that around 50 per cent of American Nobel Laureates have been students of previous prizewinners[59] *may* be due primarily to an extremely efficient, albeit uncoordinated, process of selective recruitment. However, the evidence summarized above seems to furnish reasonable grounds for accepting tentatively that structured differences in facilities, patronage and social contacts are also important.

It appears, then, that members of prestigious centres benefit not only from a biased allocation of recognition, but also from a *differential distribution of opportunities for doing high quality work.* The consequent differences in the receipt of recognition are likely to become self-reinforcing owing to the existence of the Matthew Effect in science, whereby the more eminent a scientist is the greater the credit he will receive for a given contribution.[60] This phenomenon is most evident in cases of collaboration between men of unequal reputation, where recognition tends to be given primarily to the person of greater repute; and in cases of independent multiple discovery, where priority tends to be given to the work of the scientist who is known to have made major contributions before. Merton suggests that although the

Matthew Effect is most evident in such situations, it is likely to operate quite generally in science[61] and to play a part in creating the marked inequality in the distribution of scientific rewards. In other words, the allocation of rewards in science can be seen as working according to a principle whereby any initial advantage or disadvantage becomes cumulative.

On this assumption, if we were to follow a cohort of scientists through their careers, we would expect to find that inequalities in productivity and in receipt of rewards steadily increased. Allison and Stewart have recently attempted to find out whether or not this does actually happen.[62] Their method was to divide a cross-sectional population of biologists, mathematicians, chemists and physicists into several strata according to their career age. These strata were taken as representing a single cohort in its passage through time. Apart from biology, they found that the distribution of publications and of citations in each of the disciplines does become increasingly unequal over time. They conclude that the differences in individual productivity and recognition which appear early in the career of scientific cohorts, and which may be associated at that point with underlying diffferences in ability, motivation, patronage, points of entry into research, and so on, continue to increase cumulatively, presumably as a consequence of social processes such as the Matthew Effect which occur within the research community.

I have so far given copious evidence of the existence of a small élite stratum in science, as well as some indication of how this élite is created and sustained. A prominent feature of this élite is that its members tend to cluster within a few select universities. Given this fact, we would expect them to interact informally with more than average frequency; and as we have noted, they tend to have had close contact with many of the younger scientists who are destined to enter the élite in due course. In addition, there is clear evidence that members of the élite, even when they are geographically separated, are influenced primarily by the work of other élite scientists and are most intensively engaged in professional communication with their peers.[63] All that remains to be shown, therefore, in order to demonstrate that the scientific élite meets all the requirements given at the beginning of this section, is that this élite exerts considerable influence over the professional activities of non-élite members.

There can be little doubt that this is so. Firstly, it is clear that members of the scientific élite tend to occupy positions of formal authority. They are often, for example, heads of large research groups and are mainly responsible for directing the long term utilization of the costly equipment involved and for guiding the work of less senior members.[64] Such groups are always faced with a wide range of significant problems, only a few of which can be pursued. The resulting choices between competing avenues of research always require the approval of eminent group members and are usually made directly by these members.[65] Elite scientists also dominate those committees which allocate research funds to individual scientists, to research groups and to areas of inquiry.[66] In addition, as we have noted above, eminent scientists exert a great deal of informal influence. Because the criteria of scientific adequacy and value within any research area are so specialized and subtle, the task of deciding which contributions are acceptable and which are important tends to be given to those who are already recognized as having contributed significantly to the field, and, therefore, as having command of the necessary intellectual skills.[67] Thus those scientists who wish to advance their careers and/or to produce acceptable contributions to knowledge must comply with the cognitive standards set and exemplified by these leaders; whilst those who aspire to élite membership must be prepared to undertake the same prolonged commitment as their leaders to the solution of problems defined as significant in accordance with the culture of the research community.

GOVERNMENT AND THE ACADEMIC ELITE OF SCIENCE

Membership of the scientific élite is made possible by one's professional reputation. Almost by definition, therefore, the names and main achievements of members of the élite are likely to be well known to other researchers. To put this another way, members of the élite are likely to be highly visible within the scientific community. This has been documented by the Coles who have shown, for a sample of American physicists, that only a few were highly visible to other physicists and that 61 per cent of the variance of visibility could be accounted for by the effect of quality of work, rank of department and prestige of highest award.[68] In addition, they showed that visibility varied, independently of quality, from one specialty to another; men working in elementary particle physics being most visible on average, those in atomic and molecular physics being somewhat less visible, whilst solid state physicists were least visible. It appears then, that there are structured differences in visibility among scientists which correspond closely to differences in professional recognition. This fact is important because it leads to the reasonable inference that eminent scientists and recognized centres of scientific excellence are likely to be most visible, not only to active scientists, but also perhaps to people outside the research community. When influential laymen seek to make contact with the research community, it seems likely, therefore, that they will do so via the highly visible scientific élite.[69]

Since the earliest days of modern science, attempts have been made by laymen to influence the development of scientific knowledge by gaining the active cooperation of eminent scientists; and periodically members of the scientific élite have sought to influence national policy, usually, but not always, in relation to the issue of government support for science.[70] But it is only in very recent decades that the intermediary role of the scientific élite has become firmly institutionalized, both in Britain and America. Before the Second World War there were few direct and effective links in America between government and the academic research community.[71] But this situation was transformed by the Manhattan Project, after which certain branches of science became increasingly regarded by politicians and by scientists as being of great military, economic and hence, political significance. Consequently, during the late 1940s and 1950s, a complex hierarchical system of scientific 'advisory' bodies was gradually established in parallel with the bureaucratic hierarchy of government.[72] The members of this network have been engaged in advising government on issues which have a large technical component, in helping to implement major technological programmes such as that involved in putting a man on the Moon, and in regulating government support for academic research.[73] The number of scientists participating in this advisory machinery in America appears to be very small. Wood has estimated that, for federal issues, between 200 and 1,000 individuals directly influence the availability of scientific knowledge and its application.[74] Rosenthal, working from information contained in *American Men of Science*, arrived at a core group of 900 scientists, with an 'active élite' of around 400. In Mullins' view, however, these estimates are too small. He suggests that 'at least 1,500 people have participated in the top level committees since 1950'.[75] These men tend to be significantly older than American scientists in general, to have come from a small number of élite centres and to have made at least a respectable and usually a major contribution to science.[76]

The situation in Britain, although different in many details from that in America, is similar in certain crucial respects. A series of advisory committees and research councils has been created, within which the connections between government and the research community have been institutionalized. This advisory system has been arranged hierarchically; committees at the lower end of the hierarchy being concerned with relatively specific scientific issues, such as the evaluation and funding of particular research proposals, whilst those at the higher end deal preponderantly with decisions involving social, economic and political, as well as purely scientific, considerations. Membership of these committees is seen by scientists as a reward for scientific achievement and it is bestowed exclusively upon those whose intellectual stature is widely recognized. Probably in part because the administrative and funding system of British science is more highly centralized than that in America,[77] its advisory élite appears to be even more socially homogeneous.[78] In particular, as we would expect in the light of the earlier discussion, it is dominated by members of the Royal Society and by scientists educated at Oxford, London and, especially, Cambridge.[79]

It is reasonably clear that at the national level there is, in both countries, a fairly small number of scientists actively engaged in regulating the relationships between government and academic research; that these scientists tend to be associated with a few recognized centres of scientific excellence; and that a large proportion of those involved are members of that intellectual élite which arises out of the exchange of professional recognition for valued information within the research community. What remains to be examined here is the impact of this élite upon national policy. This issue raises two reasonably distinct questions. The first is: How and to what extent does this élite influence the formulation and implementation of governmental policy toward science? The second is: How and to what extent does the élite influence governmental policy toward issues other than the development of science itself? I have separated these two broad issues in order to allow for the possibility that scientists vary in their response to political pressure depending on whether or not their own professional activities are directly involved. The second issue is too big to be discussed usefully in this brief paper. In the next section, therefore, I shall deal only with the first of these two questions, and I shall concentrate on academic science in Britain.[80]

THE CONTROL OF ACADEMIC RESEARCH

There can be no doubt that government support of academic research is primarily instrumental.[81] The massive sums of money necessary to maintain modern, high technology science are furnished because 'basic', academic research is seen as being the fundamental source of military and industrial technology.[82] It is often admitted that fundamental research leads to practical results by rather a roundabout route. But this does not mean that the motive for supporting it is not utilitarian.[83] Indeed, in recent years, there has been increasing concern in official circles to regulate 'basic' research so as to ensure that its practical 'benefits' are forthcoming as directly and as quickly as possible. Thus in a recent official report 'uncontrolled basic research' was likened to a form of scientific roulette, the results of which were extremely unpredictable, and therefore likely to meet the 'country's needs' only by chance.[84] Consequently, it was recommended that there should be a much greater degree of control over basic research from outside the research community, by those who were responsible for identifying and meeting these 'needs'. This policy of attempting to control academic research so as to maximize practical pay-off is often given at least verbal support by official scientific organizations. The following statement, for instance, is taken from the Science Research Council Report[85] for 1970-71 and is a typical expression of the kind of justification of basic research which appears to be required in the present day political climate.

> Much of [the research suppported by the SRC] is deliberately chosen to underpin future application and development in particular fields ... but the Council would expect others to assume the responsibilities of particular applications in view. In many cases, an important aim of SRC support for long-term basic research is to enable university departments to give help to industry and Government Departments on shorter-term projects and thus bring more social and industrial relevance to their research and teaching.[86]

During the 1960s a science policy, involving an attempt to regulate financial support to the various fields of basic research in accordance with their estimated tendency to generate practical 'benefits', has become increasingly explicit in government circles in Britain and elsewhere.[87] As we have seen in the quotation given immediately above (and there are many statements in SRC Reports expressing the same idea), this policy appears to have been formally endorsed by that body which is responsible for financing most of the scientific research carried out in British universities. However, we must remember that formal statements of intent on the part of bodies such as the SRC are likely to be formulated in the light of the current political climate and in response to the demands currently being made on the academic research community. In other words, such statements are not necessarily an accurate guide to the way in which academic research grants are actually distributed. Moreover, from the analysis above of the reward system of academic science and of the way in which the scientific élite is created, we would hardly expect the leaders of the research community to be intensely concerned with the practical uses of scientific knowledge. Throughout their careers they have been rewarded for single-minded pursuit of specialized knowledge and for their mastery of esoteric skills. Their ascent of the scientific hierarchy has been brought about by strong and prolonged commitment to scientifically defined goals. In general, it is clear that concern with the industrial or social application of scientific knowledge is irrelevant to success within academic science, even in those disciplines which have historically a close connection with industry and government. Thus Blume and Sinclair found that very few of their large sample of British university chemists were interested in the practical implications of their work, and that those who were interested tended to be least integrated into the academic research community and to be relatively unlikely to have made a significant contribution to scientific knowledge.[88] This is the only available study which explores directly the connection between the receipt of professional rewards in science and concern with external issues. There are other studies, however, which show, first of all that it is unusual for researchers to be concerned about the political or moral implications of scientific work[89] and, secondly, that academic scientists tend to support strongly the principle that they should be free to choose their own problems in terms of intellectual rather than practical significance.[90]

In view of these considerations, we would expect the scientific élite, the members of which have conformed to the highest degree to the requirements of the research community, to be extremely unreceptive to attempts by laymen to define closely their professional objectives. Furthermore, their resistance to external pressures is likely to be particularly effective whenever the system of informal social control by competent colleagues, which is so important in maintaining commitment to research goals within the scientific community, extends into the governmental decision-making machinery as it does in Britain. Thus the Council of the SRC, although it always contains a few representatives from industry, is dominated by university scientists. The same is true of the more specialized research boards within the SRC. Accordingly, one of the central and indeed explicit principles guiding the distribution of research funds by the SRC is that of assessment by competent scientists in terms of scientific merit;[91] that is, funds are disbursed among competing research proposals in much the same way, and by much the same criteria, as professional recognition is distributed among competing research findings. At the same time, as we have noted, decisions on the support of research and postgraduate training are also supposed to be taken in the widest context of national needs. In official documents, no indication is normally given that the principle of allocation in terms of scientific merit and that of allocation in terms of national need may sometimes be incompatible. As long as we have no direct record of the discussions in which research funds are allocated, we must use indirect means to investigate how the scientific-political élite carries out this task. We can gain some insight into the processes at work by comparing the actual distribution of research funds with what we would expect on the basis of differing allocative principles.

If the principle was being fully implemented of distributing basic research funds in accordance with likely practical benefit, the latter being conceived primarily in economic terms, we would expect to find that fields such as solid state physics would receive disproportionately great support owing to their relatively close links with industrial technology. In contast, if the principle of scientific merit was paramount, we would expect to find that fields with high scientific prestige, such as high energy physics, radio astronomy and molecular biology, would receive a disproportionately large share of the funds, despite the general recognition that they have a low probability of practical, economic pay-off.[92] Official figures on the distribution of basic research funds do not easily reveal the amount allocated to particular scientific specialties. Nevertheless, although the following conclusion can undoubtedly be refined by more detailed analysis than is attempted here, in general it appears that there is a pronounced bias in Britain in favour of fields with high scientific status but low economic relevance. To illustrate this, let us look in particular at high energy physics and at astronomy which, together, appear to absorb not far short of two thirds of SRC research funds.

> High energy physics is one of the main fronts of science. . . . It tries to establish the fundamental laws of physics which are at the base of all we know about matter.[93]

Accordingly, as we have seen above, it is widely regarded by research scientists as an important and prestigious field and tends to attract a good proportion of gifted students. It is not noted, however, for its contribution to productive technology or to economic growth.

> The advances in elementary particle physics and the new astronomical discoveries open up the greatest challenges to scientific curiosity. Further work in these fields may well find its justification primarily on cultural and educational criteria, rather than on expected military or economic spin-off.[94]

In other words, support for research on elementary particles can only be strongly justified by the principle of scientific merit; for unless its results were scientifically valuable this specialty could make no claim to be outstanding in cultural or educational terms. Yet the amounts of money needed and obtained by high energy physics are enormous. In 1968, for example, 43 per cent of the total SRC budget for that year was devoted to research in high energy physics. To put this another way, the amount of money allocated for research to the 200 or so high energy physicists in Britain was equal to one tenth of the sum distributed by the UGC for operating the university system as a whole.[95] In his study of high energy physics in Great Britain, Gaston reaches the following conclusion:

> The fact that the number of scientists who spend this proportion [of research funds] does not approximate their proportion of all scientists in Great Britain underscores the expensive characteristics of this Big Science and emphasizes the relative value that the scientific community (through the Council for Scientific Policy and the Science Research Council) puts on HEP as a research priority.[96]

A similar situation appears to exist in Britain in relation to astronomy and related fields. In 1972-73, for example, approximately 20 per cent of direct expenditures by the SRC was devoted to astronomical research, the upkeep of optical and radio observatories, and of space research stations.[97] Once again, this is out of all proportion to the number of scientists involved. Furthermore, although the broad area of study includes 'space research', the work undertaken appears to have little likelihood of technological pay-off. For, as the SRC Report for 1972-73 states, about two thirds of the space research programme is in support of astrophysics (i.e., the understanding of the physics of celestial bodies and celestial processes). It is, of course, true that astronomy and space research, like high energy physics, are necessarily capital-intensive areas of research. Consequently, if research in these fields is undertaken at all, it is bound to absorb a disproportionate share of resources. But this begs the central question. In a situation in which government policy, and the professed policy of the SRC, is to combine the maintenance of high quality basic research with systematic concern for national, and particularly economic, needs, how is it that such a large proportion of resources is devoted to intellectually significant but economically unproductive fields?[98] In the light of the earlier discussion, part of the answer seems to be that the scientific élite, which is primarily responsible for regulating the growth of academic science, is strongly committed to the ethos of the basic research community and also oriented to that community as its main audience.

In Britain there is no single formal mechanism for directing the evolution of academic science. Instead, various kinds of committees are formed at irregular intervals to report back to the funding agencies on developments in specific fields and to advise on the nature and degree of support required by those fields. The reports of such committees provide further indirect evidence about the processes by which support is distributed. There is no space here for more than a cursory reference to this source of data. Let me simply suggest a few of the features which seem to be characteristic of these reports.[99] In the first place, presumably because complex judgements of scientific achievement and promise are involved, responsibility for formulating policy tends to be placed initially in the hands of leading participants from the fields in question and from neighbouring areas. Secondly, although questions of economic and social benefit are sometimes raised, the focus of discussion tends to be consistently narrowed in such a way that recommendations can be justified solely on grounds of scientific significance. Accordingly, claims are typically advanced for very substantial financial support almost entirely on the basis of a discussion amongst eminent research specialists of narrowly defined scientific issues.

There is one further kind of evidence which provides support for the idea that attempts to guide the academic scientific community in a utilitarian direction have been on the whole relatively ineffective, namely, that it is difficult to show any strong links between the development of economic technology and advances in academic science. For instance, Schmookler has shown that in America, technological innovation in four important industries, railroads, agriculture, paper making and petroleum refining, occurred in response to economic demand and not as a spin-off from fundamental research.[100] Similarly, Langrish and his colleagues found little evidence of any direct, observable link between university science and technological innovation in British industry:

> We have paid particular attention to the relation of basic science to innovation [in economic technology]. . . our failure to find more than a small handful of direct connections is the more striking for the fact that we set out deliberately to look for them.[101]

Although these findings are fairly clear-cut, their interpretation is far from easy. It may be, for example, that advances in academic science are usually passed on to succeeding generations of students before these students carry the new knowledge into social contexts where its practical applications can be perceived and pursued.[102] Moreover, the application of scientific knowledge may depend more on economic entrepreneurship than on the control of basic research in terms of criteria of applicability, at least partly because it is more difficult for the research scientist to identify areas of practical application than it is for the technologist to select the body of scientific knowledge appropriate to his needs. Nevertheless, despite the difficulties of precise interpretation, these findings provide tentative support for the argument that the British scientific élite has successfully resisted strong utilitarian pressures for research which has fairly clear, short-term practical application.[103]

CONCLUDING REMARKS

The main line of argument developed in preceding sections can be briefly summarized as follows. In Britain and America, the academic research community contains a small, influential and socially visible scientific élite. Although entry to the research community itself is significantly affected by broad social factors, recruitment into the élite from among research scientists is largely independent of external influences. The élite is created by a series of processes operating within the research community and within the university system, whereby professional recognition of scientific achievement becomes distributed in a markedly unequal fashion. These processes produce an élite, the members of which are widely regarded by fellow scientists as having made a major contribution to scientific knowledge. The members of the élite tend to be those who have demonstrated the highest commitment to an ethos in which the search for scientific knowledge is the highest objective and needs no further justification. It is primarily via this élite that the links are forged between the academic research community, on the one hand, and the wider society, and government in particular, on the other hand.

In Britain, at the highest levels of science policy, it is clear that politicians and civil servants play a decisive role; for example, in determining what proportion of total public expenditure is to be devoted to scientific research. In addition, it is clear, not only that these outsiders wish to ensure that the increasingly costly research activities of academic scientists generate practical, economic benefits, but also that this utilitarian rationale for fundamental research is frequently endorsed verbally by research scientists themselves. However, when science policy comes to be implemented in detail, involving the allocation of resources between particular areas of research, the technically competent scientific élite assumes the dominant role. At this more specific level, the deliberations of élite members are usually shielded from direct pressure and, consequently, the decisions which ensue tend to be based almost exclusively on considerations of scientific significance. As a result, there is minimal contact between academic science and industrial use, and a marked bias in the allocation of support in favour of scientifically prestigious fields rather than those whose findings seem most likely to lead to economic application.

It must be stressed that this thesis selects for attention only one part of the complex series of relationships which links research scientists to the wider society. I have concentrated, for example, on the distribution of research funds by the SRC, because this body is the main source of support for academic science. However, the entire expenditure on all the Research Councils amounts only to about one sixth of the Government's total R&D bill,[104] most of which is devoted to research having clear practical (including military) objectives. In addition, the deployment of the massive sums spent on industrial research is mainly controlled by non-researchers and is intended to bring demonstrable economic returns. Moreover, capital-intensive academic research itself generates contracts, jobs and profits for industry. These points cannot be denied; nor can they be ignored when we wish to describe the place of science overall in industrial societies. The scientific community in general is intimately involved in the industrial system of economic and military production. My concern here, however, has been to examine only how academic research fits into the broader context and to present a preliminary analysis of the role played by the academic élite of science.

The argument presented above must be regarded as tentative and incomplete. If the analysis is to be taken further, we need to have numerous detailed studies of the part played by the élite in promoting and restraining the development of particular research areas. This is likely to involve our recognizing explicitly that 'the scientific élite' is not, in practice, an entirely homogeneous, solidary social grouping, but is internally divided and often characterized by conflicts of interest.[105] Thus, by extending our analysis of the research community to include external, political relationships, we are led to perceive more clearly the need for a political analysis of the research community itself.[106]

Another line of investigation which arises from the argument developed in earlier sections is that of exploring the effects of differences in political and administrative context upon the actions of the scientific élite. In Britain, despite the highly centralized system of government funding, the academic élite has resisted attempts at external control with considerable success. In other countries, the context within which the élite operates is often rather different. In America, for example, the university system and the sources of financial support are much more diversified and uncoordinated than in Britain. It has been argued that, as a result, the American scientific community is more internally divided, more competitive and, hence, more innovative.[107] In the light of our present discussion, we must also ask how such organizational differences influence the cohesion of scientific élites and, consequently, whether scientific élites in America are more or less effective in pursuing their own interests than in Britain.

We also clearly need to investigate more closely the extent to which, and the ways in which, the findings of academic science influence economic development; the means by which the scientific élite is able to maintain its exchange relationship with government without responding positively to the demand for tangible results; and the extent to which, and the circumstances under which, social influences upon the conduct of scientific research are transmitted by *non*-élite scientists. These, then, are some of the issues and avenues of investigation which arise from, and are highlighted by, this attempt to explore the mediating role of the scientific élite. In addressing ourselves to such problems we will be returning, with an improved understanding of the nature of the research community, to those broader questions in the sociology of science which have received little attention in recent years.

NOTES

This paper was presented to the Conference on the Sociology of Science held at the University of York, UK, on 16-18 September 1975. I wish to thank Brian Wynne and David Edge for commenting helpfully on the first draft of this paper.

1. R.K. Merton, *Science, Technology and Society in Seventeenth Century England* (New York: Harper and Row, 1970). (Originally published 1938.)

2. R.K. Merton, 'The Normative Structure of Science', in R.K. Merton, *The Sociology of Science* (Chicago and London: University of Chicago Press, 1973). (Originally published 1942.)

3. B. Barber, *Science and the Social Order* (New York: The Free Press, 1952).

4. This point has been made strongly by S.S. Blume, *Toward a Political Sociology of Science* (New York and London: The Free Press, 1974).

5. For example, S. and J. Cole, 'Visibility and the Structural Bases of Awareness of Scientific Research', *American Sociological Review*, Vol. 33 (June 1968), 397-412.

6. For example, J. Gaston, 'The Reward System in British Science', *American Sociological Review*, Vol. 35 (August 1970), 718-32.

7. For example, N. W. Storer, *The Social System of Science* (New York: Holt, Rinehart and Winston, 1966).

8. For example, D. Crane, *Invisible Colleges* (Chicago: University of Chicago Press, 1972).

9. For discussion of élites, see T.B. Bottomore, *Elites and Society* (London: Watts, 1966); also A. Giddens, 'Elites in the British Class Structure', in P.H. Stanworth and A. Giddens (eds), *Elites and Power in British Society* (London: Cambridge University Press, 1974).

10. Because I am concerned here primarily with relations between the research community and the wider society, I have necessarily avoided a full discussion of the complexities of the élite structure of science. Such a discussion would deal with various kinds of scientific élite and would recognize that what I have called 'the scientific élite' is made up of a complicated series of overlapping élites, which are themselves often in competition. See J.R. Ravetz, *Scientific Knowledge and Its Social Problems* (Oxford: Clarendon Press, 1971); also R.D. Whitley, 'Umbrella and Polytheistic Disciplines and their Elites', elsewhere in this issue.

11. The range of phenomena regarded as 'natural' will vary over time and from one society and social group to another.

12. See relevant sections of B. Barnes (ed.), *Sociology of Science* (Harmondsworth, Middx.: Penguin Books, 1972).

13. M.J. Mulkay, G.N. Gilbert and S.W. Woolgar, 'Problem Areas and Research Networks in Science', *Sociology*, Vol. 9, No. 2 (1975), 187-203.

14. S. and J. Cole, *Social Stratification in Science* (Chicago and London: University of Chicago Press, 1973), Chapter 8.

15. M.J. Mulkay and D.O. Edge, 'Social, Cognitive and Technical Factors in the Growth of Radio Astronomy', *Social Science Information*, Vol. 12, No. 6 (1973), 25-61.

16. J. Ziman, *Public Knowledge* (London: Cambridge University Press, 1968).

17. But see H. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon', *Sociology*, Vol. 9, No. 2 (1975), 205-24.

18. T.S. Kuhn, 'The Function of Measurement in Modern Physical Science', in A.C. Crombie (ed.), *Symposium on the History of Science* (London: Heinemann, 1963), 31-60.

19. W.O. Hagstrom, *The Scientific Community* (New York: Basic Books, 1965).

20. R.K. Merton, 'Priorities in Scientific Discovery', in *The Sociology of Science* op. cit. note 2.

21. Cole and Cole, op. cit. note 14.

22. Op. cit. note 6.

23. Op. cit. note 14. It should be recognized that women and certain racial and class groups are considerably under-represented in the scientific community. These kinds of 'particularistic' factors are clearly of very great importance in determining *entry* to the scientific community.

24. A.J. Lotka, 'The Frequency Distribution of Scientific Productivity', *Journal of the Washington Academy of Sciences*, Vol. 16 (19 June 1926), 317.

25. D. J. de Solla Price, *Little Science, Big Science* (New York and London: Columbia University Press, 1963), 43.

26. The association between receipt of recognition and production of results deemed to be 'significant' or of 'high quality' (quality and significance are indistinguishable in most studies) may well be exaggerated by the procedures which are normally used to measure these variables. This is so because there is often conceptual overlap between the indicators of recognition and of quality. Consequently, the apparent association between these variables may be, to an unknown extent, a product of our research techniques. Moreover, the processes whereby results come to be defined as 'significant' have been little explored by sociologists. See N. Shanin-Cohen, 'Innovation and Citation' (Unpublished paper, National Council for Research and Development, Hebrew University of Jerusalem, 1974).

27. Op. cit. note 14.

28. S.S. Blume and R.Sinclair, 'Chemists in British Universities: A Study of the Reward System in Science', *American Sociological Review*, Vol. 38 (February 1973), 126-38.

29. Cole and Cole, op. cit. note 14.

30. R.K. Merton, 'Priorities in Scientific Discovery', op. cit. note 20.

31. Cole and Cole, op. cit. note 14.

32. H. Zuckerman, 'Stratification in American Science', *Sociological Inquiry*, Vol. 40 (Spring 1970), 235-57; Price, op. cit. note 25.

33. H. Zuckerman, 'Nobel Laureates in Science: Patterns of Productivity, Collaboration, and Authorship', *American Sociological Review*, Vol. 32 (June 1967), 391-403.

34. I.I. Mitroff, *The Subjective Side of Science* (Amsterdam: Elsevier, 1974), 91.

35. Gaston, op. cit. note 6.

36. Hagstrom, op. cit. note 19.

37. A.H. Halsey and M. Trow, *The British Academics* (London: Faber and Faber, 1971); A.M. Cartter, *An Assessment of Quality in Graduate Education* (Washington, D.C.: American Council on Education, 1966).

38. H. Zuckerman, 'The Sociology of the Nobel Prizes', *Scientific American* (November 1967), 25-33.

39. Zuckerman, op. cit. note 32.

40. H. Eisner, 'University of the FRS', *New Scientist* (25 January 1973), 197.

41. Halsey and Trow, op. cit. note 37.

42. Ibid.; Zuckerman, op. cit. note 32; Cole and Cole, op. cit. note 13; L. Moulin, 'The Nobel Prizes for the Sciences from 1901-1950', *British Journal of Sociology*, Vol. 6 (1955), 246-63.

43. K.W. Deutsch, J. Platt, and D. Senghaas, 'Conditions Favouring Major Advances in Social Science', *Science*, Vol. 171 (5 February 1971), 450-59. The research methods used in this study are open to criticism.

44. L. Hargens and W.O. Hagstrom, 'Sponsored and Contest Mobility of American Academic Scientists', *Sociology of Education*, Vol. 40 (Winter 1967), 24-38.

45. D. Crane, 'Scientists at Major and Minor Universities', *American Sociological Review*, Vol. 30 (October 1965), 699-714.

46. Blume and Sinclair, op. cit. note 28, 135.

47. Gaston, who did not control for quality of output, found no connection between university affiliation and recognition for British high energy physics. However, the number of British universities engaged in this field is rather small and there may be a tendency for high energy physicists to be located at élite centres. See note 6.

48. R.H. Knapp and J.J. Greenbaum, *The Younger American Scholar: His Collegiate Origins* (Chicago: University of Chicago Press, 1953); J.L. Holland, 'Undergraduate Origins of American Scientists', *Science*, Vol. 126 (6 September 1957), 433-37; Halsey and Trow, op. cit. note 37.

49. Robbins Report on *Higher Education* (London: HMSO, 1963).

50. L. Groeneveld, N. Koller and N. Mullins, 'The Advisers of the U.S. National Science Foundation', *Social Studies of Science*, Vol. 5, No. 3 (August 1975), 343-54.

51. These figures have been computed from Tables 4.1 and 7.1 in Halsey and Trow, op. cit. note 37, Chapter 10.

52. Crane, op. cit. note 8.

53. Hagstrom, op. cit. note 19; B. Barber, 'The Functions and Dysfunctions of Fashion in Science', *Mens en Maatschappij*, Vol. 43, No. 6 (1968), 501-14.

54. G.N. Gilbert, 'The Development of Scientific Specialties: the Case of Radar Meteor Astronomy', to be published in G.L. Lemaine et al. (eds), *New Perspectives on the Emergence of Scientific Disciplines* (Paris: Mouton, 1976).

55. M.J. Mulkay, *The Social Process of Innovation: A Study in the Sociology of Science* (London: Macmillan, 1972).

56. W.D. Garvey and B.C. Griffith, 'Scientific Information Exchange in Psychology', *Science*, Vol. 146 (25 December 1964), 1955-59.

57. Zuckerman, op. cit. note 32; Ziman, op. cit. note 16.

58. Cole and Cole, op. cit. note 14.

59. Zuckerman, op. cit. note 32.

60. R.K. Merton, 'The Matthew Effect in Science', in *The Sociology of Science*, op. cit. note 2.

61. The Coles' attempt to explore the extent of the Matthew Effect is methodologically unconvincing. See op. cit. note 14, Chapter 7.

62. P.D. Allison and J.A. Stewart, 'Productivity Differences among Scientists: Evidence for Accumulative Advantage', *American Sociological Review*, Vol. 39 (August 1974), 596-606; see also Cole and Cole, op. cit. note 14.

63. Price, op. cit. note 25; Cole and Cole, op. cit. note 14; Crane, op. cit. note 8.

64. E.g., G.M. Swatez, 'The Social Organization of a University Laboratory', *Minerva*, Vol. 8, No. 1 (January 1970), 36-58.

65. G. Lemaine, B-P. Lecuyer et al., *Les Voies du Succes: sur quelques facteurs de la reussite des laboratoires de recherche fondamentale en France* (Paris: CNRS, 1972), offset.

66. Blume, op. cit. note 4.

67. Ravetz, op. cit. note 10.

68. Op. cit. note 5. A scientist's visbility score was the percentage of respondents who said that they were familiar with his work.

69. W. Krohn and P. Weingart, 'The Origins of Agricultural Chemistry', to be published in Lemaine et al., op. cit. note 54; J.D. Bernal, *Science and Industry in the Nineteenth Century* (London: Routledge and Kegan Paul, 1953).

70. J. Haberer, *Politics and the Community of Science* (New York and London: Van Nostrand Reinhold, 1969).

71. R. Lapp, *The New Priesthood: The Scientific Elite and the Uses of Power* (New York: Harper and Row, 1965).

72. R. Gilpin and C. Wright (eds), *Scientists and National Policy Making* (New York and London: Columbia University Press, 1964).

73. D.K. Price, *Government and Science* (New York: New York University Press, 1954).

74. R.C. Wood, 'Scientists and Politics', in Gilpin and Wright, op. cit. note 72.

75. In op. cit. note 72; also D.S. Greenberg, *The Politics of American Science* (Harmondsworth, Middx.: Penguin Books, 1969); and N.C. Mullins, private communication.

76. Two careful quantitative studies have been carried out in America by Mullins and his associates: N.C. Mullins, 'The Structure of an Elite: the Advisory Structure of the U.S. Public Health Service', *Science Studies*,

Vol. 2 (1972) 3-29; and L. Groeneveld et al., 'The Advisers of the U.S. National Science Foundation', op. cit. note 50. In the first study, it was concluded that, although on the whole the turnover of PHS advisers is rapid, there does appear to be a small group which has remained in the PHS for some time and which has moved into positions of influence. See also the forthcoming book by Mullins, *Power, Social Structure and Advice in American Science: The United States Federal Science Advisory System from 1950-1973.* There is an urgent need for studies of this kind to be carried out in Britain.

77. J. Ben-David, *Fundamental Research and the Universities* (Paris: OECD, 1968).

78. Compare the figures given by Blume, op. cit. note 4, Chapter 6, with those in Groeneveld et al., op. cit. note 50.

79. Blume, op. cit. note 4, Chapter 6.

80. For an account of the wider political context, see N.J. Vig, *Science and Technology in British Politics* (London: Pergamon Press, 1968).

81. A. de Rueck, M. Goldsmith and J. Knight (eds), *Decision Making in National Science Policy* (London: Churchill, 1968).

82. L. Sklair, *Organised Knowledge* (London: Hart-Davis, MacGibbon, 1973).

83. J. Salomon, *Science and Politics* (London: Macmillan, 1973).

84. *A Framework for Government Research and Development* (London: HMSO, 1971). (The Rothschild Report.)

85. The SRC provides most of the funds for academic research in astronomy, biology, chemistry, mathematics and computing science, engineering and physics. The other major source of funds for academic research is the UGC. See Council for Scientific Policy, *Report of a Study on the Support of Scientific Research in the Universities* (London: HMSO, 1971).

86. SRC, *Report of the Council for the year 1970-71* (London: HMSO, 1971), 2.

87. C. Freeman et al., 'The Goals of R and D in the 1970s', *Science Studies,* Vol. 1, Nos. 3/4 (October 1971), 357-406.

88. Op. cit. note 28.

89. Mitroff, op. cit. note 34, 172.

90. Hagstrom, op. cit. note 19, 106.

91. SRC Report, op. cit. note 86.

92. On the whole, government is interested in short-term pay-off. Thus Rothschild refers to a delay of 7 years between basic research and practical application as being in many cases unavoidable, but significantly longer delays are to be avoided. There is general agreement that any practical benefits from advances in molecular biology or high energy physics will develop, if at all, on a much longer time scale. See note 84.

93. High Energy Physics Advisory Panel, 'The Status and Problems of High Energy Physics Today', *Science,* Vol. 161 (5 July 1968), 11.

94. Freeman et al., op. cit. note 87, 405.

95. J. Gaston, *Originality and Competition in Science* (Chicago and London: University of Chicago Press, 1973).

96. Ibid., 17.

97. SRC, *Report of the Council for the year 1972-3* (London: HMSO, 1973).

98. I should make it clear, perhaps, that I have no particular sympathy for

this instrumental policy towards science.

99. See, for example, the *Report of the Working Group on Molecular Biology* (London: HMSO, 1968). The eight members of this committee, all FRS, claim that there is a considerable gap between the 50 to 100 leading molecular biologists in Britain and the remaining 1,700 or so. They recommend the establishment of new centres of excellence in order to balance those already in existence at Oxbridge and London, and 'to focus the relevant scientific disciplines in a common attack on fundamental biological problems' (p. 9). See also the *Report of the Working Group on the Implications of the Proposed 300 GeV Accelerator* (London: HMSO, 1968). This committee (seven members, all FRS) recommends a long term commitment, rising to £6 million p.a., even though it recognizes that economic and technological benefits are 'mainly indirect and at present marginal' (p. 23). See also the SRC report on *Radio Astronomy* (London: HMSO, 1965). This committee (twelve members, nine FRS) recommends a policy of special encouragements and financial support, despite the fact that 'the economic value of radio astronomy is probably small' (p. 4). For some North American examples, see *Physics in Canada* (Ottawa: Science Secretariat, Special Study No. 2, May 1967).

100. J. Schmockler, *Invention and Economic Growth* (Harvard: Harvard University Press, 1966).

101. J. Langrish et al., *Wealth from Knowledge* (London: Macmillan, 1972, xii.

102. D.J. de Solla Price, 'Science and Technology: Distinctions and Interrelationships', in Barnes (ed.), op. cit. note 12; see also M. Gibbons and R. Johnston, 'The Roles of Science in Technological Innovation', *Research Policy*, Vol. 3 (1974), 220-42.

103. No account is taken here of recent changes following the Rothschild Report; see note 84.

104. R. Williams, 'Independence and Accountability after Rothschild', *R & D Management*, Vol. 2, No. 3 (1972), 131-35.

105. Greenberg, op. cit. note 75.

106. A first step in this direction has been taken by M. Blisset, *Politics in Science* (Boston: Little, Brown and Co., 1972).

107. Ben-David, op. cit. note 77.

B. Alternative Criteria for Scientific Choice?

Social Merit as a Criterion of Scientific Choice: Its Application in Dutch Science Policy

I. TH. M. SNELLEN

TWENTY years ago Dr Alvin Weinberg enumerated the criteria for scientific choice.[1] He distinguished between the "internal criteria" of recent and prospective quality of research into particular problems and "external criteria" such as technological merit and social merit. The assessment of the likelihood that research of high quality on a given set of problems will be done can be made on the basis of assessments of the achievements of workers in the field in question, and of reflective opinions by qualified scientists about the ripeness of the field for further important developments and its implications for progress in adjacent fields. The external criteria are to be applied on the basis of urgency of the practical problems to which the prospective knowledge is to be applied. The urgency of the practical problems is difficult to assess, yet that is what must be done if the criterion of "social merit" is to be applied. Since Dr Weinberg's original paper, the search for reliable and preferably quantitative methods for the making of decisions in science policy has not ceased. Computational methods of determining the urgency of the practical problems have become more and more sophisticated. After the rather simple cost-benefit analyses in monetary terms of the early years of the 1970s, cost-effectiveness analysis in which non-monetary values are considered has been explored. This inclusion of non-monetary measures has improved the realism of the computations, but has made it impossible to find a single measure for the evaluation of the effects which are expected to flow from the acquisition of knowledge and its application. The analysis has been improved by the assignment of weights to the various objectives, and in this manner the scores of costs and expected benefits have been rendered comparable.

The assignment of weights to the social merits of competing claims for the funds to be allocated has, however, been rather arbitrary. This is a major difficulty of this method of determining the merits of alternative objectives. Recently new methods have been developed, in an effort to meet the objections against cost-benefit analysis, by estimation of the relative merits of different, alternative objectives. A new procedure for deciding which objectives are more urgent has been applied in the Netherlands. This new method proceeds through the comparison of pairs of alternative programmes.[2] The precise results produced by comparisons of pairs of alternative objectives and programmes reduces the arbitrariness of the estimates of costs, benefits and other effects anticipated from the competing objectives and from the research which is thought to contribute to their attainment. This procedure assumes that the effects of the development and application of the knowledge to be gained from research can be estimated. These new methods suffer, however, from the lack of a sound theoretical basis. They are therefore used mainly to provide a basis for comparing the results of qualitative arguments—which are based on the concrete judgements of experts—with those of quantitative calculations. Nevertheless, the development of a fully-fledged general theory might well be too difficult a task, at least for a long time to come.

---

[1] Weinberg, Alvin M., "Criteria for Scientific Choice", *Minerva*, I (Winter 1963), pp. 159–171.

[2] Werkgroep Evaluatiemethoden van de Commissie voor de Ontwikkeling van Beleidsanalyse, *Evaluatiemethoden, een introductie* ('s-Gravenhage: Staatsuitgeverij, 1982).

16-36 Minerva V.21 Spring.1983 2

The application of the criterion of social merit to the choice of programmes of research has become an established practice in the Netherlands. It works through sectoral councils for science policy which develop long-term plans for governmentally-supported, applied research which are based on considerations of social merit. These plans are offered to the minister concerned in an advisory form.

The Situation in the Netherlands: Sectoral Councils for Science Policy

In the Netherlands, the government has for many years been the main source of funds for scientific research. In 1982, about hfl. 3,500,000,000 were spent by the government for this purpose. This sum was allocated to the following fields, among others: energy: hfl. 152,000,000; agriculture: hfl. 257,000,000; building and urban environment: hfl. 44,000,000 and physical planning: hfl. 29,000,000. In 1974 the Minister for Science Policy issued a *Science Policy Memorandum*[8] in which the major objectives of Dutch science policy were promulgated. The intention was to improve the quality and efficiency of scientific research and to articulate it with the urgency of the social problems of Dutch society. National sectoral councils were envisaged as a forum for joint consultation between the government, the other "users" of the results of research and scientists. These consultations were to produce programmes of research, to run over a number of years, in areas or sectors which are adjudged to be of a high degree of urgency. For the agricultural sector, a national council for agricultural research—the Nationale Raad voor Landbouwkundig Onderzoek (NRLO)—had been in existence since 1957. When the sectoral councils were instituted in the second half of the 1970s, research relevant to fisheries was assigned to the agricultural sector, since agriculture and fisheries fell under the responsibility of a single ministry. In 1976 a provisional advisory council for research on the urban environment—the Voorlopige Raad van Advies voor het Onderzoek betreffende de Gebouwde Omgeving (VRA–OGO)—was set up. It is composed of government, scientists and

---

[8] *Science Policy Memorandum* (English summary). (The Hague: Government Printing Office, 1974).

"users", including the building and housing industry which has devoted little attention to research.

An advisory council for scientific research in development problems—the Voorlopige Raad van Advies voor het Wetenschapelijk Onderzoek in het kader van Ontwikkelingssamenwerking (RAWOO)—was the next sectoral council to be created in 1978. One feature of this council is that the intended "users" of the research, the economically underdeveloped countries and the people of these countries, are not represented in it. The RAWOO tries to compensate for this through direct contacts with scientists in those countries, through permanent connections with outstanding advisers in various countries, and *ad hoc* consultation with experts on subjects such as energy and health in undeveloped countries. In 1980, a sectoral council for research in energy was installed as successor of the national steering group for energy research, which had been created in 1973. Representatives of the main research institutions and producers in the field were active in a council for energy research—the Voorlopige Raad voor Energie Onderzoek (REO). Later on, the preponderantly "technocratic" composition of the council was modified by the introduction of proponents of "alternative" sources of energy and trade union and consumers' representatives. This had negative effects on the harmonious conduct of business within the council; it reduced the efficiency of its discussions and apparently also its persuasiveness with the governmental authorities responsible for energy policy. The council for national environmental research—the Voorlopige Raad voor het Milieu- en Natuuronderzoek (RMNO)—which was installed in 1981 was the successor of the national steering group. This group not only had the task of formulating programmes but also that of management in the field of environmental research. In contrast, the new body had only an advisory role. For all practical purposes, the programming board for research on land use—the Programmingscollege Ruimtelijk Onderzoek (PRO)—which was created in 1981 was treated as one of the sectoral councils, although its field of interest is not a sector of society but an element of a number of different sectors. The dependence of land use on independent developments in a variety of sectors of society, and the comprehensive scope of policies and research on land use, determine the character of deliberations in the PRO. In the near future, sectoral councils for educational research and for research on the care of health, to cover not only medical research but also research on organisations and institutions for the care of health, may be instituted.

Sectoral councils communicate with each other about common approaches to problems such as methodology, the assessment of "social needs", the estimation of the benefits of research, the setting of scales of priority and effectiveness in dealing with research institutes, governmental departments and the public at large. Sectoral councils also consult with each other on substantive topics of common concern. It is inevitable that the various fields assigned to the different councils should overlap. This is in general true for PRO which deals with research in land use, the RAWOO which does research on development which cuts across all parts of society, and the RMNO, the concern of which with environment touches on the interests of every sector. For the other sectoral councils, overlaps are more specific and less dispersed. Nevertheless, biogas as a source of energy, for example, is a subject which touches on agriculture as well as on energy. In such cases, sectoral councils can follow diverse courses of action; they can consult each other, they can form a common working group, or they can simply disregard each other.

Representatives of the department corresponding to the sector and of the Ministry of Education and Science—the Ministerie van Onderwijs en Wetenschappen—participate as advisory members of each sectoral council. In some, but certainly not all, councils, these representatives contribute to persuading the relevant ministry to accept and elaborate the proposal of the sector for particular research projects and whole research programmes. This effectiveness, however, is limited because sectoral councils are restricted to an advisory role and do not themselves decide the allocation of funds. If the sectoral councils were allowed to allocate some funds for research, the number of research workers might be increased and this might make the system as a whole more attractive for politicians and high civil servants. A proposal to improve the standing of the councils *vis-à-vis* other advisers, pressure groups and lobbies is now under consideration. The demands of interest groups count far more among politicians who think mainly in terms of the short run than do the recommendations of the sectoral councils for long-term programmes of research. Recently the council for energy research (REO) has fallen victim to such attitudes of politicians.

Until now all sectoral councils—except the council on environmental research (RMNO)—have published two or more long-term advisory plans. Apart from that, they have also given specific advice on organisational and other more limited subjects. The influence of this advice on the programmes of research institutions and on research workers, and the acceptance by the public of this advice about research, is difficult to assess. The long-term research plans of the sectoral council on agricultural research have been influential. Other sectoral councils have been less successful in obtaining the acceptance of their recommended research programmes, since the policies of governmental departments on the existing programmes and activities of research institutes have run in an opposite direction. The effectiveness of the sectoral councils in their recommendations for research, and in their realisation of the practical social ends which the research is supposed to serve, can be judged only after a long time. This renders them very vulnerable to criticism.

The jurisdiction of the sectoral councils is limited. Research which is of primarily scientific or intellectual interest, or which does not deal with practical matters, is not subject to the recommendations of the sectoral councils. These kinds of research are carried on mainly at universities.

Research proposals on these matters are reviewed by peers; those judged to be of good scientific quality are subsidised by the council which is responsible for the support of basic research—the Nederlandse Organisatie voor Zuiver-Wetenschapelijk Onderzoek (ZWO)—from the funds allocated by the government. Research projects which are commissioned by governmental departments to help them in the execution of their policies also fall outside the jurisdiction of the sectoral councils. Thus, the councils cover a middle area between fundamental research and research intended to serve immediately the execution of governmental policy.

The main task of the sectoral councils is to advise the minister concerned about preferred directions for scientific research intended to serve practical ends in their sector. In addition to the sectoral councils, other advisory committees on research have been formed for topics for which no sectoral council exists, such as social work and provision for aged persons. The *meerjarenplannen* (long-term plans), which are developed for this purpose, are mainly based on estimates of the quality of the available pool of scientists and on the likelihood of obtaining scientifically-sound results from the investigations undertaken. The sectoral councils are composed of representatives of scientists working in the field, of governmental departments and of users—both "productive" and "consumers"—of the desired scientific results. Thus, interest, competence and political representation are combined in the composition of the sectoral councils and the other advisory committees.

The importance of carefully devised programmes of research, and the establishment of grounds for choosing among them, is growing in view of the belief that social problems regarded as pressing will—bearing in mind the scarcity of funds available for research—be brought closer to solution by the use of knowledge gained through research. The Netherlands spends only about 1 per cent of the world's total expenditure on research; it is also sensitive to "social problems" inside the country and is very much committed to the rational solution of social problems. This makes a judicious science policy a high desideratum, and entails a search for clear and applicable criteria for choosing research projects and programmes for financial support.

The Problematic Nature of "Social Needs"

Well-informed persons are normally aware of which research institutes and groups of research workers are doing good work in their respective fields of science. Supplementary evidence, such as citation indices, are available to lend support to their expert judgements, to show which scientific fields are being intensively worked and which are being left fallow. Interviews with scientists by qualified persons can give some insight as to the fields which leading scientists think should be cultivated. The assessment of the "social merit" of a proposal for research is much more difficult. The delineation of the social needs which can be satisfied or alleviated through the application of knowledge gained from research is the difficult task of the sectoral councils. It is difficult to decide what the "need" is and then to decide on the research which in principle ought to disclose the most efficacious measures for dealing with that need. These problems are made even more difficult by the fact that the carrying out of a research project sometimes takes many years. As a result of this, it is necessary to try to foresee what will be "needs" in the future, when the results of the research will become available. Research set in train now can, in principle, and at best, reveal the procedures for dealing with "needs" which have not yet been discerned. The relationship between needs and problems may in this context be described along lines drawn by Professor Robert Merton, who defined social problems as "the substantial, unwanted discrepancies between what exists in a society and what a functionally significant collectivity within that society seriously (rather than in fantasy) wants to exist in it".[4] In this definition of social problems, there is a discrepancy between an existing, objective situation and a subjective "definition of the situation", which is the normative element of the unsatisifed desire. The practice in the Netherlands is to use the term "need" to refer to the desired situation, the situation which if realised would satisfy the "need". Sectoral councils are especially interested in needs for which research programmes could describe the existing situation, offer insightinto the procedures which would change the existing situation into a desired direction, and describe the subjective attitudes which make objectivesituations into "problems". The councils play an important role in defining situations as problematic; this is a condition for their choice of research programmes. They take on themselves to say what the "needs" are; they are the judges of whether a situation is "problematic".

Sectoral councils try to avoid the danger that they only reproduce the dominant views on "needs" and "problems". The national council for agricultural research (NRLO), the council for energy research (REO), and the advisory council for environmental research (RMNO) have deliberately included representatives of views different from their own. As a result, discussions within the councils have become more explicitly political. This makes exigent demands on the skill of the chairmen and secretariat of the councils to work out compromises between conflicting views. At the same time, "minority opinions" areavoided as much as possible by the sectoral councils since evidence of disagreement could weaken the persuasiveness of the recommendations for the particular research programmes and projects put forward. A rounded and balanced view on desirable programmes for research in a sector has to be achieved through arguments and discussions in which the diverse participants have to make concessions to opinions and proposals with which they disagree.

---

[4] Merton, Robert K., "Social Problems and Sociological Theory", in Merton, Robert K. and Nisbet, Robert (eds). *Contemporary Social Problems* (New York: Harcourt Brace Jovanovich, 1971), p. 306.

The "social needs of the future" are extremely difficult to foresee because such predictions require prediction of future objective situations and future desires. The situation is made even more difficult because the predictions about situations and desires themselves change. The dominant view on "future social needs" and the urgency of satisfying them may change almost overnight. This is clear when we look at predictions about the future supply and demand for energy, or predictions of the size of the population or of the housing situation in the Netherlands. The unreliability of so many forecasts should be a warning against the recommendation of "crash programmes" in which large amounts of resources are to be invested. In the field of energy, because of the very high price of crude oil, many alternative sources were canvassed; scientific research was needed for their assessment. The research programmes of the sectoral council concerned were much affected by this situation. Then, when OPEC failed to maintain the high price of crude oil, research in some of the programmes, such as that on the gasification of coal, had to be curtailed.

Demographic predictions for the Netherlands for the year 2000 have also had to be lowered within a few years from more than 21 million to 14.5 million inhabitants. This has affected, of course, many other predictions regarding objective situations and the probable need for housing. A first response to the lowered prediction was the reduction of plans for housing construction. Later on, unforeseen shortages occurred in the supply of housing, for example, a shortage of houses for single persons and couples without children. Among the changes causing this shortage were an unforeseen growth in the divorce rate and the early leaving of their parental home by young persons. However, because of the present economic recession, young persons have remained at home longer. Each of these unforeseen developments has in turn generated new research programmes which soon became irrelevant. Successive research programmes reflect these changes which were unforeseen in previous deliberations. Fortunately, many social needs are generally not as variable and so more stable research programmes can be promulgated; but this does not apply equally to individual projects. Some sectoral councils make use of the forecasts[8] of the council for scientific advice on government policy—the Wetenschappelijke Raad voor het Regeringsbeleid (WRR)—which purport to predict "structural changes" in Dutch society between the present and the year 2000.

Apart from the limited predictability of future situations and the "social needs" which are expected to accompany them, the characterisation of the needs themselves is problematic. There is often little consensus as to what is a desirable condition; some persons might think that there is a "need" which would be satisfied by certain social arrangements, while others envisage other "needs" which can be satisfied only by quite other social arrangements. Thus, the assimilation of ethnic minorities may be seen by a majority of the Dutch people as a "social need" for national or social harmony in a multiracial society, while this solution may be considered by the minorities themselves as a wide divergence from the norm; for the latter the divergence from the norm lies in the attitude of the majority. What are called "needs" are in fact the predicted desires of the group to which the "needs" are attributed. Social research may be useful in gaining insight into the diversity of opinions with respect to "social needs", i.e., desires and social problems as these are conceived by different groups in society. Research might help to discover any possible basis for the formation of consensus within Dutch society and within the sectoral council as well.

---

[8] Wetenschappelijke Raad voor het Regeringsbeleid, *Beleidsgerichte Toekomstverkenning: Dl. I, Een poging tot uitlokking* ('s-Gravenhage: Staatsuitgeverij, 1980).

No general guidance is given to the sectoral councils to help them out of these difficulties. By the selection of members who are prepared to try to find practicable compromises and who take an independent position *vis-à-vis* their "constituency", discussions in sectoral councils in practice usually conclude in generally accepted research programmes. In view of the diversity of the definitions of "needs" and of "problems", and the limited reliability of the predictions of future "social needs" to which social research might be relevant, it is not surprising that techniques of assessment of the "social merit" of research programmes have received much attention from the sectoral councils.

Thus far, the councils have followed three courses of action in determining the social merit of alternative programmes of research: (1) by focusing research programmes directly on "social needs" generally acknowledged at present; (2) by relating research programmes to social needs defined by policy-analysis and (3) by relating research programmes to social needs defined by deduction from general ideals.

Relations between "Social Needs" and Research Programmes

The research workers, the "users" of research, and the persons with power of decision who serve on the councils may be asked to indicate the specific research programmes which they think are called for if certain needs, taken as given, are to be satisfied—or, put differently, if certain social problems are to be solved. The needs to be satisfied or the problems to be solved can also be discovered by a sample survey of the population as a whole or by asking certain categories of persons what they deem to be the main needs or problems. It is also possible to obtain a list of needs and problems and schedule of their urgency by seeking the opinion of experts. (The experts may be encouraged to speculate freely through "brainstorming sessions" or "Delphi-rounds" or the "snowball-approach".) Lists of needs and problems can be obtained by hearings, conferences, seminars, and other types of discussions with the "producers" or "consumers" who will use other forms of direct contact with productive or consuming "users" of the results of research. It is also possible to solicit "position papers" and "trend reports", using the procedures which have become fashionable in these circles recently, such as available forecasting techniques: "extrapolations", "scenario-writing" and "morphological boxes". Finally, the representatives of the various interests on the councils may seek the views of those they represent about "needs" and "problems". These techniques of formulating lists do not necessarily result in consensus, and do not in themselves offer ready-made decisions as to what research should be supported.

The most fruitful way of proceeding from a list of needs and problems to the choice of research topics which will be relevant to them, is also the most difficult way. The simplest way to proceed would be to assign different degrees of urgency or weights to the components on the list, and then to delineate the research programmes required to discover the facts and propensities which must be known in order to deal with them. Research programmes relevant to the most urgent needs would be given precedence over those relevant to less urgent needs.

Of course, there are numerous pitfalls in this procedure. For political reasons, some members of a council might argue that one particular research programme is more appropriate than another. It is therefore necessary to show explicitly how the results likely to be achieved by a particular programme will contribute to making effective policies which are intended to satisfy needs or to solve problems.

The Use of Policy-Analysis to Connect Social Needs with Research Programmes

Governmental declarations of policy: When declarations of long-term policy are produced by a department or by the government, sectoral councils may use them as points of departure for the enumeration of needs and problems. The definition of present and future social needs and problems, by the government, may be in partial correspondence with the definition of needs and problems by the citizenry, which itself is usually not unanimous. Not all the needs and problems which are thought to be urgent in one or another part of the citizenry are regarded as urgent by government. An advisory council which restricted itself to such declarations would be casting its net too narrowly; it would not do what it ought to do as an independent advisory body. Nevertheless, such declarations of policy are helpful and their systematic study is very desirable in delineating "social needs" and ranking them in order of urgency.

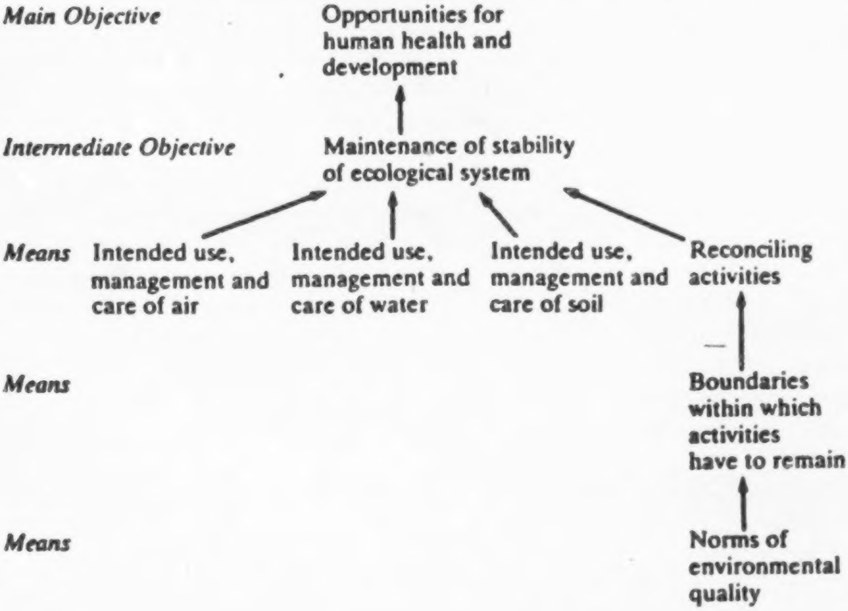
A set of analytical procedures has been developed for the systematic analysis of policy-documents, and applied to the definition of needs in Dutch environmental policy.[8] The policy-document in question is dissected into "policy-elements"—ends and means—which are arranged into a "goal-tree" or "policy-tree". A "policy-tree" is one construction of the formulation presented in a policy-document in terms of ends, means, repercussions and logical implications (Figure).

---

[8] Kuijpers, G., Graaf, H. v.d., and Polhuis, P. L., "Beschrijving van het Milieubeleid", *LASOM*, reeks no. 5 en 6, Bijlage 11 ('s-Gravenhage: Staatsuitgeverij, 1980).

FIGURE

Derivation of Research Problems from the "Policy -Tree"


Main Objective
Opportunities for human health and development
Intermediate Objective
Maintenance of stability of ecological system
Means
Intended use, management and care of air
Intended use, management and care of water
Intended use, management and care of soil
Reconciling activities
Means
Boundaries within which activities have to remain
Means
Norms of environmental quality

Questions of Policy	*Topics for Research*
1. What kinds of intended use, management and care of air, water and soil are necessary for the maintenance of the stability of ecological systems, and through this for opportunities for human health and development?	1. Which procedures for the management and care of air, water and soil are effective for the maintenance of the stability of ecological systems? What does each procedure contribute? By whom can these procedures best be carried out?
2. In which sectors are measures for intended use, management and care most effective with respect to the maintenance of the stability of ecological systems?	2a. Which present activities will endanger the stability of ecological systems, unless present policies are changed? b. What are the adverse consequences of each of these activities on the stability of each part of the ecological system? c. Which activities can be changed so as to diminish or eliminate their danger to the stability of ecological systems?

d. How much time is required for these changes?
e. What are the foreseeable consequences of discontinuing activities which cannot be reconciled with maintenance of ecological systems?

SOURCE: Kuijpers, G., Graaf, H. v.d., and Polhuis, P. L., "Beschrijving van het Milieubeleid", *LASOM*, reeks no. 5 en 6, Bijlage II ('s-Gravenhage: Staatsuitgeverij, 1980).

This method of analysing the declarations of policy issued by the government makes explicit what is only implicit in the document itself. The reconstruction of a declaration of policy into a policy-tree makes it easier to see what research is required to supply the information necessary for the attainment of the goals, and to determine the most effective means for attaining those goals. The research programmes or projects which are drawn from the policy-tree are not intended to define "needs", which are assumed to be known through other procedures. The research is intended to throw light on the cost of achieving the ends of the policy, to tell what conditions must be met if the end is to be achieved, and what are the most effective and economical procedures.

All this assumes of course that the scientific disciplines which are called into action by the research programme are capable of answering these practical questions, that they have the necessary knowledge already, or that they can obtain it through reliable techniques and provide it in time for it to be used in the execution of policy. Research programmes dealing with the "higher order" of the particular policy-tree, being more relevant to the attainment of the central objectives of the policy, are usually given precedence over those which are relevant only to minor features of the policy.

This analytic method has limited value because it draws exclusively on written documents. It has also proved to be very time-consuming because of the huge amount of analytical work to be done. The analysis of a single document with the aim of constructing a policy-tree, may contain hundreds of "means–ends relationships" about each of the questions which might be answered by research. A long-term plan for reducing atmospheric pollution entailed the formulation of 164 such relationships. Another drawback is that this method gives precedence to present needs and problems at the cost of future ones; this deficiency arises from its self-restriction to the study of governmental declarations of policy, which are usually preoccupied with the near future rather than the remoter future.

The value of the method of the policy-tree for the choice of research programmes and projects depends on the degree of specificity with which it defines its subsidiary or derivative goals, and the means to be used to attain them. If the definitions are precise, then it is easier to determine which research projects will throw light on the efficacy of the recommended means. But this feature of this particular method of making decisions about which research programmes and projects are to be recommended for sponsorship is not unique to the policy-tree.

Deduction of needs and research programmes from general ideals: If "social needs" and "problems" are defined as discrepancies between the existing situation and a more or less clear notion about a desirable or ideal situation, "needs" and "problems" can be defined by deduction from the desirable or ideal situation. The first step in such a deduction is to state in a systematic and concrete way the abstract ideal, for example, "living well", for each of the spheres of life such as housing, work, safety and education in which it has to be realised. The conditions or means which are thought to be important for the attainment of the situation desired in each different sector can then be defined. This analysis goes on until the level of particular concrete actions in policies is reached.[7] The "planning, programming, budgeting system" (PPBS) adapted and applied by governmental departments in the Netherlands, and further developed for this special use by a committee on policy analysis—the Commissie voor de Ontwikkeling van Beleidsanalyse (COBA)—is an attempt to build such a deductive framework.

Starting from the specification of any particular ideals, a complete "policy-tree" of the chain of subsidiary ends and the means presumed to lead to them might be derived. Theoretically, it would be possible for each sectoral council to develop on this basis a long-term plan for scientific research in its own field. The sectoral council responsible for aid to developing countries works more or less on these lines. A deficiency of the work of this council is its lack of representatives of the developing countries. It must therefore begin with its own declaration of the ideal to define the most urgent "needs" and "social problems" of the countries on which research is required. In accordance with Dutch policy for dealing with developing countries, the situation of the most deprived groups in the countries concerned is chosen by the council as the focus of its own long-term programme of research. The method of defining needs and problems in the light of an ideal suffers from limitations similar to those of the other method. It is perhaps more burdensome than the use of governmental declarations of policy because a complete policy for the sector in question has to be constructed; then the particular programmes and projects for research have to be derived from that policy.

A third means of determining "social needs" is to assemble experts and allow them, through the various techniques available to them, to define the "needs" which are likely to require satisfaction in the near and remoter future. The experts may rely on their own ethical and political ideals, as refined by intimate knowledge and experience of the fields concerned, they may use "Delphi" methods or draw upon public opinion polls to determine which "needs" it is most urgent to satisfy. Once they arrive at an agreed schedule of needs, it still remains to decide which research programmes or projects will best provide the knowledge required for execution of the policy arrived at. This task is common to all three methods of establishing a schedule of the urgency of needs. The carrying out of this task requires familiarity with the various fields of science, the existing state of knowledge in those fields, and availability and willingness of scientists in those fields to perform the desired research.

---

[7] Kuijpers, G. "Beleidsontwikkeling en de vaagheid van idealen", in *Beleidsanalyse '74-6* ('s-Gravenhage: Staatsuitgeverij, 1976).

The Availability of Qualified Research Workers and the Specification of Topics for Research

Even for well-informed persons such as members of the sectoral councils, it is difficult to develop a programme, since the successful execution of the research programmes they recommend depends on the availability of research workers willing to undertake the proposed research. Intimate contact with academic scientists and scholars and with those in public and private research institutes is indispensable for the sectoral councils. They must be abreast of the quality of the research already being done in the fields for which they are responsible, and they must have adequate ways of communicating with appropriate sections of the Dutch scientific community. Some of the sectoral councils have gradually established such contact with the scientific community, so that they are informed about the opportunities for the desired types of research and the available funds.

The national council for agricultural research—Nationale Raad voor Landbouwkundig Onderzoek (NRLO)—which is already more than 25 years old, has developed for this purpose a body of about 1,000 "participants", organised in chambers, programme advisory committees, co-ordination committees and committees for the study and analysis of policy. The council for national environmental research—(RMNO)—has taken over the arrangements which existed under its predecessor—a national steering group—which not only specified research programmes but managed them. The programming and study groups, created by the steering group, have been valuable instruments for the RMNO in finding the research workers required by their research programmes.

In the first years of their existence, the other sectoral councils gave special attention to the development of research programmes and projects on the basis of their projected future "needs" in the subjects for which they were responsible. Their advisory function for governmental departments, which were the main financial patrons of research, took precedence over the execution of research programmes. More recently, sectoral councils have spent additional time and effort on the execution of the research programmes proposed in their long-term plans. They organise discussions with administrators and research workers in universities and research institutes;

they invite politicians and civil servants to discuss proposals contained in their long-term plans and to indicate which parts of these they would be willing to support with funds.

Apart from the NRLO, which attempts to formulate research projects in specific terms, the other councils confine themselves to the formulation of general research programmes. For the most part, sectoral councils are advisory bodies; they have no funds of their own for the support of research. The effectiveness of their work depends completely on the persuasiveness of the proposals which they make to ministries, and on the attractiveness of their programmes to members of the scientific community.

Very recently, however, the Ministry of Education and Science—the Ministerie van Onderwijs en Wetenschappen—has begun to make funds available to enable sectoral councils to draw together and support research groups, who will be prepared to work out projects in accordance with sectoral councils' own long-term plans.

The Formulation of Strategy for Scientific Research

The Dutch sectoral councils have to work within the framework of a comprehensive national policy for scientific research. The national policy deals with the directions in which national efforts in research should be developed and what means will be required for them. Individual scientists, whole research institutes and each sector of society confront similar questions about the topics that interest them. Councils are expected to answer these questions for their own sectors through schedules of priority, indicating the fields of research into which more resources should be put and others in which efforts could be reduced. To the extent that the advisory long-term plans of sectoral councils are accepted by governmental departments and other agencies, they exercise considerable influence on Dutch science policy in their particular fields. However, they compete with each other and with politicians and civil servants who are usually more influential than any particular sectoral council. One council might have more influence on national science policy than its counterparts.

Practical and fundamental criticisms of these approaches to the formulation of science policy present themselves. From a practical point of view, it is difficult, even if possible, to define and enumerate social needs and problems and then to derive research programmes from them. None of these approaches provides a reliable basis for deciding on the relative urgency of the alternative research programmes of the competing sectors. One factor which affects the efficacy of the sectoral councils is that the relevance of research differs from one sector to another. Present methods of deriving research programmes from "social needs" and "social problems" imply that the knowledge produced by scientific research always has the same relevance for meeting needs and dealing with problems. In some parts of social life, scientific knowledge can reveal aspects otherwise completely unknown; in others, it adds little to what is needed for dealing with a practical problem. It has been assumed that all scientific research is equally relevant and that only the "need" or "problem" which it is to serve varies in urgency. Because of this basic assumption, the social merit of research programmes has been treated as a unidimensional variable. Although different individuals and groups may disagree in their views of the relative urgency of social needs and problems, and in their choice of research projects and programmes for their satisfaction or solution, research in general has been thought always to have the same relevance. According to this view, particular projects may vary in relevance but not research as such; every piece of knowledge which fills a gap in our existing stock of knowledge could be compared with every other piece of knowledge as far as its social value is concerned—assuring that we know the relative urgency of the needs to which it is related, and the effectiveness of its application to alleviate or reduce them.

Linking Knowledge to "Social Needs" and "Problems"

All three procedures for determining the urgency of needs and problems are open to criticism because they fail to take into account that scientific knowledge has, among others, at least three fundamentally different functions in relation to "social needs" and "problems". This fundamental difference makes research programmes related to one of these functions not comparable to research programmes which perform the other functions. Any research programme may be assessed with respect to each function and any one of them might be rated as "high" in its performance of one function and as "low" in its performance of the others.

The most general function of scientific research is to increase, broaden, deepen or improve our understanding of natural and social phenomena. If such understanding can be furthered in some way in some part of society, that itself is a social merit. This is as true of fundamental research as it is of mission-oriented research. This function of research programmes might be called the illuminative function. With respect to practical action, however, scientific developments may disclose new opportunities of action and new objects of demand. As a result, the level of aspiration or demand can rise. The biomedical sciences are a case in point; advance in these sciences has led to rising demands and indirectly to new "social needs" and "problems" which were previously less salient. In this way existing and acknowledged social problems—being discrepancies between actual situations and desired situations—may be solved, but new ones might at the same time be created through these scientific developments. Scientific knowledge not only provides opportunities: it also increases the general sensitivity to features of social and economic life hitherto regarded as undesirable, but which were unnoticed before or were accepted as unavoidable. Modern ecological, chemical and biological studies, for example, have uncovered many features of the natural environment and of the impact of human actions on it.

Many developments in the natural and social sciences throw light on the effectiveness or efficiency of existing technological and social arrangements. These arrangements touch on not only the material conditions of existence but also on ideal values such as security, justice and freedom. In every sector of society, specific standards are used to measure the value of the material and ideal goods concerned. The more the provision of ideal goods depends on material arrangements, such as arrangements for safety, and on administrative agencies, the more easily cost-benefit analysis and balancing between ideal goods and material goods can be applied. Most of the long-term plans of sectoral councils for science policy consist of programmes which are intended to perform this function of knowledge. Most programmes for research on energy, health, building, environment, minorities and the aged are directed at the effective and efficient provision of benefits in those fields or to those categories of individuals. This function of research programmes may be called the technological function.

Some research programmes are directed to "social problems" which are constituted by actions and institutions; these are legitimately performed and maintained, but they affect different groups in society in very different ways, some injuriously, some advantageously. The policies which the research programmes are intended to serve are more or less palliative; they alleviate social problems without necessarily "solving" them definitively. Among the examples of social problems created by social arrangements is the damage, material and immaterial, arising as a consequence of the routeing of traffic within cities, or of the siting of nuclear power plant and of air- and water-polluting factories. The uneven distribution of costs and benefits of policies among different groups or sectors of the population is correlated with the distribution of power in society. Research may be designed to throw light on such arrangements.

Such is the research that was recently designed by the advisory council on scientific research in developmental problems (RAWOO). This council has developed a programme of research on the "international dimensions of development problems". The research is intended to analyse the positive and negative effects of the international distribution of economic, legal and political power on the developing countries. Its rationale is derived "from the widely accepted contention that many, if not all, of the problems which developing countries encounter on their path of development cannot be fully understood without reference to the international economic and political field of forces within which they have found themselves".[8] Research programmes like this might increase the ability of developing countries to deal with these international forces by indicating ways in which they could withstand or circumvent them. Through urban research, to mention another example, persons who are adversely affected by some urban developments may gain insight in the economic and political mechanisms which determine the physical environment in which they live, and in this way increase their capacity to protect themselves against these adverse effects. Much "oppositional research" may be put in this category. Decisions to support this kind of research programme are not taken for purposes of illumination of opinion, or for technological purposes, but in order to affect the distribution of advantages and disadvantages within society or among societies. A lobby supporting the use of nuclear energy might perhaps not be in favour of giving the highest preference to research that tries to determine the dangers of nuclear waste disposal, although many persons—rightly or wrongly—are much concerned about them. Whether this kind of research will be executed, in which way and by whom, does not depend on cost-benefit considerations regarding alternatives for waste disposal, but on the existing distribution of political power among interest groups. The arguments for such programmes are first and foremost political. This function of research programmes might be called the political function.

---

[8] Advisory Council for Scientific Research in Development Problems, "The International Dimensions of the Development Problem", in *An Inventory of Current Research in the Netherlands* (The Hague: Government Printing Office, 1982).

Thus, insights gained by scientific research can function in society to improve our awareness of the possibility, desirability or acceptability of arrangements in some sectors of society; they can improve the effectiveness of efficiency of these arrangements, or they can make us conscious of the ways in which problems created by our policies in particular sectors of society have arisen and could be relieved. In its first-mentioned function, research contributes to the quality of political and social discussion in society, and through this to the quality of policies as such related to a sector and the problems therein. In the second function, research contributes to the effectiveness and efficiency with which arrangements in a sector are created and maintained. In the third function, research would contribute to the alleviation of social problems caused by the adverse consequences of policies or arrangements which have been democratically accepted. Obviously, these functions of research for different social purposes differ quite substantially. In the first function, the cultural or intellectual end is paramount; in the second, the economic or technological end; and in the third function, the political end is paramount. Because of these qualitative differences in their functions, the research programmes related to them are not comparable. It is not reasonable to attempt to apply a single set of criteria of choice among them.

Criteria of cost and benefit are hardly applicable to research programmes that aim at the improvement of social and political discussion. For research aimed at the effectiveness and efficiency of arrangements, these criteria are of primary importance. But for scientific research which is oriented towards social problems in which the distribution and redistribution of power are involved, criteria of cost and benefit are not sufficient. In the choice of any research programme its function must be a first consideration, prior to the decision regarding the particular subject-matters of research. Only when the function has been decided can decisions begin to be made regarding the scale of financial support to be allocated to it. The kinds of questions to be answered if the research is to contribute to the quality of social, intellectual and political discussion, differ from those which must be asked if the research is to contribute to the effectiveness and efficiency of such arrangements or to improvements in the distribution of power. The criterion of social merit turns out to be heterogeneous in its constitution. There are in fact three separate and distinct criteria of social merit.

The Quality of Social, Intellectual and Political Discussion

In a pluralistic society, beliefs may differ as to the right and most desirable properties of the social arrangements which should be created and maintained. Some persons prefer the benefits of largeness of scale, some think that "small is beautiful", some prefer modern medical care, others might prefer "alternative medicine". Scientific research may play an important role in a discussion about these desired conditions, as it illuminates the opportunities, threats, advantages and disadvantages attaching to specific arrangements or policies. Scientific research which deals with such questions improves the quality of social, intellectual and political discussion. Although the results of the research concerned may not be unambiguous, they can be a means to improve the public discussion. Publicists might write more easily about the issues, the ordinary man might become better informed, parliamentary committees might organise hearings, which are then published, pressure- or interest-groups might adapt their arguments, and politicians might become more aware of what is at issue. Thus, the quality of the debate may improve even if the research itself does not produce scientifically conclusive answers.

Questions to be dealt with by scientific research for the performance of this illuminative function include: the quality of arrangements which are needed and feasible in the sector concerned; the light that can be thrown by scientific research on ideas like an "ecologically balanced environment", "good living space", "acceptable development aid" or "good health"; the quantity of provisions that are being sought; how "social needs" can be more accurately defined and whether they are socially acknowledged; and when provisions are sufficient. The kinds of research which are needed to perform this illuminative function are very heterogeneous.

Research which attempts to answer these kinds of questions will have illuminative effects. Because attitudes and beliefs and technological possibilities change continuously, discussion of these matters never stops. Scientific research directed to this end will therefore never cease. The Dutch council for scientific advice on government policy—Wetenschappelijke Raad voor het Regeringsbeleid (WRR)—recently completed a study on possible future beliefs and attitudes and their significance for technological, administrative and social alternatives in the various sectors of society.[9] The beliefs are ideal types of "sociocratic" or "technocratic" attitudes, combined with "Christian", "socialist" or "liberal" political dispositions. For each of the six resulting ideal typical attitudes, the study tried to determine the technological, administrative and social alternatives which would be best suited to it. Whatever the scientific or intellectual shortcomings of this investigation, it certainly contributes to the self-understanding of Dutch society; this is one aspect of the illumination of opinion.

The Effectiveness and Efficiency of Arrangements

In many sectors of Dutch society, arrangements for the provision of social and other services are created by governmental and private initiative in co-operation. Research directed at improvement of the effectiveness or efficiency of some of these arrangements is pertinent to both the private and the public sector. In the field of energy, for example, effectiveness and efficiency are standards applied to production, storage, distribution and use of energy. In the field of health, effectiveness and efficiency are standards applied to the provision of hospital care; the care of ambulatory patients; the provision and number of surgical operations; provisions of preventive medical services such as hygiene and inoculation, and the co-ordination of, for example, intramural and extramural medical care. In urban affairs, effectiveness and efficiency are standards legitimately used to assess the production of houses and offices and the distribution of living accommodation. In aid to developing countries, most standards of effectiveness and efficiency which are useful in assessing conditions in advanced countries can be applied there as well—but not always, as in situations in which Western medical practices drive out sound traditional practices. Research aimed at improving effectiveness and efficiency has an economic rationale. The direction and scale of this research are determined by economic considerations; research is intended to improve the results of given allocations of scarce resources in the enhancement of well-being.

The Origin and Solution of Social Problems

A certain level of provision or a certain kind of social policy may be socially desirable and still generate serious problems. Some groups may experience more disadvantages than others from a particular arrangement such as the location of an airport, a road, or a nuclear energy plant. Research can be useful to find out the exact disadvantages, as well as the advantages, and how they could be compensated or avoided. With this kind of research, we are at the heart of conflicts over the exercise of power in the pursuit of parochial interest. Such conflicts are about the distribution of disadvantages and their infliction on persons who suffer from them without corresponding benefits. Research on such problems will generally give rise to controversy. In cases like this, the research itself has to be considered as a kind of compensation to the disadvantaged, which may limit the power and rewards of the group that causes the damage or benefits from it. Because of the nature of this kind of research, other criteria of social merit have to be applied to decide what proportion of the resources available for research should be spent on these topics rather than others.

---

[9] Wetenschappelijke Raad voor het Regeringsbeleid, *op. cit.*

Criteria of Social Merit in Research

We have seen that research questions on completely different kinds of social needs and problems should not be placed in one line and judged by a single set of criteria of social merit. Let us now consider which criteria of social merit are most appropriate in determining the amounts of money which have to be spent on the three kinds of research.

As to the first kind of research, its purpose is to improve the quality of public knowledge and discussion. It has an intellectually illuminating function. Cost-benefit considerations can hardly be applied to research of this sort without making utterly arbitrary assumptions. The only reasonable way to determine the amount of money to be spent is to allocate a lump sum each year as a proportion of the total expenditure on research. This amount can be related as an overhead to the total expenditure on research with a technological function.

Research to improve effectiveness and efficiency in a sector has to pay for itself. In the economic sector, it therefore can best be approached by cost-benefit analysis and in the welfare sector by an appropriate cost-effectiveness analysis. In a sector like the care of health, appropriate measures of effectiveness still have to be developed. Not only within sectors of society, but in principle also between sectors, these kinds of cost-effectiveness analysis can be applied as a way of deciding which projects or programmes of research should be supported.

For the third group of research programmes, criteria of social merit to determine the desired amount of support to be allocated are difficult to find and to apply in practice. Groups which feel imperilled by some technological risk might propose the research to be done on the subject, while those who in some way might profit from the technological innovation might be reluctant to have public funds spent on it. Measures in consonance with democratic principles would relate the desirable amount of support for research with oppositional political functions to a set of criteria: for example, the disparity between the advantages for one group caused by a policy and the disadvantages for another; the risk of social unrest that might be caused by the arrangement, or the damage that might result from the social unrest; the cost of compensatory measures for damage caused by negative external effects; and the size of the population affected.

The larger the population that might be adversely affected by an arrangement such as a nuclear power plant, the more it would cost to compensate them for the loss of houses near the plant; for instance, the greater the risk that disorders will occur and cause damage, and the more profitable is the use of nuclear energy for society at large, the more reason there is to meet the demand for research on possible negative effects of nuclear production or waste disposal, put forward by the groups which might be negatively affected.

Conclusion

The first steps taken by the various sectoral councils to apply the criterion of social merit in the choice of research projects and programmes have inevitably been very halting and lacking in precision. This was only to be expected. The effort has, however, been worthwhile, because it has brought very important issues to the fore. It has faced many genuine difficulties but has also raised many hitherto unsuspected issues. In this respect it represents a genuine advance in the discussion of science policy.

Of course there are very important issues with which this effort has not dealt. For one thing nothing has been said thus far about the proportion of the total funds provided by the government of the Netherlands which should be devoted to basic research and to applied or technological research. It says nothing about the allocations required for the training of scientists. It takes for granted that scientists will be available but says nothing about their education and training.

It likewise says nothing about the problems of the distribution of the best scientific talents between research decided on the basis of social merit and research projects chosen on other grounds. There are many other fundamental problems of science policy with which the discussions regarding the work of the sectoral councils have not dealt.

All this notwithstanding, the work of the Dutch sectoral councils is a genuine contribution to the international analysis of the criteria of scientific choice.

○